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FURTHER EVIDENCE OF ONE-TRIAL ASSOCIATIVE LEARNING

By IRVIN ROCK and WALTER HEIMER, New School for Social Research

In a previous paper it was shown that in traditional paired-associates learning associations are not formed by a process of gradual strengthening.[1] Rather associations are formed on a single trial, and the previous experience with a pair, prior to the trial on which it is finally learned, does not seem to be of any value. When unlearned pairs were removed from a list after every trial and replaced by new ones (experimental group) the number of trials to criterion was not more than when the traditional procedure was used (control group).

The purpose of the present paper is to explore further the role of repetition in rote-learning. There are certain objections which can be made to the technique referred to above. One is that if any *wrong* associations should be formed, *S*s in the control group have to overcome them; *S*s in the experimental group do not, since all pairs which they fail to get right on any trial are eliminated.[2] A second is that the technique is not sufficiently sensitive to reveal a possible advantage for pairs experienced previously. A third is that the *S* who gets new pairs substituted for wrong ones has a possible advantage because many of these may be, for him, easier than the old, unlearned ones are for the *S* who learns by the traditional procedure; if so, the disadvantage of difficulty may offset the advantage of repetition for the control group. These three objections will be taken up in this paper.

EXPERIMENT I

The first experiment is an attempt to deal with the objection concerning the formation of wrong associations, but it also bears on the objection con-

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* Received for publication August 27, 1957.

[1] Irvin Rock, The role of repetition in associative learning, this JOURNAL, 70, 1957, 186-193.

[2] The authors are indebted to Professor Hans Wallach for pointing out this possible objection.

cerning the sensitivity of the test. The procedure involves interchanging the items of pairs which have not yet been learned in such a way as to form new pairs. Thus, suppose that Pairs A-B, C-D, E-F, and G-H are presented on the first trial and that Pair A-B is learned. On the second trial *S* is presented with A-B (again) and, in addition, with pairs such as C-F, E-H and G-D. The interchanging of items after each trial continues until *S* reaches a trial when he gets right all pairs presented to him.

This procedure is similar to the one given the experimental group in the paper previously reported.[3] In both cases, learned pairs remain in the list and new pairs are substituted for unlearned ones. In both cases, *S* has to acquire all pairs without benefit of repetition. In the present experiment, however, the new pairs consist of recombinations of previously experienced items. If, therefore, *S* has formed any wrong associations to the left hand item of any pair, he is here not relieved of this possible disadvantage by the substitution of a brand new pair, as in the previous experiment. The item now reappears and should carry with it the wrong association. Furthermore, if the incremental theory is at all correct, *S* should have formed partial, sub-threshold linkages between the two members of each pair. The reappearance of the left hand item with a different right hand item should necessitate overcoming that partial association if the new one is to be formed. Hence the present experiment also can be considered to be a more sensitive test of the one-trial learning-hypothesis. This new experimental group would have to *overcome* partially formed associations in comparison with a control group which follows the traditional procedure and which presumably has the *advantage* of such partial associations. The procedure is thus analogous to the cue-reversal design which has been used to test the continuity theory of discriminative learning in rats.[4]

Method. From the pool of 80 pairs of nonsense-syllables used previously, 8 were randomly selected for each *S*.[5] To facilitate interchanging items, the syllables were printed on small rectangular envelopes which fitted over the upper left and right corners of a 3 × 5-in. card. They could then be easily taken off and exchanged at the end of a trial. The plan for recombining unlearned pairs was as follows: When the pairs to be used for a given *S* had been selected, the two items of each pair were (arbitrarily) assigned a number from 1 to 8, which was written on the opposite side of the small envelope. Thus, the two items of one pair were labelled 1 and 1, the two of another pair 2 and 2, and so forth for all 8 pairs. After the first trial, the envelopes of the unlearned pairs were laid out in order, one under the other, forming two columns corresponding to the left and right hand items of each pair. The interchanging was then accomplished by shifting all items in the second column down one place, the last one moving into first place. These new combinations were fastened to the 3 × 5-in. cards and they were then mixed randomly with learned pairs, if any, for the next trial. This procedure continued until *S* reached a trial on which he got all pairs correct. (If on a given trial *S* made an error on a pair which he previously had got right, its two envelopes also were included in the columnar arrangement according to the number of the left hand item and therefore participated in the interchange.) The only exception to the above procedure occurred toward the end, when one *S* got 7 pairs corect on one trial. Since it was then impossible to interchange anything, repetition was not eliminated in the learning of that last pair. It should be noted, however, that 9 of the 15 *S*s went from 6 or fewer pairs correct on a given trial to all 8 correct on the following trial, which meant that in most cases there was no repetition at all.

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[3] Rock, *op. cit.*, 186-193.

[4] T. C. McCulloch and J. C. Pratt, A study of the pre-solution period in weight discrimination by white rats, *J. comp. Psychol.*, 18, 1934, 271-290.

[5] Rock, *op. cit.*, 189.

Since the number of possible combinations is limited, after several trials certain items may end by being paired with each other for the second time, particularly because learned pairs are no longer available for interchange. Thereafter, as the number of remaining unlearned pairs goes down, there may occur a third or even a fourth such repetition of previously combined items. One may be inclined to regard such instances as contradictory to the stated intention of determining the difficulty of learning interchanged pairings in contrast to that of learning pairings which preserve the original combination as in the traditional method. These exceptions may therefore be thought to reduce the difference in treatment which the two groups receive, but one can argue the matter the other way around. If *S fails* to get a pair right on the trial when the two items appear together for the second or third time, then from the standpoint of an incremental theory it ought to be all the more difficult on the subsequent trial to form a *new* association to the left-hand item of such a pair. That left-hand item should now have an even stronger sub-threshold bond for a particular right-hand item which must be overcome if the new association is to be formed. In any event, among all the pairs learned by *S*, such repetitions were the exception. For the most part, the procedure is correctly described as one involving the re-combination of previously experienced pairs into new pairs.

In all other respects, the procedure for the group followed that of the control group of Experiment II described previously.[6] Each card was presented for 3 sec. with a 5-sec. interval between successive cards, as determined by a metronome. *S* was instructed to associate the two members of each pair with one another with a view to being able later to respond with the right hand member when the left hand member was presented. He was told not to be concerned with the serial order of cards, since it would be changed from trial to trial. Testing after each trial was accomplished by covering the right hand item and presenting *S* with the left one at the rate of one every 5 sec. There was a 30-sec. interval between each test and the subsequent learning-trial. The order of presentation again was random. A response was scored correct if *S* pronounced the syllable correctly, and partially correct responses were scored as wrong. *S* was told whether his response was correct or not.

The results of this experimental group were to be compared with those of the control group of Experiment II described in the previous paper. *S*s were selected, therefore, from the same type of population and at the same times of the day. Performance on the first trial was considered to be a rough index of learning ability. Since the average number learned on the first trial did not differ significantly from the average for the control group, it was not necessary to depart from random sampling in any way. As in the case of the control group, 15 adults, mostly college students, were employed.

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[6] *Ibid.*, 189.

Results. As previously reported, the mean number of trials to criterion for the control group was 8.1 ($SD = 3.0$). If three *S*s are included who failed to reach the criterion of one errorless trial, scoring them as 15+, the median was 8.5 (semi-interquartile range = 3.2). The mean number of errors to criterion was 26.7 ($SD = 11.4$). For the present experimental group, the mean number of trials to criterion was 7.3 ($SD = 2.8$). One *S* failed to reach criterion. If he is included, scoring him as 15+, the median was 7.0 (semi-interquartile range = 2.25). The mean number of errors to criterion was 25.5 ($SD = 12.2$). None of these values differs significantly from the corresponding value for the control group. A plot of the mean number of correct responses per trial yields highly similar curves for the two groups.[7]

This result, therefore, supports the previous findings and in addition deals with the objection concerning the emergence of wrong associations. Since this procedure is one which ought to put *S* at an even greater disadvantage than that employed with the previous experimental group from the standpoint of an incremental theory, the outcome can also be viewed as an answer to the objection concerning the insensitivity of the previous experiment as a test of the one-trial learning hypothesis. A very impressive qualitative finding was that hardly any *S*s indicated awareness of the fact that pairs had been interchanged when they were questioned about it at the end of the experiment.

As in the case of the previously reported Experiment II, there are certain additional reasons why one would expect the performance of the control group to be superior. The use of recall rather than a matching test of recognition would lead one to expect that many failures to respond would have been successes had a matching test been given. This means that for the experimental group some pairs which *S* had actually learned were broken up and interchanged to form new pairs. As a result *S* would have to overcome such prior associations in order successfully to form the new ones required by the interchanging. *S*s in the control group, on the other hand, have the advantage of retaining these associations for solidification on subsequent trials. Related to this is the fact that the stringent rule was followed that a pair which *S* got right on previous trials was nevertheless broken up and interchanged with other pairs if ever he failed to get it right on a subsequent trial. In the present experiment, however, familiarity with the items should *not* be considered as a factor which would give an advantage to the control group. Although the interchanged pairings are new for the experimental group, the specific items are not, any more than they are for the control group.[8]

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[7] The curves are virtually identical for the first several trials where there are practically no instances of repetition of previously combined items in the experimental group.

Again, however, as in the previous experiments, it can be argued that the experimental group has an advantage as far as difficulty of pairs is concerned. It is possible that the interchanging results in a few pairs on each trial which are particularly easy for a given *S* to learn, perhaps for purely idiosyncratic reasons. For the control group, some pairs may be easy, but these presumably are learned at the beginning, and the rest may remain relatively difficult. In some cases the pairs resulting from interchanging for the experimental group were unquestionably easier to learn. These involved combinations which *E* would ordinarily not allow in making up pairs for an experiment on associative learning, as for example FAX–VAQ or FAH–WEH. Such pairs were, however, exceptional. Even if this factor of differential ease of learning did play some role, it is very probably counterbalanced by the factors mentioned above. In any case, one would imagine that if the incremental theory were correct all factors such as those being discussed now would pale into insignificance in comparison with the detrimental effect which the interchanging of pairs should have. In fact, according to the widely held interference theories of forgetting as due to competition among different responses to the same stimulus, there should be very poor performance indeed in the experimental group. A direct attempt to deal with the argument concerning difficulty of learning is made in Experiment IV below.

EXPERIMENT II

As noted above, Experiment I can be considered to be a more sensitive test of the one-trial learning hypothesis than the experiments reported in the previous paper, but even this experiment does not give a satisfactory answer to the following argument: Suppose that repetition has tended to establish some sub-threshold associative linkages between pairs which *S*s in the control group did not yet get right. Still, it might be argued, on any given trial only a few of these pairs can be expected to be learned because of the difficulty created by intra-list interference. Since it is not too difficult for *S*s in the experimental group to learn a few new pairs on any trial, the result is that *S*s of both groups learn about the same amount per trial, and thus the number of trials to criterion is about equal for the two groups. In other words, it might be argued that the number of trials to criterion is not a sufficiently sensitive measure.

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[8] There is now fairly good evidence that familiarity with the individual items is a definite aid in forming associations. See F. D. Sheffield, The role of meaningfulness of stimulus and response in verbal learning. Doctoral thesis, Yale University, 1946; C. I. Hovland and K. H. Kurtz, Experimental studies in rote-learning theory: X. Pre-learning syllable familiarization and the length-difficulty relationship, *J. exp. Psychol.,* 44, 1952, 31-39.

This argument is far from convincing, and, even if correct, it is an admission that repetition is not playing much of a role in the formation of associations in the traditional rote-learning experiment. Nevertheless, it remains of interest to inquire whether, by some other more sensitive technique, it can be demonstrated that associations are acquired by a process of gradual strengthening.

*Method. S*s are presented with a list of 36 pairs of first names. There are three trials, on each of which the order of pairs is randomized. Following the third trial a matching test of recognition is given. From among the pairs which *S* gets wrong, eight are randomly selected, and these are mixed with eight new ones which have been set aside in advance. On the fourth trial, *S* is given these 16 pairs to learn, following which he is given a matching test. The experiment poses the question whether there will be any advantage on this fourth trial for pairs seen previously for three trials, but not yet learned as far as the criterion of correct response is concerned, over pairs never seen previously. This technique may be considered to be a sensitive one, since it allows for the possibility of learning more of the previously seen pairs than the new pairs during that fourth trial.

In a preliminary experiment, learning of the pairs prior to the final critical trial was tested by recall rather than matching. On the critical trial there was a statistically significant superiority for the previously seen (repeated) pairs. Recall is a much simpler test to give and it is the one generally used with paired associates, but in this situation it is desired to eliminate all pairs which have been learned. As noted above, if one tests by recall one gives the repeated pairs a spurious advantage because, in this case, some will be included which already have been successfully associated. It is, therefore, important to use the most sensitive test available following the third trial, because only in this way will the presence of all established associations be revealed.[9]

The matching test for any pair was given on the back of the 3 × 5-in. card used in learning. It contained the left hand member of the pair and a choice of eight alternatives for the right hand member arranged in a circular pattern. The eight alternatives were derived in the following way: There were altogether 44 name-pairs ordered in 11 blocks of 4 each for purposes of a counter balanced design. For the first *S*, the first and third blocks were set aside to be used on Trial 4 to constitute the 8 new pairs. The remaining 9 blocks constituted the 36 starting pairs. For the second *S*, the second and fourth blocks, and for the third *S* the third and fifth blocks, were set aside for use as the new pairs, and so forth. The eight alternatives on the matching test for a pair from the first block consisted of the four right-hand members of the pairs of this block and four of the pairs of the second block. For a pair from the second block, they consisted of the right hand member of the pairs from this block and of those from the third block, and so forth. Consequently, for most of the repeated pairs, all eight test-alternatives were members of pairs which *S* had seen previously. For the new pairs in the final test, four of the test-alternatives were members of pairs which *S* had seen a total of four times, and four were members of pairs which *S* had seen once. The matching test therefore differs somewhat for repeated and new pairs, but the difference is not one which might be thought to favor the new pairs. Since the test for most of the repeated pairs contains items all of which belong to repeated pairs, it is possible that *S* can eliminate some of them as choices because he recognizes them as belonging to pairs which he has learned. The possibility of doing this is considerably less with the new pairs, where half the test-items are new. The converse argument—that the test for new pairs is easier because *S* will restrict his choice to the four new items—is not too plausible because all new items have been seen during the fourth learning trial. Furthermore, all eight test-alternatives for all pairs, repeated and new, also have been seen when they appeared as alternatives in the matching test for the various pairs after the third trial. As a result, it is unlikely that *S* could tell with any certainty which items belonged to repeated unlearned pairs and which to new ones. In both cases, *S* would be confronted by items all of which were familiar because they were names, and all of which would no doubt be recognized as items which had been encountered previously in the experiment.

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[9] This reasoning applies to the test given after the third trial and not to the final test given after the fourth trial. Following the fourth trial, it no longer matters whether a more difficult test is used, since this affects old and new pairs alike. In the experiment to be described, however, matching was used throughout.

Names were used because they were familiar. Had nonsense-material been used, the repeated pairs might have gained an advantage due to the fact that the individual items would have become somewhat familiar. Since, however, name-pairs are easy to learn for many people, it was necessary to start with a rather long list if a reasonable number were to remain unlearned after several repetitions. This explains why as many as 36 pairs were used. Given-names or nicknames of one-syllable length frequently encountered in English usage were selected, with an equal number of male-male, female-female, male-female, and female-male combinations.

During all learning trials, each card was exposed, with the aid of a metronome, for 3 sec., and there was a 5-sec. interval between cards. In the test following the third trial, *S* was given ample time in which to respond, and he was allowed to guess, since the object was to weed out any pairs which *S* knew. In the final test, *S* was allowed 10 sec. to make his selection.

Results. In addition to the 15 *S*s for whom data are reported, there were 3 *S*s who were disqualified because they learned more than 28 pairs during the first 3 trials, and for whom, therefore, the requisite 8 unlearned pairs were not available on the fourth critical trial. The average number of pairs learned at the end of the third trial for the 15 *Ss* was 18.6, or roughly half of the total of 36 pairs. The mean number of repeated pairs learned on the fourth trial was 3.9 ($SD = 1.9$) and the mean number of new pairs learned was 2.8 ($SD = 1.2$). The difference is significant at the 5% level (t for paired observations = 2.6).

Before concluding that this result demonstrated that a sub-threshold associative connection had been formed and preserved as a result of the three exposures of the repeated pairs, we gave some thought to possible alternative explanations. One possibility was that, even though familiar material was used, the individual items making up the repeated pairs were reinforced by virtue of the factor of recency and so were given a spurious advantage. This possibility was ruled out by an experiment in which the names were first preëxposed by presenting them individually for three trials in random order. Subsequent learning of pairs formed from these names proved to be no better than of other name-pairs.

EXPERIMENT III

Another explanation of the outcome of Experiment II has to do with the possibility that some of the pairs scored wrong after the third trial actually had been learned to a strength adequate for a correct response. *Ss* may nevertheless have failed to answer correctly because at the time of the test, for one reason or another, they were below threshold.[10] This explanation is plausible because it must be remembered that a test did not follow each of the three learning-trials, but only the last. In view of the rather long time required to conduct this experiment it was deemed inadvisable to lengthen it still further by including these additional test-trials. Had we done so, however, many pairs scored wrong after the third trial might have been scored right after one or both of the other two trials, and as a result they would have been eliminated as 'learned.' Experiment III was conducted to determine whether this hypothesis was correct.

Method. The plan followed that of Experiment II, the major difference being that in this experiment a 5-min. intermission of light conversation was introduced after the test following the third trial. The purpose of this interval was to allow for the possibility that reminiscence might occur for those pairs scored wrong. After the intermission, *S* was told he would have another chance with those pairs he had missed. Only the latter were shown. Any of those which *S* now remembered was set aside and the eight repeated pairs for the critical fourth learning-trial were selected from those which again failed to give evidence that they had been learned.

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[10] To speak of an association as 'below threshold' in this connection is not to admit that associations are established incrementally. 'Momentary failure of recall' would be an equally good description and this is consistent with the widely recognized distinction between learning and performance.

The only other difference in procedure was that the interval between pairs, during the first three learning-trials only, was shortened from 5 to 3 sec. to hold down the total length of the experimental session. The intermission and test for reminiscence brought the time required to well over an hour.

Results. In addition to the 15 *S*s for whom data are reported, there were 10 *S*s who were disqualified because they learned more than 28 pairs, as evidenced by their performance on the first test-trial. The mean number of pairs learned by the 15 *S*s used was 19.3. These *S*s got an average of 3.6 additional pairs correct on the subsequent test which allowed for reminiscence.[11] One additional *S* was disqualified at this time because his reminiscence-score brought his total number learned over 28. The evidence of a substantial amount of reminiscence confirms the suspicion that in Experiment II a certain number of pairs scored wrong after the third trial had nevertheless been successfully associated. The question now remains whether it was the inclusion of some of these pairs among the eight presumably unlearned in the final critical trial which explains why more repeated than new pairs were learned in Experiment II. The results of the present experiment answer this question in the affirmative. The mean number of repeated and new pairs learned on the fourth trial was identical; namely 3.6 ($SD = 1.9$ for repeated pairs and 1.4 for new pairs).

The results of this experiment, therefore, confirm the previous findings that if associations are not learned sufficiently well to lead to success on a given trial, there is no evidence that any incremental, partial linkage is established. It is necessary, however, to examine one other objection.

EXPERIMENT IV

It may be argued that when learned pairs are eliminated, *S* is left with pairs which are for *him* more difficult. Consequently, he may learn randomly selected new ones somewhat more easily, thus offsetting the advantage which the repeated pairs might otherwise be expected to have. This objection is plausible in the respect that it is no doubt true that for each *S*, even with nonsense-material, but possibly more so with other material, there are certain pairs which are easy to associate because they suggest a mnemonic device and no doubt these are always among the first learned. It is very difficult to test this objection because ease of learning is defined idiosyncratically. The following procedure finally was devised:

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[11] The average number correct on the test for reminiscence on the basis of chance—assuming each *S* guessed all the time—would be 2.1. The reminiscence-score of quite a few *S*s was significantly higher than could be expected to occur by chance. Guessing, moreover, did not occur all the time.

Method. The plan of the experiment was to present first a list of seven paired items for one trial in order to ascertain which were easy and which difficult for each *S*. The unlearned ones were set aside for future use. Then *S* was given a list of 12 paired items for 4 successive trials, followed by a test. The unlearned pairs from this list also were set aside for future use. The repeated presentation of this list of 12 pairs corresponds to the procedure used in Experiments II and III. On the final critical trial, four unlearned pairs from the repeated list were randomly selected and mixed together with four unlearned pairs from the short list. Since the latter pairs are those which were *not* learned on the one trial given, they cannot be construed to be easy ones. In fact, the starting number of pairs in both lists was selected on the basis of pretesting such that it was expected that about the same proportion of the short and long lists would be learned in one and in four trials respectively. Thus, for example, if on the average roughly one-third of the list of seven paris is learned in one trial and roughly one-third of the list of 12 pairs is learned in four trials, then it can be presumed that those unlearned in both lists are of about equal difficulty. In both cases, the unlearned pairs would constitute the most difficult two-thirds of the list. The unlearned pairs of the shorter list correspond with the new pairs in Experiments II and III. The experiment poses the question whether pairs experienced four times, but not learned to the criterion of a correct response, are easier to associate than those experienced once, but not learned to the criterion of a correct response. This method of dealing with the objection concerning difficulty has the drawback that the 'new' pairs are no longer new. To compensate for this, the repeated pairs are presented for four rather than for three trials. The theory of incremental learning, however, should still predict a clear preference for the repeated items.

Pairs of two-place numbers were used. They were familiar, as were names, but they had the additional advantage for this experiment of being more difficult to learn, with the result that not too long a list was needed. With the additional trial for the short list and the additional fourth trial for the long list, the experiment would have taken too long to perform if the 36 name-pairs had been employed. The two-place numbers were taken from a table of random numbers with the additional criterion of equalizing the number of times each digit from 0 to 9 appeared in the 19 pairs used. To control for possible differences in difficulty of the particular number-pairs placed in the short and the long lists, the pairs of the two lists were interchanged after the seventh and again after the fourteenth *S*.

During the learning trials for the short and long lists, the pairs were presented in random order for 2 sec. with a 2-sec. interval between cards. On the matching tests given to determine which pairs had been learned, 8 sec. was allowed per item for the short list and 16 sec. per item for the long list. The reason for this difference was that the matching test for the pairs of the short list contained the 7 right hand members of the pairs used in learning that list, whereas the test for the pairs of the long list contained the 12 right hand members of the pairs used in learning that list. More time was required to survey the alternatives in the latter case. The items were arranged in a circular pattern. During the final critical learning-trial, the 8 pairs were presented in random order for 3 sec. with a 5-sec. interval between cards.

A recall-test was employed following the presentation of the list of eight pairs. The time per item was 5 sec. As noted above, there is no objection to testing by recall following the last critical trial.

If more than three and more than eight pairs were learned in the short and long lists respectively, there would not have remained four pairs in both cases for the final critical trial. For this reason, and to ensure that the proportion of pairs of the short list learned in one trial would be more or less equivalent to the proportion of the long list learned in four trials, the following criteria for inclusion of *S*s was established. Each *S* had to learn no less than two but no more than three pairs of the short list; he had to learn no less than three but no more than eight pairs of the long list .

Results. A considerable number of *S*s were disqualified because they failed to learn the requisite number of pairs on either the short or the long list. Others were not included in the results given below because they failed to learn any pairs in the final critical trial. For the 20 remaining *S*s, the mean number of pairs learned for the short list was 2.3 or 33% (2.3/7); the mean for the long list was 5.3 or 44% (5.3/12). Although these percentages are not identical, they are sufficiently close to warrant the statement that on the average, for both lists, the easiest third (approximately) of the pairs was eliminated, where *easiest* is defined as those learned first. The question now remains whether of the remaining unlearned pairs, presumed to be of about equal difficulty for both lists, the repeated ones had an advantage on the final critical trial.

The mean number of pairs correct on the final trial was 0.80 ($SD = 0.81$) for pairs from the short list and 1.05 ($SD = 0.74$) for pairs from the long list. The difference falls far short of statistical significance ($t = 0.93$). Thus there was no advantage for the repeated pairs over the pairs experienced only once previously. It may be noted that these scores are quite low in comparison with those obtained on the final trial of the preceding experiments. There are two reasons for this. First, a recall-test was employed rather than a matching test. Secondly, number-pairs are more difficult to learn than name-pairs. The scores would have been even lower if the many *S*s who failed to get any items correct on the final trial had not been eliminated. Of those *S*s who were included, many succeeded in learning only one pair. This poorer performance is not, however, a valid objection to the experiment. Of those few pairs learned by each *S*, the possibility certainly existed for the advantage to be given to the repeated ones.[12]

This experiment is particularly important because it does not contain any of the possible flaws mentioned at the beginning of the paper. As in the case of Experiments II and III, the technique is a sensitive one and the objection concerning the difficulty of unlearned pairs has been met.

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[12] In this experiment, no attempt was made to take reminiscence into account. Possibly some pairs of both lists would have been eliminated had this been done, with perhaps a greater proportion in the case of the repeated list.

As to the possibility of wrong associations, developing to the repeated pairs, thereby opposing the advantage of repetition, the following point must be considered: Unlike the situation in Experiment III, in the present experiment *S* has had one previous test with pairs from both lists. Thus, if any such wrong associations occur during these preliminary tests, they are just as likely to do so for the pairs seen once as for the pairs in the repeated list.

Although the results do not reveal a significant advantage for the repeated pairs, one could say that there was a trend in their favor which might become significant with a larger number of cases. For this reason it was decided to repeat the experiment with an additional 20 *S*s. The procedure was identical except that, as a further precaution, an effort was made to exclude from the final critical trial wrong pairs to which *S* had given an erroneous response from both the short and the long list, *i.e.* to include only pairs which were wrong because *S* made no response at all. It was not always possible, however, to exclude such cases, but a complete record was kept of the nature of the error of every pair included in the final test.

The results were as follows: Again a considerable number of *S*s were disqualified for failure to learn the requisite number of pairs in either list or to learn any at all on the final critical trial. For the 19 valid *S*s (one more was disqualified because of an error by *E*), the mean number of pairs of the short list learned was 2.4 or 34% (2.4/7); the mean number of the long list was 4.5 or 37% (4.4/12). These values are even closer to one another than was the case for the first group. The mean number of pairs correct on the final trial was 1.1 ($SD = 0.77$) for the short list and 1.1 ($SD = 0.77$) for the long list. There is thus no difference at all for this group. One can also combine the data of the two experiments. When this is done, the means are 0.95 ($SD = 0.77$) for the short list and 1.08 ($SD = 0.79$) for the long list. For the combined N of 39, this difference still is far short of significance ($t = 0.68$).

It was not possible in every case to exclude errors of wrong response from the final list for this second group, because there would not have been four error-pairs of no response left for each list in the great majority of cases; but this happened primarily for the short list. For the repeated pairs in the long list, it was only necessary to include 6 such instances, the remaining 70 being errors of no response. For the short list there were a total of 33 errors of wrong response, the remaining 43 being errors of no response. Thus the objection concerning the possible disadvantage for repeated pairs caused by the presence of wrong associations is more than met. Very few such instances enter into the critical list whereas many more from the non-repeated list do. In point of fact, there was only one case in the total of 39 errors of wrong response in which *S* repeated that particular wrong response on the final critical trial. Furthermore, the proportion of correct responses during the final trial was about the same for no response as for error-pairs of wrong response.

DISCUSSION

The question arises of the implications of these results for other types of learning-situation. It is possible that in conditioning, the actual achievement of the association is established on one trial, as Voeks has argued in support of Guthrie's theory.[13] Evidence for one-trial learning in animals has appeared occasionally in the literature.[14] In problem-solving situations, the sudden appearance of solutions has of course long been cited by cognitively oriented psychologists in opposition to theories which stress the role of repetition. It has been shown that children may acquire an understanding of a logical relationship suddenly, on a particular trial.[15] In the type of problem involving discrimination between two alternatives, Krechevsky has argued for a discontinuity of the learning process.[16]

The question may be raised as to why acquisition does not always occur in one trial if the point of view developed in this paper is essentially correct. In the case of human associative learning, as noted in the earlier paper, the difficulty rests with the interference set up by the presence of the many other items or pairs in the list. Most *S*s are able to form only a few associations on any given trial. Why this is the case is not known, but in the opinion of the authors the procedure initiated by Ebbinghaus of learning a long list is a very complex one indeed, one which cannot be completely understood without realizing the role played by higher mental processes. Special attitudes of intention to learn some pairs, and perhaps even intention to ignore other pairs on a given trial, enter in. Furthermore, success often depends upon the discovery of a suitable mnemonic device. If, on the other hand, a situation is created whereby *S* is confronted with only one pair of items, then even without intention, learning will occur readily on one trial.[17] If conditioning does not occur on one trial, as Voeks maintains, then an explanation is needed as to why it does not occur on the very first trial. While there are explanations available consistent with Voeks's position, another possibility is that attention to, and awareness of, the contiguity of the conditioned and unconditioned stimuli is required.

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[13] V. W. Voeks, Acquisition of S-R connections: A test of Hull's and Guthrie's theories, *J. exp. Psychol.*, 47, 1954, 137-147.

[14] B. B. Hudson, One-trial learning in rats, *Psychol. Bull.*, 36, 1939, 643; A further study of one trial learning in rats, *ibid.*, 36, 1939, 518.

[15] George Kreezer and K. M. Dallenbach, Learning the relation of opposition, this JOURNAL, 41, 1929, 432-441; J. L. Lacey and K. M. Dallenbach, Acquisition by children of the cause-effect relationship, *ibid.*, 52, 1939, 103-110.

[16] I. Krechevsky, 'Hypotheses' in rats, *Psychol. Rev.* 39, 1932, 516-532. (It is possible, of course, to hold for discontinuity in discriminative learning and at the same time to hold for the formation of associations by repetition in rote-learning.)

In all the experiments reported here and in the previous paper, pairs of items have been used, with sequence of pairs randomized from trial to trial. The question arises, therefore, as to the applicability of these findings to serial learning. The decision to use pairs was based on the belief that the lowest common denominator in associative learning is a pair of items. Serial learning involves, among other things, localizing the items in a spatio-temporal sequence; it also involves the possibility of multiple associations to each item. Furthermore, it has a unidirectional character which is not necessarily true of the paired-item situation. It is, therefore, a more complex situation, a complete understanding of which awaits clarification of fundamental processes. Nevertheless, it seems reasonable to suppose that such a list is learned to criterion by the acquisition of segments of it, each on one trial, rather than by the gradual emergence of the entire list as the incremental theory would have it. Difficulties would surely arise, however, in attempting to test this assumption by techniques like those employed here. This problem therefore remains as a challenge for the future.

The results of the above experiments confirm the findings of the previously reported work and, by eliminating possible flaws in the earlier experimental design tend to cast considerable doubt on the incremental theory of forming associations. Yet it seems clear from experience in daily life and from the results of experiments on overlearning that associations become particularly strong and dependable with repetition. The only conclusion to be drawn is that repetition will have a beneficial effect upon an association only after it has been successfully established. It is probable, therefore, that pairs learned during the first few trials will become relatively strong associations by virtue of the many repetitions they are given thereafter, whereas those learned during the last few trials will remain relatively weak. Preliminary results of experiments testing for subsequent retention of pairs acquired at different stages of learning confirm this reasoning, but they will not be reported at this time. Underwood has obtained evidence pointing in the same direction.[18]

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[17] F. B. Springfield, The learning and retention of a single pair of associates, Bachelor's thesis, New School for Social Research, New York, 1955.

[18] B. J. Underwood, *Experimental Psychology*, 1949, 525-527.

It is probable that failure to distinguish between the influence of repetition before and after initial acquisition has contributed to the widespread belief in the incremental theory. We can observe the beneficial effects of repetition in daily life and perhaps even the relationship between frequency and associative strength.[19] Experiments on overlearning have given quantitative expression to this relationship.[20] The fact that lists of items are learned only with repetition then appears as one more instance of the universality of the principle of gradual learning.

SUMMARY

Four experiments were performed to investigate possible objections to the results previously reported that associations are formed in one trial.

Experiment I was designed to control for the possibility that the technique of substituting new pairs for unlearned pairs eliminates one of the usual causes of difficulty in learning a list of pairs; namely, wrong associations. By interchanging the items of pairs after every trial, it was shown that the new pairs thus derived were also no more difficult to learn than repeated pairs in the traditional procedure, although here the new pairs might be expected to carry with them any wrong associations that had been formed.

Experiment II was designed to provide a more sensitive test of the incremental theory of associative learning. After three repetitions of a long list of paired names, unlearned pairs were pitted against new pairs on a final critical trial. A significantly greater number of repeated than new pairs was learned.

Experiment III was designed to test the possibility that the results of Experiment II were due to the fact that some pairs considered to be unlearned actually were learned. By allowing for reminiscence, additional repeated pairs were excluded from use on the final trial. The results now showed no difference between repeated and new pairs.

Experiment IV was designed to control for the possibility that new pairs were given a spurious advantage in the above experiments because they are, on the whole, easier than those of the initial pairs which remain

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[19] For some reason, however, the many examples in daily life of the non-beneficial effect of repetition prior to initial acquisition have not impressed psychologists as important for theory. We may, for example, dial a telephone number again and again without learning it. Once we make up our minds to commit it to memory, however, we are apt to succeed at the very first attempt. Thereafter, each repetition may strengthen the association.

[20] See W. C. F. Krueger, The effects of overlearning on retention, *J. exp. Psychol.*, 12, 1929, 71-78.

unlearned. Pairs experienced once but not learned were pitted against those of a longer list experienced four times but not learned. Although here it could be presumed that the two sets of pairs were of equal difficulty, there was still no significant difference in the number of each learned on the final critical trial. This experiment was shown not to contain any of the possible flaws discussed in the paper.

These results were interpreted as supporting the conclusion that in rote-learning associations are formed on one trial and not by a process of gradual strengthening. The probable role of repetition subsequent to initial acquisition in strengthening associations was discussed.

THE FORMATION OF CONCEPTS WITH NON-VERBAL AUDITORY STIMULI

By FRED L. ROYER, VA Hospital, Perry Point, Maryland

Though research in the formation of concepts has been limited largely to vision, a few extensions have been made into other fields. The methods used in the study of concept-formation are of wide applicability. Leeper, for example, claims that they are paradigms in the study of learning;[1] the present author believes that they have special value in the learning of the meaning of sounds.

The sounds that have the most significance for organisms are marked, as stimuli, by extreme variability, great complexity, and temporal patterning. They are not amenable to prolonged inspection or control by the organism to the extent that visual stimuli are. In spite of these characteristics, sounds provide important information to most organisms, and, in humans, are the source of the most vital information, *i.e.* social communication. This study attempts to extend the experiments on concept-formation into the auditory field and to test hypotheses arising from the experiment as a model for learning-theory.

Concept-formation is here defined as the process of learning a specific response to recurring psychologically invariable stimulus-features in a variaible stimulus-context. Symbolically, the recurrent stimulus-situation (S) will consist of an invariable component (a) and a variable component (x, y, or z); the organism learns to respond (R_a) to recurrent complex, variable events $(S_{a,x};\ S_{a,y};\ S_{a,z})$.

In the typical experiment in concept-formation, of which Hull's is representative,[2] the subject (*S*) is presented with a number of stimulus-objects to which he learns appropriate responses; when this learning has occurred, *S* is presented with a number of new objects, having some feature in common with the first, to which he learns to give the same responses as before. This procedure is repeated until the concept is learned. The demonstration of the attainment of the concept is anticipating the correct response to a new list of objects on their first presentation. The test of the learning which is most often used is a generalized list in which new objects are presented representing all the concept-categories after a certain number of learning-lists have been completed.

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[1] Robert Leeper, Concept-formation as a paradigm of learning, *Amer. Psychologist*, 4, 1949, 386 f.

[2] C. L. Hull, Quantitative aspects of the evolution of concepts, *Psychol. Monogr.*, 28, 1920, (No. 123), 1-85.

This typical experiment is limited as a model by the fact that the experimenter (*E*) provides a common response in using the technique of paired-associates.[3] A logical modification of this model was made to increase its generality to situations of free response and is symbolized as:

$$\begin{array}{l} S_{a,x} \longrightarrow R_{x'} \\ S_{a,y} \longrightarrow R_{y'} \\ S_{a,z} \longrightarrow R_{z'} \\ \quad\cdot \\ \quad\cdot \\ \quad\cdot \\ \quad\cdot \\ S_{a(n)} \longrightarrow R_{a} \end{array}$$

where $S_{a,x}$, $S_{a,y}$; and $S_{a,z}$ represent situations, occurring sequentially in time (the vertical axis), made up of an invariable and a variable component. $R_{x'}$, $R_{y'}$, and $R_{z'}$ represent different responses being made to some aspect of their relative variable components of the situation. At some point in time and under circumstances not made immediately explicit by the model, a response (R_a) occurs specifically to any situation in which the invariable component, a, appears together with any number of variable components, (n).

A brief exposition of the theoretical treatment of the model in its present germinal state is presented to clarify the development of the problem and demonstrate its potential fruitfulness for provoking new hypotheses for research, since it is an associationistic, continuity, non-field theory from which discontinuity, latent learning, and cognitive phenomena may be predicted. By its rationale, it should be amenable to the mathematics of probability. Although it may be more taxing on the reader, only an abstracted presentation, with a minimum of supporting observations and logical development, can be made because of the limitations of space.

(1) The basic postulate of the theory is that all neural structures are modified and the modification sustained as a function of the frequency of stimulation. The modification results in a reduction of the variability of behavior, since there is an increase in the probability that certain structures will participate in a behavioral event.

(2) The model places particular importance on the stimulus-events through time. From the phenomenological point of view, it is an emphasis on the reduction of the variability of the surrounding world for the organism through the modification of afferent structures (which, for want of a better name, is meant to include all structures except the corticofugal ones). Support for the postulated process of modification of afferent structures is found in data showing reductions of the threshold of

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[3] This limitation is no greater, of course, than that posed by *E*'s restriction on a rat's repertoire of responses by placing it in a T-maze.

perceptibility of a stimulus and the threshold of discrimination as a function of stimulation.[4]

(3) A response is additional stimulation for the organism. This well-known fact has considerable significance. In *tabes dorsalis,* the fasciculi carrying proprioceptive fibers are damaged. The person so affected is unable to walk except when he can watch his feet. It appears that the proprioceptive stimuli exercise some controlling role in voluntary motor responses and that another modality can substitute for the missing 'feedback.' The development of motor skills indicates that there is something more important than simple feedback; something must happen to the information fed back.

We observe that in the human neonate (perhaps because of some general drive-state) diffuse variable motor activity occurs. Despite the variability, or rather because of it, certain proprioceptive stimulations occur more frequently than others and result in modifications of the stimulated afferent structures. We conceptualize these modified structures which control the effector as 'kinetic schemata' and are considered to be the essential building blocks of behavior. Motor skill, then, may be interpreted as resulting from the increasing strength of the kinetic schemata developing out of the frequency of stimulation of certain structures; each sequential reduction in variability of motion increases the probability of restimulation of more greatly restricted proprioceptive structures, hence further reducing the variability.

(4) This modification process in other sensory modalities is conceptualized as 'sensory schemata.' This term may be applied to the modification resulting from recurrent stimulation by a particular stimulus. Schemata developing from many different stimulus-objects with common features we define as 'concepts.' A consequence of the theory as applied to concepts is that the more varied the experience with stimulus-objects with a common feature, the greater and more general is the modification and, of course, the greater the probability that many more new objects will elicit the response. This has been experimentally supported.[5]

(5) Most of our concepts include sensory information from many modalities; *e.g.* apples are red, round, firm, smooth, crunchy, and have a certain taste. By logic alone one can see that it would be impossible for the organism to maintain any systematic knowledge of his surroundings unless there were some unity of these polymodal events. This suggests that all simultaneous, or nearly simultaneous, neural modifications form a unified modification.

It is also logical that what forms as a unit acts as a unit. This leads to our second postulate that simultaneous events form a unitized modification and the involved structures function as a unit in the initiation and control of effectors. This unitized functioning is seen in *tabes,* as previously mentioned.

(6) This brief introduction to the theory has attempted to form a ground-work for that which follows. Other aspects of the theory, such as principles of reorganization of schemata, the interpretation of the law of effect in terms of the law of frequency and the congruity of stimulus-events with schemata, and the concept of be-

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[4] William Bevan and Karl Zener, Some influences of past experience upon the perceptual thresholds of visual forms, this JOURNAL, 65, 1952, 434-442; E. J. Gibson, Improvement in perceptual judgments as a function of controlled practice or training, *Psychol. Bull.,* 50, 1953, 401-431.

[5] M. F. Callantine and J. M. Warren, Learning sets in human concept formation, *Psychol. Reports,* 1, 1955, 363-367.

havior modes, require a shift from the usual frame of reference. Since they are not particularly pertinent here and since extensive treatment of them would be necessary to accomplish the shift, they will not be dealt with.

EXPERIMENT I

The theory described above has not dealt directly with the stimulus-aspects of the paradigm. In presenting the model, we have referred to the stimulus-situation in terms of everything happening at a moment in time. Also, it is possible and quite customary in research to consider a single stimulus and, hence, to define the invariable feature of several single stimuli in a series of events as any point along a molar-molecular continum. Thus it might be described as a 'pure' physical stimulus, e.g. a specific wave-length of light; or it might be described as a triangle, an apple, a person, or a noise. At an even more complex level, it might be described as a relationship among objects: numerosity, arrangement; or as a sequence of objects or events.

With respect to auditory stimuli, the invariable feature might be described as a tone of 1000 ~ per sec. in a complex tone or as a specific relationship among the various frequencies comprising the complex tone. The purpose of this experiment was to provide basic information about concept-formation in audition by testing whether the effects of differential learning could be observed in the formation of concepts based on auditory dimensions at such a molecular level.

The independent variable in the experiment was the ordering of stimuli to which a common response was to be learned according to a common physical dimension. That is, since complex stimuli are by definition multi-dimensional, concepts may be formed to any one dimension of the stimuli by requiring the same response to be made to those stimuli with a particular constant dimension. In the case of complex harmonic tones, two stimuli may be assigned a common response on the basis of either the harmonic structure or a particular prominent frequency in the complex, depending upon which were constant in the two stimuli.

The dependent variable was the number of correct identifications of stimuli in the generalized lists.

Method: (1) Subjects. To obtain the greatest possible homogeneity among *Ss* and to utilize *Ss* with experience necessary for making the kind of discriminations demanded, 20 *Ss* were drawn from a population of musicians. This parent population was defined as majors in music education and members of the University's concert band.

(2) Apparatus: (a) Recording. A block diagram of the recording system is shown in Fig. 1; all of the units of the system were commercially produced except the

attenuator-mixer-amplifier, which was specially designed and constructed. Three audio-oscillators supplied signals of one volt each to the attenuator-mixer-amplifier. A monitor system in the latter unit supplied each unmixed signal to a frequency-counter and a voltmeter. The frequency of each signal was determined grossly by the frequency-counter; a cathode-ray oscilloscope was used for fine tuning of any pair of oscillators by means of observed Lissajous figures. Each unmixed signal was appropriately attenuated before mixing.

The output of the mixed signal was adjusted to a level of one volt, and was fed through one of three timing switches to the tape-recorders.[6] Speech was supplied by a microphone and amplifier through another timing-switch to the tape-recorder. The timing-switches were so adjusted that upon activation they successively closed the signal-circuit for 3 sec., provided a 2-sec. interval of silence, and closed the microphone-circuit for 1 sec.

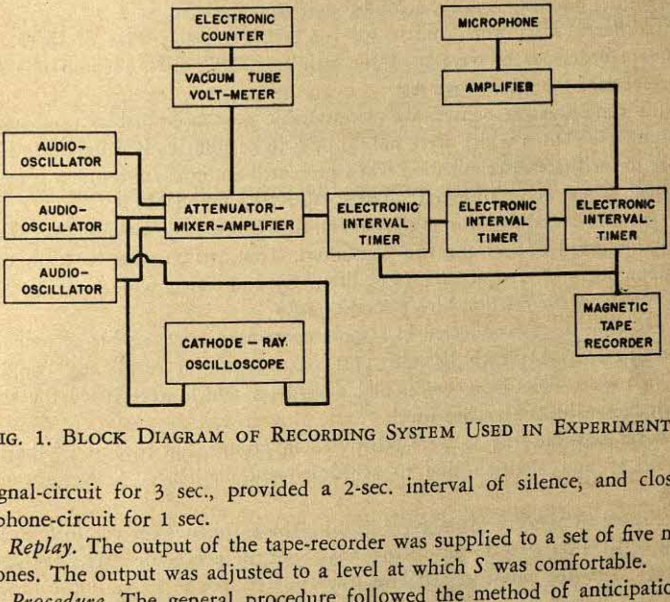

FIG. 1. BLOCK DIAGRAM OF RECORDING SYSTEM USED IN EXPERIMENT I

(b) Replay. The output of the tape-recorder was supplied to a set of five matched earphones. The output was adjusted to a level at which *S* was comfortable.

(3) Procedure. The general procedure followed the method of anticipation used by Hull,[7] and Heidbreder.[8] It was arbitrarily decided that six 'concepts' would be learned.

The stimuli were complex tones synthesized from three pure-tone generators. Each stimulus-complex was so constructed that it could be used in each of two conditions. In one condition the concepts were based upon a recurrent identical frequency. For these concepts, six frequencies, approximately 250 mels apart, were selected: 120, 340, 660, 900, 1400 and 2000 ~ per sec. For example, HIF was the appropriate

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[6] A description of the timers and their operation may be found in T. A. Hunter and J. S. Brown, A decade-type electronic interval-timer, this JOURNAL, 62, 1949, 570-575.

[7] Hull, *op. cit.*, 1-85.

[8] E. F. Heidbreder, The attainment of concepts: I. Terminology and method. *J. gen. Psychol.*, 35, 1946, 173-189.

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response for all complex tones characterized by the presence of a 120 ~ tone, as 120, 240, and 480~ .

In the other condition the concepts were based upon a recurrent harmonic relationship among the three frequencies comprising the tone. For these concepts six different sets of ratios were chosen. These ratios are expressed as partials of a harmonic complex tone.[9] These ratios were: 1:2:4, 10:11:12, 4:5:7, 5:8:12, 5:8:9, and 3:4:9. For example, BEP was the appropriate response for all complex tones characterized by a relationship of 1:2:4 among the partials, as 120, 240 and 480 ~. Our examples for the two conditions show that the same tone might be identified either by the presence of a partial of a particular frequency 10 db. greater in intensity than the other partials comprising the complex, as in the first condition mentioned above, or by the qualitative aspect arising from a particular relationship of the partials in the complex, as in the second condition mentioned.

Eight different lists were constructed for each condition. Five of these lists, in six random orders each, were used for training. Three lists, each in one random order, comprised the generalized test.

For the conceptual responses, six monosyllabic nonsense-syllables of low associative value were chosen which were judged by *E* to be sufficiently different in phonetic structure to facilitate discrimination when presented auditorily through a communication-system. They were: BEP, HIF, DAX, VEK, FAJ, and MUB.

The *Ss* were scheduled for a convenient time-block. The time-blocks were then randomly assigned one of the two conditions. Thus, 10 *Ss* were to learn concepts based upon identical frequencies (Condition E) and 10 *Ss* were to learn concepts based upon harmonic relationships (Condition R).

The experiment was conducted in a room containing a large table accommodating five listening-posts. At each listening-post were a pair of earphones, two colored pencils, a pack of 3 × 5-in. cards, and a card on which were listed the six nonsense-syllables used as response-words.

Whenever possible, the *Ss* were scheduled in groups, but because of the limited supply of *Ss* and attrition due to schedule-manipulation, 4 *Ss* were run individually.

After instructions were given for adjusting the earphones for comfort, they were removed and the *Ss* were given instructions about the procedure. After familiarizing themselves with the nonsense-syllables, they put on the earphones; then the tape-recorder was turned on and a listening-trial administered. Upon completion of this trial, the *Ss* began learning the first list, writing the anticipated responses before the correct response was given. If *S* had not rendered the list perfectly after the presentation of the six orders, he turned his card over and began another repetition. If *S* learned the list before other members of the group, his earphones were turned off.

Beginning with each new list, the *Ss* were instructed that the same names would be used to identify new sounds in the next list. No listening-trial was given. When the fifth list had been learned, the *Ss* were instructed to identify new sounds on the basis of what they had already learned. When the test was completed, the tape was replayed to allow *S*s to make any corrections they wished.

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[9] A harmonic complex tone consists of a fundamental and a series of overtones; in expressing the components of the complex, Number 1 represents the partial which is the fundamental. The number assigned to each successive partial represents the mathematical ratio of that frequency with respect to the fundamental.

Results. The results of the analysis of variance of the number of correct identifications of stimuli in the generalized test are summarized in Table I. The analysis yielded an *F*-ratio of 3.04, which is not significant. We concluded, therefore, that there are no significant differences in forming concepts in a variable context between those based upon a recurrent, identical, auditory stimulus-element and those based upon a recurrent relationship among auditory stimulus-elements.

Table II presents the means and sample variances for the two conditions. It will be noted that the mean number of correct identifications is so low that one must question whether anything was learned conceptwise. The observation led to an inspection of the raw data to determine if the *S*s of both conditions were responding similarly, but on a dimension different from either of the dimensions selected by *E*. If so, errors would be artifacts. Assuming this to be the case, one would expect that, the *S* having learned one list, responses would be assigned to those stimuli sounding most alike upon first presentation of the next list.

TABLE I

SUMMARY OF ANALYSIS OF VARIANCE: EXPERIMENT I

(Number of correct identifications of stimuli in test of generalization)

Source	df.	Sum of squares	Mean square	F-ratio
Between groups	1	9.8	9.80	3.04*
Within groups	18	58.0	3.22	
Total	19	67.8		

* Not significant.

TABLE II

MEANS AND VARIANCE OF THE NUMBER OF CORRECT IDENTIFICATIONS OF STIMULI IN TEST OF GENERALIZATION: EXPERIMENT I

Condition	*N*	Mean	Variance
E	10	4.40	4.49
R	10	5.80	1.96

Responses made to each first presentation of a list of stimuli were compared with responses to the immediately preceding list of stimuli for both conditions. A definite trend was observed. Regardless of the condition, the *S*s appear to respond similarly on the basis of a qualitative aspect of the stimuli determined by two factors simultaneously: the harmonic relationship of the frequencies in the complex and the frequency-range in which they appear.

Discussion. The results of the analysis of variance indicate that no differential learning-effects could be observed in the formation of concepts based upon the dimensions selected. One might be inclined to advance the hypothesis that the discriminations required of the *S*s in the 'frequency-element' condition were too difficult. Nevertheless, the discriminations required of the *S*s in the 'relationship' condition were timbre-discriminations by the classical definition of the physical properties of a sound which produce the qualitative effects. Certainly these *S*s have had considerable training and experience in making these kinds of discriminations.

The inspection of the raw data led to the conviction, which could not be statistically tested, that the *S*s under both conditions were responding commonly to a dimension not selected by *E*. This is a finding of importance to theoretical considerations of learning and of interest in audition.

In an attempt to determine whether the trend found in the raw data was an artifact or a plausible hypothesis, the author found that a theory of timbre differing from the classical one has been made. Bartholomew, in presenting these two theories, observes that neither "tells the story alone."[10] The classical harmonic theory, on the basis of which the relationship-stimuli were constructed in this experiment, states that the characteristic tone-quality is due entirely to the relationship among the partials; the quality should not vary, therefore, with changes in the frequency of the partials if the relationship remains constant. The formant theory, on the other hand, states that the characteristic tone-quality is due to the relative strengthening of whatever partial lies within a fixed or relatively fixed region of the musical scale. The observations noted here support, in general, the formant theory; but they suggest that it is not a particular partial but several partials in a relatively constant mathematical relationship which lie within a relatively fixed region that determines the quality.

It is important theoretically that there may be limits to identifying the invariable component of a stimulus as physically measured. This emphasizes a part of the definition of concept-formation given above: that the invariable component in several situations must be *psychologically* invariable. This means, of course, that *E* must choose his stimulus carefully. One may not have difficulty learning a response to a particular stimulus, just as the *S*s in this experiment had no trouble learning a list; but, if a test of generalization of the behavior to another stimulus is crucial to testing an hypothesis, one may well obtain negative results where the common dimension is arbitrarily selected.

EXPERIMENT II

In the theoretical discussion, the hypothesis of the modification of sensory structures was advanced in the context of stimulus-events which consistently accompany a response-event. The model suggests, of course, that not all stimulations are contiguous in time with a particular response. Theoretically, we must assume that a strong sensory schemata may develop by frequent stimulation while having a very weak potential for eliciting any of the many different responses with which it has occurred. It may be hypothesized that such a schemata has a behavioral potential proportional to its strength. This would increase the probability of its eliciting a response if conditions are appropriate for establishing a response. This hypothesis has direct implications for latent learning and is one hypothesis tested in this experiment.

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[10] W. T. Bartholomew, *Acoustics of Music*, 1942, 16 f.

This indicates that the formation of schemata may develop under two conditions: (1) when a particular stimulus occurs consistently together with another stimulus with the capacity to evoke a response; and (2) when a particular stimulus occurs frequently but without correlation with another stimulus with the capacity to evoke a response.

Now the model study of concept-formation indicates that variable complex stimuli can be ordered hierarchically; thus we might conceptualize people on the basis of the most frequent stimulations and later fractionate the concept into men and women on the basis of less frequent but consistent differential stimuli. Responses may be learned to either the more general order or the lower orders. A wealth of observational data from developmental psychology indicates that most frequently a higher order response is learned first and that finer discriminations and differential responses are learned later. We would hypothesize that differential learning-effects occur depending on whether differential response followed or did not follow learning higher order responses. This hypothesis was also tested in this experiment.

It will also be apparent that the different conditions of learning lower order responses may have been preceded by conditions favorable to the formation of schemata, as discussed above. Hence, interactive effects might be expected to be obtained.

Method: (1) Design. A factorial design was chosen to investigate the possible independent and interactive effects on the formation of concepts of the two variables: (1) experience with stimuli prior to learning differential responses and in the absence of specific differential reinforcement; and (2) learning hierarchial responses. Two conditions were established for each variable: there is one condition in which prior experience is given (*PE*), and one in which it is not (*NPE*); there is one condition in which higher order concepts are learned and, subsequently, discriminative lower order concepts (*HL*); and another condition in which lower order concepts are learned (*L*). In the latter condition, to equate for practice-effects, higher order concepts are learned on irrelevant material. Within the factorial design, then, there were four conditions: *NPE–HL, PE–HL, NPE–L,* and *PE–L.* The dependent variable was the number of correct identifications of the stimuli in the generalized test.

(2) Subjects. A total of 28 *S*s served in Experiment II. They were drawn, as in Experiment I, from the population of musicians previously mentioned.

(3) Apparatus: (a) Recording. A block diagram of the recording-system is shown in Fig. 2. This system differed slightly from that reported in Experiment I. Six audio-oscillators were used instead of three. One oscillator included a frequency-modulator; the Békésy-type audiometer permitted varying the frequency and intensity of its signal as well as pulsing of it. Since responses were not recorded on the tape in this experiment, a white noise generator supplanted the microphone of the system of Experiment I. This allowed a brief burst of noise which served as a signal to *E* to present the appropriate response.

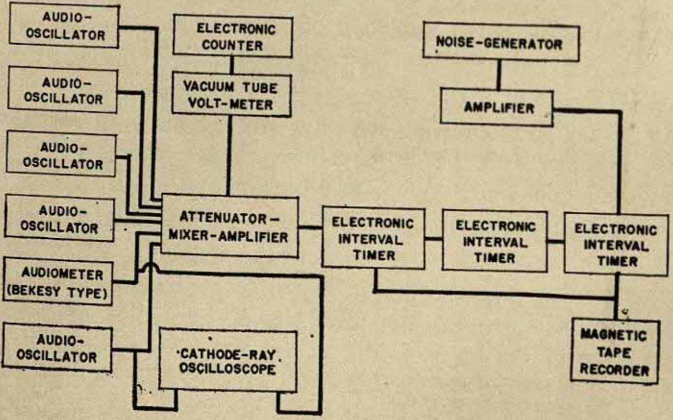

FIG. 2. BLOCK DIAGRAM OF RECORDING SYSTEM USED IN EXPERIMENT II

(b) Replay. The replay-system was similar to that reported in Experiment I. A microphone and amplifier with a 'talk-switch' were placed in parallel with the output of the tape-recorder to allow presentation of the response-words. *E* monitored presentation of the stimuli through one of the five matched sets of earphones.

(4) Procedure. The general procedure in this experiment was also the method of anticipation. Six concepts were developed. Eight different lists were constructed for the stimuli to be used by all groups. Five were used for the training-series, three for tests of generalization. For each training list, six different random orders of presentation of the stimuli were prepared; for each generalized list, one random order was prepared.

Most sounds which an organism must discriminate are not static ones such as those used in Experiment I. Therefore, the stimuli used in this experiment include some involving temporal changes along various dimensions. The physical dimensions of the stimuli are indicated in Table III. The table presents the physical characteristics of the sounds constituting the two sets of stimuli required by the design;

i.e. one set for higher and lower order concepts and one set for higher order concepts only, to be used for irrelevant practice-material.

For the conceptual responses, nine different monosyllabic, nonsense-syllables of low association-value were selected. Syllables used for higher order concepts were: JAT, ZOG, and TEV. For lower order concepts the following were used: BEP, HIF, DAX, VEK, FAJ, and MUB.

TABLE III

DESCRIPTION OF THE PHYSICAL CHARACTERISTICS OF SOUNDS WHICH SERVED AS THE BASIS OF THE CONCEPTS: EXPERIMENT II

(Part A describes the sounds that served as the basis of lower order concepts for the *HL* group of *S*s; Part B, those that served as the basis of higher order concepts for the *L* group.)

	Higher order		Lower order	
	Name	Description	Name	Description
Part A	TEV	One partial changes frequency; others are stable	BEP	Ascending change of 3rd partial
			FAJ	Descending change of 2nd partial
	JAT	One partial is frequency modulated; others are stable	HIF	2nd partial frequency modulated
			MUB	6th partial frequency modulated
	ZOG	Stable tone; no change	DAX	Partials 1, 3, 4, 5, 7, 9; all same relative intensity
			VEK	Partials 1, 2, 4, 6, 8, 10; respective relative intensity: −5, 0, −5, −10, −10, −5 db.
Part B	TEV	One partial changes in intensity; others are stable		Decreasing change of 6th partial
				Increasing change of 3rd partial
	JAT	One partial is pulsed; others are stable		3rd partial pulsed at 1/sec. rate
				6th partial pulsed at 2/sec. rate
	ZOG	Stable tone; no change		Partials 1, 2, 4, 10, 11, 12; respective relative intensity: −10, −5, 0, 0, 0, 0 db.
				Partials 1, 3, 5, 6, 7, 9; respective relative intensity: −5, 0, 0, −5, −5, 0 db.

The *S*s, 28 in number, were scheduled for a convenient time-block. The time-blocks were then randomly assigned one of the four conditions. Seven *S*s were used in each of the four conditions.

After the earphones had been adjusted for comfort, they were removed and *S*s in the prior experience-conditions (*PE–HL* and *PE–L*) were simply instructed to listen to a number of sounds recorded on the tape. When the earphones had been replaced, the *Ss* listened to all eight lists of the relevant stimuli, through two repetitions, making a total of 96 stimulations or 16 per concept.

The *Ss* in these two conditions then followed the same procedure as the other groups. They familiarized themselves with the nonsense-syllables for the higher order concepts. Following instructions, a listening-trial was administered. Upon completion of this trial, the *S* began learning the first list, writing the anticipated response before the correct response was given. When three higher order lists had been learned, they familiarized themselves with the nonsense-syllables for the lower order concepts.[11] They were then instructed to follow the same procedure as before.

When the fifth lower order list had been learned, the *Ss* were instructed to identify new sounds on the basis of what they had just learned, *i.e.* using the lower order responses. When the test was completed, the tape was replayed to allow the *Ss* to make any corrections they wished.

TABLE IV

Summary of Analysis of Variance: Experiment II

(Number of correct identifications of stimuli in test of generalization)

Source	df.	Sum of squares	Mean square	F-ratio
Between experience	1	66.07	66.07	13.35*
Between hierarchies	1	0.07	0.07	0.01
Experience×hierarchies	1	26.07	26.07	5.27†
Error	24	118.86	4.95	
Total	27	211.00		

* Significant beyond the 1% level.
† Significant beyond the 5% level.

Results. The results of the analysis of variance of the number of correct identifications of stimuli on the generalized test are summarized in Table IV. The means for each of the conditions are presented in Table V.

As expected, the analysis of variance indicated a statistically significant difference between the means of the conditions of prior experience. The expected difference between the means of the conditions of hierarchical order did not occur; the analysis did indicate a significant variance of experience by hierarchy, the interaction of the two variables. Thus, the results indicate that prior experience with stimuli, under the conditions of this experiment, is a significant source of variance in the formation of concepts with auditory stimuli. Also, the significant interaction indicates that although learning a hierarchial order has no independent effect upon the formation of auditory concepts, it does yield a signficant effect in the presence of prior experience.

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[11] Originally, five different lists were constructed for the groups learning higher order concepts with irrelevant material, with each list having six different random orders. When it was found necessary to make changes to cut down the time required for running a group, two of these lists were discarded to provide training in higher order material with only three lists. This was done since, in pilot runs, it was found that each of 10 *Ss*, without exception, correctly identified the higher order concepts of the fourth and fifth lists upon first presentation, demonstrating that only three lists are necessary to attain these concepts perfectly.

Discussion. The significant results of the prior experience variable indicate that *S*s with prior experience make significantly more correct generalizations than those without; in effect, they have learned the concepts better. The conditions suggest that this results from a primitive or basic learning process which operates independently of any instrumental responding on the part of the organism; it also operates independently of any specific, experimenter-controlled reinforcement. The apparent learning which takes place under these conditions supports the modified postulate.

TABLE V

Means of the Number of Correct Identifications of Stimuli in Test of Generalization by Condition: Experiment II

Hierarchical order	Experience		Total
	NPE	*PE*	
L	10.00	11.13	10.56
HL	8.00	13.00	10.50
Total	9.00	12.06	

The spontaneous comments of a number of *S*s seem to indicate that this process might be related to imagery. One *S*, commenting on the large difference between the number of trials required to learn the first and subsequent lists, stated that the first list took much longer because the sounds were novel and he had to listen to them long enough to 'visualize' them in their absence before he could learn which response went with which sound. Another *S*, with absolute pitch, said that she used pitch to learn the first list.[12] When she heard the second list she realized that pitch was not a basis of similarity; thereupon, she "thought about" the sounds and discovered the basis. Questioning revealed that this meant forming an auditory image which allowed consideration of its various aspects. The rapidity with which these images were formed in her case is remarkable.

The absence of an independent effect of the variable of hierarchical order and its significant interaction with prior experience deserves, perhaps, some special attention. While an attempt to account for the factors operating to produce such results can be only speculative, it does lead to the formulation of a testable hypothesis.

Since the prior experience variable produces significant effects, attention is focused, first, upon the condition in which it did not operate. A comparison of the means of the two sub-groups (*NPE–HL* and *NPE–L*) shows that the group which had to learn new responses to stimuli for which responses had already been learned made more errors. This suggests that these *S*s may be simultaneously extinguishing an old response and learning a new one; the competing responses then interfere with performance. Hence, the interaction may be viewed as an interaction of the facilitating effects of prior experience and the interfering effects of hierarchical order; the results, then, indicate their relative dominance in the learning task.

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[12] This judgment is based upon *S*'s performance in a study by J. F. Corso, Absolute judgments of musical tonality, *J. acoust. Soc. Amer.*, 29, 1957, 138-144.

The prior experience, of course, was designed to effect afferent modification; if it is, indeed, a basic process, it must certainly continue to operate during the learning of responses where it would be obscured ordinarily. Its maximal effect would be expected in the group with the *greatest amount of experience* with the stimuli; the group with the greatest amount of experience, *PE–HL,* did perform better than the other groups. It would seem then that even if the interference-effect were equal or stronger than with group *NPE–HL* at some point during learning, the effect of more experience with the stimuli was sufficient to counterbalance it.

Supporting research for this interpretation is found in the work of Reid.[13] He trained rats to make a black-white discrimination in a Y-maze. The animals were then divided into three groups; one group had no more trials, a second had 50 additional trials, a third had 150 additional trials. They were then run for learning a reversal. The third group, that with the most experience with the stimuli, learned the reversal more rapidly.

Considering the results of this experiment and Reid's, the postulated process of modification may be seen as providing a framework for explaining the reduction in variability of behavior and the flexibility of the organism in learning new responses when old ones become inadequate because of changing conditions in the environment.

SUMMARY

(1) The study presented here extended the study of concept-formation into the area of audition. The essential purposes of the study were: (1) to compare the learning of concepts based upon a prominent partial in a harmonic complex tone with the learning of concepts based upon the harmonic structure of a complex tone; and (2) to determine the effect upon the formation of concepts of both prior experience with stimuli and hierarchical order.

(2) The general procedure was the method of anticipation. A list of

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[13] L. S. Reid, The development of non-continuity behavior through continuity learning, *J. exp. Psychol.,* 46, 1953, 107-112.

six stimuli and six responses was presented as paired-associates; a stimulus was then presented for 3 sec. with a standard time-period of 2 sec. before presentation of the correct response. During the time-interval, *S* attempted to give the correct response. Other lists were learned subsequently, in which the stimuli varied except in a particular dimension. Upon completion of five training lists, a generalized test composed of three lists was presented.

(3) The analysis of the data indicated no significant difference between the learning of concepts based upon a prominent partial in a harmonic complex tone with the learning of concepts based upon the harmonic structure. Since performance was poor in each condition, raw data were inspected. The inspection indicated that *S*s in both conditions were probably responding to a qualitative dimension of the tones which is a function of both the harmonic structure and the frequency-range in which the partials occur. This is supportive of the format theory of tone-quality; it certainly does not support the classical harmonic theory on the basis of which the stimuli were constructed.

(4) The analysis of the data indicated that prior experience with stimuli facilitates concept-formation. The learning of a higher order concept had no independent effect upon learning a lower order concept; this learning did, however, interact significantly with the variable of prior experience. The results support a postulated primitive, basic learning-process which, as a function of the frequency of exposure to stimuli, affects subsequent learning. This process, conceived of as a modification of neural structures, operates independently of any instrumental response and of any observable, specifiable, experimentally controlled reinforcement. It acts to facilitate learning and increases the flexibility of the organism in adapting to the changing demands of the environment which make certain old responses ineffective.

(5) The study indicates, in general, that the concept-formation experiment may be applied to the problems of audition. The design could be of advantage in determining directly the behavioral effects of combining, in various ways, the many variables so minutely explored with respect to discrimination. The ease with which one or another dimension may be conceptualized could conceivably contribute valuable information on the utility of the dimension for signal devices. The design might be used to provide information on what might be termed 'auditory intelligence' which is certainly a significant factor in musical talent. This auditory intelligence is thought of as the relative ability to conceptualize auditory stimuli, their multidimensional aspects, and to learn different responses to increasingly complex and higher order features of sounds and sound patterns.

THE EFFECT OF OCULOMOTOR ADJUSTMENTS ON APPARENT SIZE

By ERIC G. HEINEMANN, Vassar College, ENDEL TULVING, University of Toronto, and JACOB NACHMIAS, Swarthmore College

An object of fixed angular size looks smaller when the eyes are adjusted for near vision than when they are adjusted for far vision. This fact was reported first by Wheatstone, the inventor of the stereoscope.[1] Wheatstone's qualitative observations were confirmed by several later investigators,[2] and quantitative studies of the phenomenon have been made by Adams, Frank, Hermans, and Swenson.[3]

The research reported below deals mainly with two questions to which the previous investigators have not given clear answers. The first is whether the changes in apparent size are related to changes in apparent distance in the manner postulated by several current theories of 'size-constancy.'[4] The second is which of the oculomotor adjustments, or possibly which combination of adjustments, controls the changes in apparent size.

The principal oculomotor adjustments that are made in response to a change in viewing distance are (1) changes in the angle of convergence* of the visual axes, (2) changes in the power of the lens (accommodation), and (3) changes in the diameter of the pupil. Convergence and accommodation are so linked that a change in one produces a change in the other, and a change in either is usually accompanied by a change in the diameter of the pupil.

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* Received for publication September 1, 1957. This research was supported by Grant G-1280 from the National Science Foundation, and was done at the Psychological Laboratories, Harvard University.

[1] Charles Wheatstone, Contributions to the physiology of vision: II. On some remarkable, and hitherto unobserved, phenomena of binocular vision, *Phil. Trans. roy. Soc. (London), Part I,* (142), 1852, 1-18.

[2] C. H. Judd, Some facts of binocular vision, *Psychol. Rev.,* 4, 1897, 374-389; T. G. Hermans, Visual size constancy as a function of convergence, *J. exp. Psychol.,* 21, 1937, 307-324.

[3] O. S. Adams, Stereogram decentration and stere-obase as factors influencing the apparent size of stereoscopic pictures, this JOURNAL, 68, 1955, 54-68; Helene Frank, Ueber den Einfluss inadaquäter Konvergenz und Akkommodation auf die Sehgrösse, *Psychol. Forsch.,* 13, 1930, 135-144; T. G. Hermans, The relationship of convergence and elevation changes to judgments of size, *J. exp. Psychol.,* 48, 1954, 204-208; H. A. Swenson, Reported in H. A. Carr, *An Introduction to Space Perception,* 1935, 362-363.

[4] R. S. Woodworth and Harold Schlosberg, *Experimental Psychology,* 1954, 480-486.

Frank did not attempt to separate the various oculomotor adjustments.[5] She measured the changes in the apparent size of a test-object that occurred when *O*s alternately fixated a mark in the plane of that object and a mark some distance in front of the test-object. A shift of fixation from one mark to the other presumably involved all three of the oculomotor adjustments listed above. Moreover, the change of fixation from one mark to another was accompanied by changes in the retinal image. The retinal image of the test-object presumably was well focused when fixation was in the plane of the test-object, blurred when fixation was on a point in front of the test-object. The size of the blurred image varies somewhat with the diameter of the pupil. Thus it is possible that the size-changes measured in Frank's experiment were caused, at least in part, by changes in the size of the retinal image.[6]

Adams, Hermans, and Swenson all used stereoscopic devices to vary the convergence while the distance of the stimulus-object was held constant.[7] In these experiments blurred vision or double images could be avoided only by a disruption of the normal relationship between convergence and accommodation. The amount of 'play' in the linkage of convergence and accommodation is, however, generally considered to be quite small. The available evidence does not warrant the assumption that changes in convergence were wholly unaccompanied by changes in accommodation. Also, the changes in convergence were almost certainly accompanied by changes in the diameter of the pupil. As in Frank's experiment, one cannot determine whether the changes in apparent size were caused by one of the oculomotor adjustments, several of the oculomotor adjustments, or the optical consequences of these adjustments.

Three related experiments that bear on these problems are described below. In all experiments the *O*s observed under conditions of complete 'reduction,' except insofar as the occurrence of oculomotor adjustments may be held to be incompatible with this concept.

APPARATUS

The apparatus used in all our experiments consisted of an arrangement for presenting in quick succession two stimulus-objects whose size or distance was to be compared by *O*. One of these, the standard stimulus, was an evenly illuminated disk that subtended a constant angle at *O*'s eye and was presented at one of five distances.[8] The other, the variable stimulus, was one of a series of interchangeable disks of different size, presented always at a distance of 400 cm.

The disks were made by cutting circular holes in plates of aluminum. The borders of these holes were ground to knife-edges. Five disks were made for the standard stimulus. Their sizes were so chosen that there was one that subtended an angle of

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[5] Frank, *op. cit.*, 135-144.

[6] For a demonstration of the fact that with inadequate accommodation, changes in the diameter of the pupil may alter the apparent size of an object see K. T. Brown, An optical illusion of spontaneous fluctuations in apparent size, this JOURNAL, 67, 1954, 533-539.

[7] Adams, *op. cit.*, 54-68; Hermans, *op. cit.*, 307-324; Carr, *op. cit.*, 362-363.

[8] Throughout this paper the reference-point for all angles and distances is the nodal point of the eye, assumed to be 7 mm. behind the vertex of the cornea.

1° at each of the following distances: 25, 33.3, 50, 100, and 300 cm. For the variable, a series of plates with holes whose diameter varied in successive steps of 5% was used. A holder permitted *E* to interchange these plates quickly.

The aluminum plates were shown in front of a piece of flashed opal glass which formed the front of a light-tight box containing a small frosted light bulb. The inside of this light-box was painted flat white, and between the frosted bulb and the flashed opal glass was placed a series of milk-glass diffusing screens. With this arrangement, the illumination on the flashed opal screen appeared completely even, and the screen had no visible grain.

In some of our experiments the light-box with its opal glass front was attached directly to the back of the aluminum plate. When this was done, two identical light-boxes were used, one attached to the standard and the other to the variable. In other experiments only a single light-box was used. It was placed about 410 cm. from *O*'s eye, *i.e.* 10 cm. behind the variable. Under these conditions, the variable and standard were seen against the same illuminated surface. Preliminary experiments showed that the location of the opal glass in no way affected the outcome of the experiments.

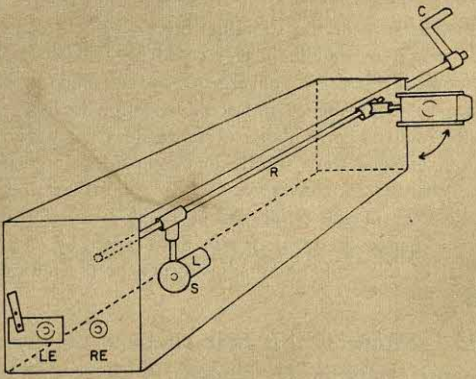

FIG. 1. SCHEMATIC VIEW OF APPARATUS USED IN EXPERIMENTS ON APPARENT SIZE AND DISTANCE

A schematic view of the apparatus is given in Fig. 1. The standard disk (S), to be used at a given time, and the holder for the variable disks were suspended from a long rod (R). This rod was so mounted on bearings that it could be rotated about its longitudinal axis by turning the crank (C). As shown in Fig. 1, the variable and the standard stimulus-objects were mounted in such a fashion that their positions in the XY plane could be interchanged by a 90° turn of the rod.

The entire arrangement was enclosed in a long, light-tight box. The inside of this box was lined with corrugated cardboard that had been painted flat black. The box was further provided with a system of stops and light-traps designed to eliminate all reflected light.

At the front of the box were mounted eye-pieces in which artificial pupils could be inserted. The right eye-piece and all the disks used were centered on a common axis to which they were also normal. The left eye-piece was provided with a sliding mount and a lever that enabled *O* to move the eye-piece quickly in the horizontal direction. When *O* observed with both eyes, rapid adjustment of the distance between the two eye-pieces was required to compensate for the change in the interpupillary distance that goes along with a change in convergence. The distance through which the left eye-piece could be moved in either direction was determined by adjustable stops. For each *O* and for each set of target-distances these stops were pre-set to give the desired interpupillary distances in the binocular experiments.

In several experiments we presented to *O*'s right eye the stimulus-disks and a fixation-point located in the center of these disks, while the left eye was shown only the fixation-point. To produce this stimulus-arrangement each of the holes that formed the disks was covered by a sheet of Polaroid. A small circular hole was cut in each Polaroid to serve as fixation-point. The size of this hole was made proportional to the distance so that all fixation-points had the same angular size. The left eye-piece was covered with a sheet of Polaroid turned 90° with respect to the Polaroid covering the disks, hence only the unpolarized light coming from the fixation-point reached the left eye.

EXPERIMENT I: APPARENT SIZE AND APPARENT DISTANCE IN MONOCULAR OBSERVATION

The first experiment is divided into two parts, one dealing with the relative apparent size of objects presented at different distances, the other with the relative apparent distance of these same objects.

Procedure. *O* observed with his right eye only. His head was lined up in such a way that, with his right eye in approximately the primary position, his visual axis coincided with the common axis of the stimulus-disks and eye-piece. In this way, no movement of the eye was required to cause two disks exposed successively at different distances to be imaged on the same region of the retina. No artificial pupil was used in this experiment.

O was shown either the standard or the variable first, and was instructed to signal *E* as soon as the disk appeared to be sharply focused. When *O* signalled, the exposure of the first disk was terminated and the second disk was presented. Approximately one second elapsed between the end of the first exposure and the beginning of the second. The illumination on the disks was turned on and off in such a way that *O* at no time saw the disks in motion. *O* was instructed to bring the second disk into focus as quickly as possible and then to make his report. In the part of the experiment concerned with apparent size, *O* was required to state whether the second disk was larger or smaller than the first. On successive trials the size of the variable disk was changed in accordance with the up-and-down or staircase method.[9] When enough judgments to compute a point of subjective equality (*PSE*) had been obtained, the procedure was repeated for standard disks at other distances. The order of presentation of the five standards was randomized and for each standard the *PSE* was determined twice, once with the variable disk always exposed first and again with the standard disk always exposed first. It took two experimental sessions lasting about 90 min. each to determine a complete function.

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[9] W. J. Dixson and F. J. Massey, *Introduction to Statistical Analysis*, 1957, 318-327.

The procedure used to investigate the relative apparent distance of the disks was identical with that described except for the following points. (1) *O* reported which of the two successively presented disks, standard or variable, was farther away, instead of reporting on the size of the disks. (2) The size of the variable disk was held constant at 1°. Thus both variable and standard disks subtended the same visual angle in this part of the experiment. (3) Only three standards were used: those at 25, 50, and 300 cm. *O* compared the distance of each of these standards with that of the variable 40 times. During 20 trials the variable was exposed first, during the other 20 trials the standard was exposed first. The judgments were distributed over two experimental sessions.

Twelve men, undergraduate students, served as *O*s in this experiment. None of them had any known ocular defects. Their acuity was measured on the Bausch and Lomb Orthorator. Eleven of them had acuity in the right eye equivalent to Snellen 20/20 or better, the remaining one had acuity equivalent to Snellen 20/25.

Each *O* served in four experimental sessions, two for judgments of size and two for judgments of distance. The size- and distance-conditions were run in the order SDDS for half the *O*s and DSSD for the other half.

Results. The results of the measurements of apparent size are shown in Fig. 2. The diameter of the variable disk that was matched to a standard disk subtending a visual angle of 1° is plotted against the reciprocal of the distance of the standard disk. The light dashed lines represent the results of the individuals *O*s. Each point represents the mean of two *PSE*s determined on two different days. The heavy black line connects the means of the entire group.

The results of matching the sizes of the retinal images would be represented by a horizontal line at $y = 69.8$ mm. in this spot. The negative slope of the mean line indicates an effect of distance on apparent size, in the direction of 'size-constancy': a 1° disk near the eye looks smaller than a 1° disk farther away.

Table I shows the results of the judgments of apparent distance. Each entry is the proportion of correct judgments. For the distances of 25 cm. and 50 cm. some *O*s perform at approximately the level of chance, 0.5, but most of them do considerably worse than chance. In other words, they usually say that the objectively nearer disk is farther away. For the 25-cm. and 50-cm. distances, 16 out of 24 are significant at the 1% level or better by the sign test. But 15 of these 16 are significantly poorer than chance. None of the proportions in the 300 cm. column differs significantly from 0.5.

These results of the judgment of distance are in good agreement with

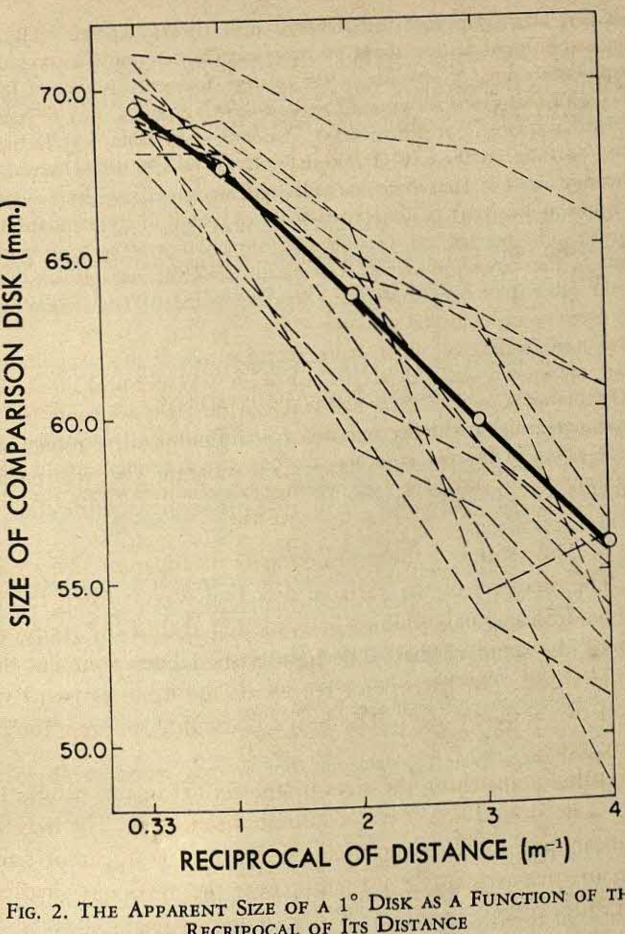

FIG. 2. THE APPARENT SIZE OF A 1° DISK AS A FUNCTION OF THE RECRIPOCAL OF ITS DISTANCE
Monocular observation with the natural pupil.

TABLE I

PROPORTION OF CORRECT JUDGMENTS OF DISTANCE

O	Distance of standard (in cm.)			O	Distance of standard (in cm.)		
	25	50	300		25	50	300
1	0.13	0.05	0.45	7	0.50	0.90	0.55
2	0.05	0.35	0.50	8	0.60	0.68	0.53
3	0.03	0.00	0.45	9	0.63	0.48	0.68
4	0.05	0.05	0.48	10	0.03	0.15	0.53
5	0.03	0.03	0.45	11	0.05	0.18	0.50
6	0.13	0.23	0.43	12	0.58	0.33	0.55

those obtained by Bappert under very similar conditions.[10] The peculiar inadequacy of distance-perception seen here may be due to the fact that the difference in the apparent size of the two disks controls the judgments of distance under these conditions. In any case, since our *O*s did not perceive the relative distances correctly, it is clear that theories that account for size-constancy in terms of processes depending on a correct perception of distance are not applicable to the phenomenon here under study. Since the *O*s looked with only one eye, explanations of the changes in apparent size in terms of binocular disparity and related factors are, of course, also excluded.

EXPERIMENT II: THE EFFECT OF ELIMINATING OCULOMOTOR ADJUSTMENTS

To make certain that the occurrence of oculomotor adjustments is indeed the essential condition for the changes in apparent size we repeated the measurements of apparent size with the following modification in the experimental conditions.

Procedure. An artificial pupil 0.5 mm. in diameter, was placed before *O*'s eye. This pinhole provides the eye with virtually unlimited depth of focus. Under these conditions no accommodative adjustment occurs in response to a change in viewing distance. To compensate for the decrease in the brightness of the disks produced by this small pupil the illumination of the disks was increased until the disks had the same apparent brightness as those used in Experiment I as determined by a successive visual match.

Seven men, undergraduate students, and one woman served as *O*s in this experiment. All had acuity equivalent to Snellen 20/20 or better as measured on the Orthorator.

Results. The results of Experiment II are shown in Fig. 3. The diameter of the variable disk that was matched to the standard disk is plotted against the reciprocal of the distance of the standard disk. The points are data from individual *O*s. The heavy black line represents the theoretical values, *i.e.* the values expected if the *O*s were matching the sizes of the retinal images. These theoretical values were computed for a reduced eye having a dioptric power of + 60 D. The line is not quite horizontal because the pinhole slightly enlarges the retinal images of near objects.[11] The agreement between the theoretical and empirical values is excellent. It is clear that in the absence of oculomotor adjustments variation of the distance of the standard disk has no effect on its apparent size.

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[10] Jakob Bappert, Neue Untersuchungen zum Problem des Verhältnisses von Akkommodation und Konvergenz zur Wahrnehmung der Tiefe, *Z. Psychol.*, 90, 1922, 167-203.

[11] Hermann Helmholtz, *Physiological Optics*, I, 1924, 127-128.

A related control experiment deserves mention here for it shows that the results are not due, perchance, to some other effect of the pinhole such as the alteration of the distribution of scattered light over the retina.[12] In this experiment ophthalmic lenses rather than a pinhole were used to eliminate the need for accommodative adjustments. As in Experiment I, observation was with the natural pupil. For three emmetropic *O*s, *PSE*s were determined between the standard disk at 25 cm. and the variable disk under two conditions. Under Condition 1 the standard disk at 25 cm. was observed with the unaided eye, but the variable disk was observed through a lens of —3.75 D. Under Condition 2 the variable disk was observed with the unaided eye while the standard disk was observed through a lens of +3.75 D. Thus under both conditions the optical distance of the two targets to be compared was the same. It was 25 cm. for Condition 1 and 400 cm. for Condition 2. As with the artificial pupil we found no effect of the actual distance of the disk on its apparent size.

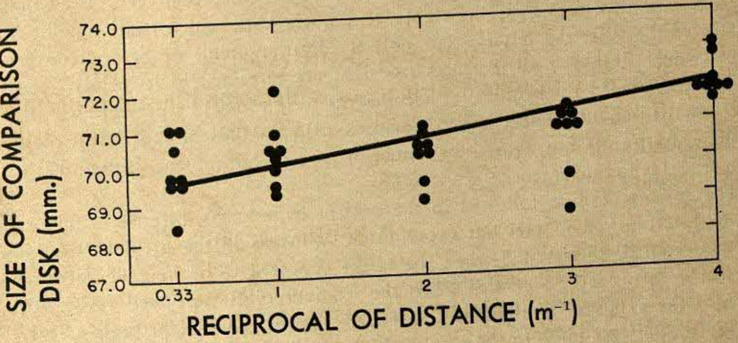

FIG. 3. THE APPARENT SIZE OF A 1° DISK AS A FUNCTION OF THE RECIPROCAL OF ITS DISTANCE
Monocular observation through a pinhole.

EXPERIMENT III: FUSIONAL CONTROL OF CONVERGENCE

In Experiment I all three of the associated oculomotor adjustments occurred together, *i.e.* the adjustment in the power of the lens was accompanied by an adjustment in the diameter of the pupil and in the angle of convergence. The change in convergence that occurred was monocular in the sense that at the completion of the adjustment to a new viewing distance the right eye, which was viewing the disk, was always in its primary position while the orientation of the left (occluded) eye varied with the distance. Experiment III was designed primarily to determine whether changes in accommodation and pupillary diameter are essential for the occurrence of the changes in apparent size.

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[12] This experiment was suggested by Dr. L. A. Riggs of Brown University.

Procedure. Five *O*s, three men and two women, took part in Experiment III. None of them had taken part in Experiment I, but four of them had served in Experiment II. The visual acuity, as determined on the Orthorator, was equivalent to Snellen 20/20 for four of them, 20/30 for the remaining one. Four of the *O*s fell within the age-range of 18 to 26 yr. The remaining *O* was a presbyope, a woman 56 yr. old, who was unable to make any accommodative adjustments under the conditions of the present experiment.

The conditions of observation in Experiment III were the same as those in Experiment II except for the following modifications: (1) A fixation-point was placed in the center of each disk. (2) The left eye, which had been occluded in the previous experiments was also presented with the fixation-point but not the disks. An artificial pupil 0.5 mm. in diameter was placed before each eye. The stimulus-arrangement then was this: the right eye saw the disk and a fixation-point in the center of the disk, the left eye saw only the fixation-point. As before, convergence was asymmetrical. For all distances the right eye remained in its primary position.

In Experiment I the oculomotor adjustments were evoked by presenting a stimulus that required an adjustment in accommodation. In the present experiment the oculomotor adjustments are evoked by imposing a requirement for fusion that serves as a stimulus to convergence.

The basic procedure followed in the present experiment differed in minor ways from that followed in previous experiments. Whereas in the previous experiments *O* was required to signal *E* when the target appeared to be in good focus, in the present experiment he signalled when the fixation-points were well fused. Also, as soon as the exposure of the first disk ended, *O* was required to press a lever that moved the left eye-piece to the new position required to view the second disk.

For each of the four non-presbyopic *O*s, the variation of apparent size with distance was measured under two conditions. In Condition 1 *O*'s eyes were left in their normal state. In Condition 2 adjustments of accommodation and pupillary diameter were prevented by applying homatropine to *O*'s eyes. For obvious reasons, Condition 2 was omitted for the presbyopic *O*. To ascertain that adequate accommodation was occurring under Condition 1 and particularly that no accommodation occurred under Condition 2, measurements of accommodation were made by a method of retinoscopy.

Measurements of accommodation. A discussion of the basic principles of retinoscopy may be found in most textbooks on physiological optics.[13] We shall discuss here only the conditions under which our measurements were made and the manner in which our method differed from the usual clinical practice.

Fig. 4 shows a schematic view of the apparatus used to determine the refractive condition of the right eye. The fixation-point (F) was mounted on an optical bench. Its distance from the *O*'s right eye could be varied from 10 to 400 cm. As indicated, the fixation-point was always presented on a line that coincided with the visual axis of the right eye when that eye was in its primary position. An artificial pupil 0.5 mm. in diameter was placed in front of each eye. Thus the viewing conditions were identical with those under which measurements of apparent size were made.

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[13] See Hugh Davson, *The Physiology of the Eye*, 1949, 402-408.

The artificial pupil for the right eye was made by drilling a small hole in the first-surface mirror (M) that was placed in front of this eye. (L) is a holder for ophthalmic lenses. The retinoscope is a clinical instrument of the spot type. *E* held the retinoscope in his hand but rested its head against a firmly mounted metal bar to keep a constant distance (50 cm.) between it and *O*'s eye.

Light from the retinoscope passed through the ophthalmic lenses, was reflected by the first-surface mirror (M), and then entered *O*'s eye. The light emerging from the eye was reflected from the mirror (M), passed through the lens and then entered the peephole of the retinoscope.

O's head was held steady by means of a biting-board and *O* was instructed to signal *E* if the fixation-points became unfused at any time. Two *E*s worked together to make the measurements. One *E* manipulated the retinoscope and reported on the motion of the reflex, and the second *E* changed lenses in accordance with the method of constant stimuli. With this method changes in accommodation as small as 0.25 D could be reliably detected.

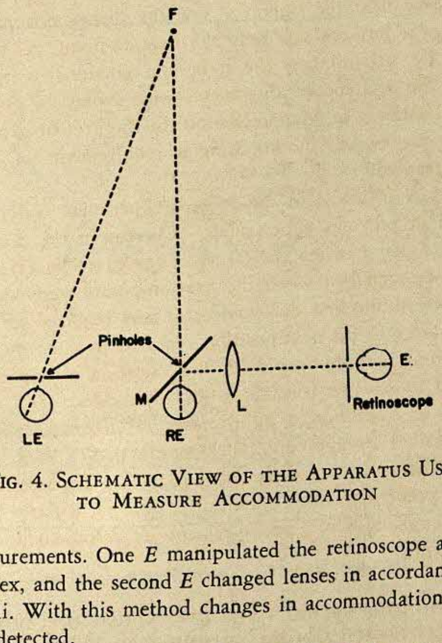

FIG. 4. SCHEMATIC VIEW OF THE APPARATUS USED TO MEASURE ACCOMMODATION

For all *O*s, the refractive condition of the right eye was measured for each of the viewing distances used in the experiment. Before the beginning of Condition 2 of the psychophysical experiment, 3 drops of 5% homatropine were applied to each of *O*'s eyes at 15-min. intervals. These applications were continued until the retinoscopic measurements showed no change in the refractive power of the right eye when convergence was varied over the range of viewing distances used. Immediately following the end of the psychophysical experiment, another set of retinoscopic measurements was made. In all cases the cycloplegia was still complete at the end of the experiment. For the presbyope, no measurable change in accommodation occurred under Condition 1.

A sample pair of functions obtained from one *O* is shown in Fig. 5 The reciprocal of the distance for which the eye is accommodated is plotted against the reciprocal of the distance for which it is converged, *i.e.* the distance of the fixation-point. The solid line represents the results for the eye in its normal condition, the dashed line shows the results after repeated applications of homatropine. Since the existence of 'convergent accommodation' has been questioned occasionally, it may be of interest to note that the accommodative adjustments that occurred were induced by changes in convergence. The pinhole-pupils eliminated any monocular optical stimulus to accommodation. Our findings in this respect are in agreement with those recently published by Fincham.[14]

Results. Fig. 6 shows the results of the psychophysical measurements. As before, the diameter of the variable disk matched to the standard disk is

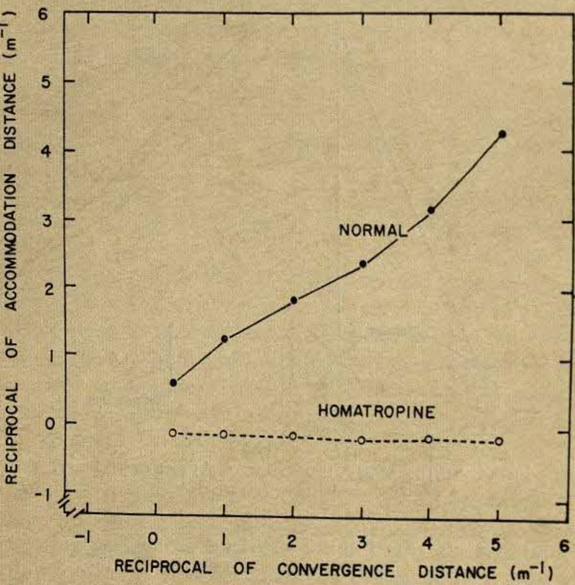

FIG. 5. RELATIONSHIP BETWEEN THE RECIPROCALS OF CONVERGENCE AND OF ACCOMMODATION

One *O* with eye in normal state and under cycloplegia.

plotted against the reciprocal of the distance of the standard disk. The points are based on data from individual *O*s, each point representing the mean of two *PSE*s. The solid black line connects the points representing the group means of the four non-presbyopic *O*s obtained under Condition 1, the dotted line connects the means of the same group obtained under Condition 2. The means of the presbyope are shown separately and are connected by a dashed line. All the functions are virtually identical with the group function obtained in Experiment I; the same tendency toward size-constancy is present.

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[14] E. F. Fincham, The proportion of ciliary muscular force required for accommodation, *J. Physiol.*, 128, 1955, 99-112.

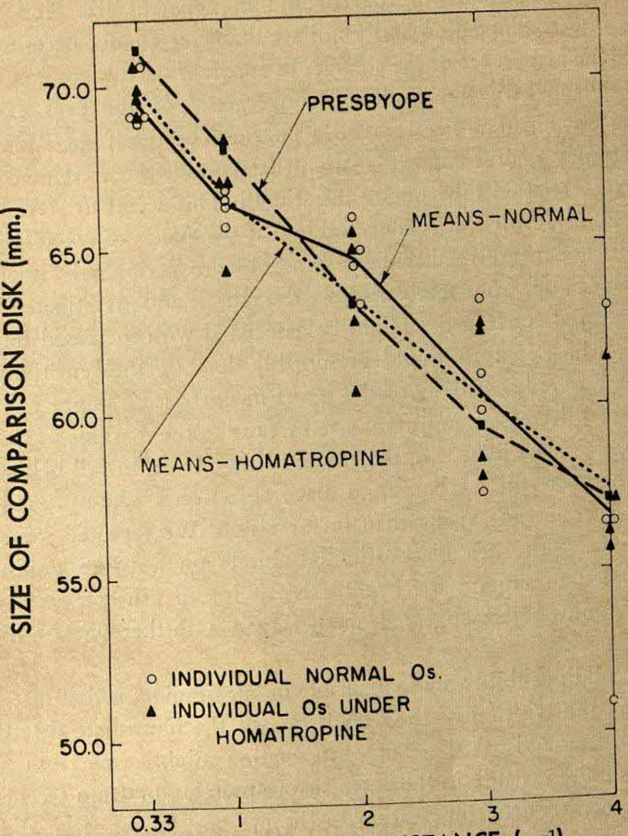

FIG. 6. THE APPARENT SIZE OF A 1° DISK AS A FUNCTION OF THE RECIPROCAL OF ITS DISTANCE WITH AND WITHOUT ACCOMMODATIVE ADJUSTMENT.

DISCUSSION

The results of Experiment III clearly show that changes in the angle of convergence, unaccompanied by changes in accommodation or in the diameter of the pupil, produce variations in the apparent size of objects viewed. The effects obtained as a function of variations in convergence alone are quantitatively identical with those obtained when the power of the lens and the diameter of the pupil are also permitted to vary.

Our experiments do not bear directly on the question of whether changes in accommodation alone are associated with changes in apparent size. What has been shown is (a) that changes in convergence are a sufficient condition for the occurrence of the changes in apparent size, and (b) that changes in accommodation and pupillary diameter are not necessary conditions for the occurrence of the changes in apparent size.

How changes in the angle of convergence of the eyes exert their influence on apparent size remains a puzzle. The results of Experiment I argue strongly against any explanation that assumes a correct perception of the distance relationships among the stimuli. This seems to leave two general possibilities. First, it is conceivable that the effects of convergence here under discussion are mediated by some obscure peripheral visual mechanism. For example, one mechanism we thought of and investigated briefly is this: Increased convergence might be associated with increased intraocular pressure, which in turn would presumably decrease the sensitivity of the retina. A lowered sensitivity of the retina in near vision might be expected to produce a decrease in apparent size by causing the margins of the image, which contain less light than the center of the image, to fall below threshold. If this were the mechanism, a black object on a white ground should appear larger in near vision than in far vision. We repeated some of our observations with such black stimulas-objects on a white ground. The results failed to support the hypothesis; as in our other experiments the nearer stimulus-object appeared smaller than a farther one of the same angular size.

The second possibility, more plausible in our opinion, is that information concerning the position of the eyes is one of the immediate determinants of judgments of visual size. Such information might come from proprioceptive impulses arising in the extrinsic eye-muscles, of from a central system that monitors the innervation to the eye-muscles, or from the visual changes, *e.g.* movements of double images, that normally accompany an adjustment of convergence. While it may be surprising, when considering a mechanism of this sort, that *O*s are unable to report correctly either on their condition of convergence,[15] or on the relative distances of points for which their eyes are converged, the existence of an effect that is confined to visual size violates no logical requirements.

It may be well to point out that the effects found by us are very small when viewed in the framework of constancy of size. The diameter of the variable that was matched to the nearest of the standard disks was about 80% of the diameter of the variable matched to the farthest standard disk. For perfect constancy of size this value would be about 6%.

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[15] S. R. Irvine and E. J. Ludvigh, Is ocular proprioceptive sense concerned in vision?, *A.M.A. Arch. Obhthal.*, 15, 1936, 1037-1049.

It would seem that the effect of oculomotor changes on apparent size plays a very minor role in producing the nearly perfect size-constancy that is found under more normal viewing conditions. This is certainly so when the objects to be compared are shown far from the eyes, as they usually are in such experiments. On the other hand, part of the reason our effects were so small may be that in our experiments, unlike most others, there was no angular separation between the two objects to be compared. Joynson has recently shown that constancy-effects increase with the angular distance between the two objects.[16] Until the influence of this variable, as well as some others (such as the time between presentation of the two objects) is better understood, precise comparisons with the results of other experiments are not possible.

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[16] R. B. Joynson, The problem of size and distance, *Quart. J. exp. Psychol.*, 1, 1949, 119-135.

FIGURAL AFTER-EFFECTS AS A FUNCTION OF THE PERCEIVED CHARACTERISTICS OF THE INSPECTION-FIGURE

By ANNE STORY, USAF Cambridge Research Center

That cognitive set affects perception has been more often asserted than demonstrated. The purpose of the following experiments is to demonstrate that, depending on the perceiver's set, his perceptions do differ, and that such differences become apparent and susceptible to study through their ensuing figural after-effects. The demonstration of an effect of set upon the figural after-effect, moreover, throws into question the adequacy of existing theories of this phenomenon — theories that make no provision for the effects of set.

In past experiments on perception, many secondary factors have been shown to interact with the crucial perceptual variable. In some experiments, what the *S*s remembered cannot be disentangled from what they perceived; in other experiments, they may have responded not to perceptual variables but to social norms and pressures. The following experiments were designed to eliminate the interaction of both memory and social factors with perception. The *S*s responded to a perceptual difference seen at the time they responded; and the perceptual difference to which they responded was socially indifferent. As far as the *S*s were concerned, *E*'s wishes were unknowable.

The experimental technique made use of after-effects. Two well-known theories of figural after-effects (*FAE*s) have been stated in terms of the physiological processes set up as the result of inspecting a figure. One of these theories, the Köhler-Wallach, asserts that inspection of a figure (the *I*-figure) gives rise to a direct contour-current in the cortex.[1] This current, as it flows through the tissues, is said to build up a gradient of resistance (satiation) around the contour. If, during the time that resistance persists, a test-figure (*T*-figure) is presented and if the cortical representation of this figure falls in the previously satiated region, the *T*-figure contour-current will be displaced to paths of lesser resistance than the satiated region. Phenomenal distortions of size, brightness, or distance may, as a consequence, be reported.

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* Received for publication August 31, 1957. This paper is an abstract from a dissertation submitted in partial fulfillment of the requirements for a Ph.D. at the University of California, which was directed by Professor David Krech.

[1] Wolfgang Köhler and Hans Wallach, Figural after-effects: An investigation of visual processes, *Proc. Amer. Phil. Soc.*, 88, 1944, 269-357.

The second theory, that of Osgood and Heyer, is based on postulates advanced by Marshall and Talbot.[2] This theory assumes that the contour of a figure is represented cortically by a statistical distribution or ridge of firing neurons. During fixation, the cells become differentially adapted (depending on their rate of firing and the length of time they are excited). In the time between inspection and the presentation of a *T*-figure, the gradient of distribution flattens, recovery of the cells being most rapid in the center where adaptation was greatest. If, now, the firing-distribution of the *T*-figure contour is algebraically added to the flattened gradient of the previous *I*-figure, the peak of the resultant distribution may be seen to shift away from the peak that would result from the *T*-figure alone. This shift serves to explain the distortions known as figural after-effects, since it is assumed that a phenomenal contour coincides with the peak of excitation in the cortex.

On the basis of both these theories, it would appear that given a specific *I*-figure, either a specific contour-current or a specific distribution of firing in the neurons will occur. Both theories account for the figural after-effects in terms of the pattern of stimulation provided by the *I*-figure. Neither specifically includes the factor of *set* as a determining variable.

EXPERIMENT I

To show that set does operate and that figural after-effects differ according to the way *S* perceives the *I*-figure, it was necessary to choose a figure that might be presented to *S* meaningfully in more than one way. The *I*-figure chosen was an ambiguous B-13 shown in Fig. 4. This figure was presented to one group of *S*s as a 'B' and to another group as a '13.' The *S*s were instructed to fixate the figure for 60 sec. Now if the *S*s' perceptions were actually so altered by their sets that the group informed that they were seeing a 'B' perceived it differently than did the group informed that they were seeing a '13,' it appeared likely that the difference would involve some kind of closure of the figure. That is, the B-group would perceive a unitary closed figure; the 13-group, two figures or numbers.

This perceptual difference should be marked by two different physiological events in the cortex. It was hypothesized that the cortical representation of a closed 'B' would be denser than the cortical representation of an open '13.' Therefore, when the ambiguous figure is fixated and then a *T*-figure is presented within the region of the previous boundaries of the ambiguous B-13, greater figural after-effects should result from inspection of the figure presented as a 'B' than from inspection of the same figure presented as a '13.'

Subjects. The *S*s were 380 students from introductory courses in psychology. They were tested individually.

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[2] C. E. Osgood and A. W. Heyer, A new interpretation of figural after-effects, *Psychol. Rev.*, 59, 1952, 98-118; W. H. Marshall and S. A. Talbot, Recent evidence for neural mechanisms in vision leading to a general theory of sensory acuity, in Heinrich Klüver (ed.), *Visual Mechanisms*, 1942, 117-164.

Materials. The figures were drawn in India ink on white cardboard. Set-figures and *I*-figure were drawn 2 in. square. The fixation-point was a small cross in the center of the card. The distance from this point to the center of the figure was 3.5 in. All cards were 9.5 × 22 in. in over-all size.

A chin-rest and card-stand were constructed of plywood. The chin-rest was set up 3.5 ft. from the card-stand. The background was made homogeneous by tacking up a curved sheet of brown paper approximately 1 ft. from the sides and 1.5 ft. behind the card-holder.

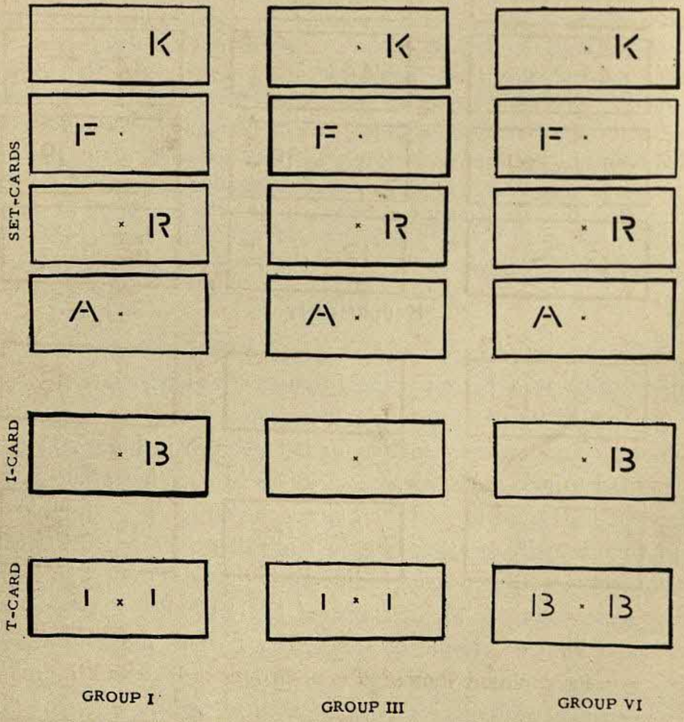

FIG. 1. CARDS SHOWN TO *Ss* OF GROUPS I, III, AND VI
The outside dimensions have been reduced relative to the size of the figures

Procedure. Seven groups of *Ss* were used. Group I was the B-group; Group II, the 13-group. The other five groups were controls. Groups III and IV followed the same procedures, including the instructions to provide a set, as Groups I and II, except that the inspection-figure (*I*-figure) was not shown them. During the 60-sec. inspection-period, the *Ss* in Groups III and IV fixated a cross on an otherwise blank-card. The *Ss* of Group V, on the other hand, saw no set-figures and their *I*-figure consisted of a *closed B* drawn exactly like the ambiguous B-13 but with top and bottom closed. Groups VI and VII followed the same procedures respectively as

Groups I and II except that the *T*-figure for Groups VI and VII was a narrow ambiguous 'B-13' whose contours fell entirely within the location of the contours of the *I*-figure. The procedure followed with each group will be clear from an inspection of Figs. 1, 2, and 3, in which the cards represented at the top are figures intended to create a set, the inspection-card produces the satiation and the test-card measures the figural after-effects.

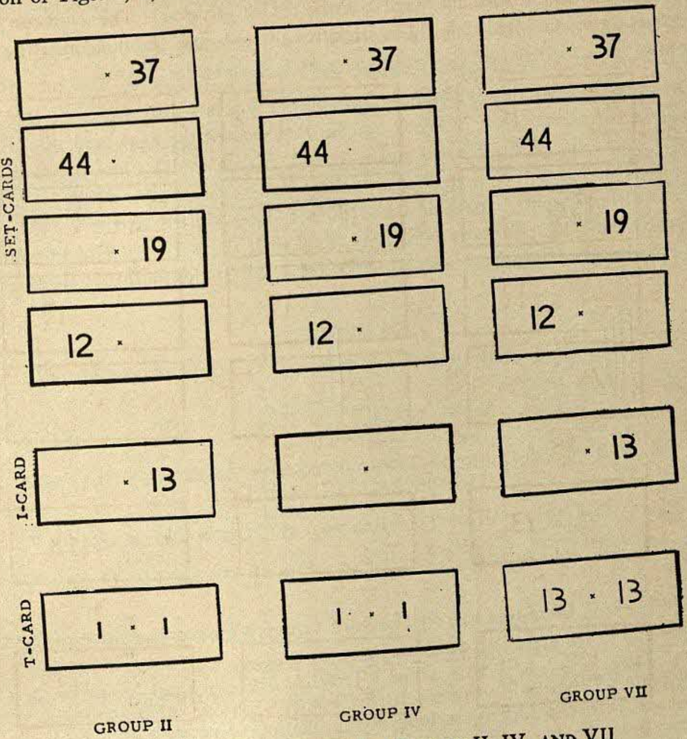

FIG. 2. CARDS SHOWN TO Ss IN GROUPS II, IV, AND VII

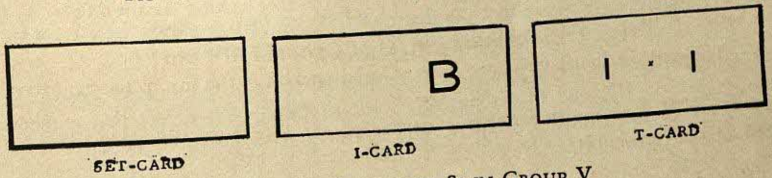

FIG. 3. CARDS SHOWN TO *Ss* IN GROUP V

Instructions. After being seated, *S* was given the following instructions:

All you have to do is look at a cross like this for 60 sec. [*E* points to fixation-point on the 'K'-card and immediately drops the card forward, revealing the 'F'-card] or one like this. [The 'F' card is left visible while *E* continues.] Cover one eye, like this. [*E* puts 3 x 5-card over one of her eyes.] It doesn't matter which eye. You may keep both eyes open and blink whenever you wish, but don't move your head. Keep your chin here. [*E* points to chin-rest.] I will time you and when the 60 sec. are up, I will show you another card. When I change cards, switch eyes like this. [*E* moves the 3 x 5-card over her other eye] and look at the second card with your other eye. I shall ask you what you see on the second card. Do you understand?

As soon as *S* had placed his chin solidly on the chin-rest and held the 3 × 5-card in front of one of his eyes, *E* presented the next card and said:

When you look at this 'x' (the fixation-point) you are still aware of 'R'? [This was shown for about 5 sec. When *S* agreed, *E* presented the next card, the 'A' card, and said:] and the 'A'? [When *S* again agreed, *E* then said:] Now the card I wish you to look at has a 'B' on it. Please look at the fixation-point. Ready? [*E* started the stopwatch, presented the *I*-card and 60 sec. later shifted to the *T*-card, saying at the same time:] Don't move your head, now shift the 3 x 5-in. card to your other eye. [For the *Ss* of Groups I and III, *E* said:] Look at this point. Is there any difference in between these two bars? [The *Ss* of Group VI were given one of the alternate cards and asked a similar question.]

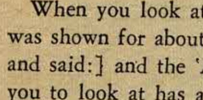

FIG. 4

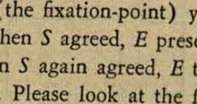

FIG. 5

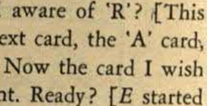

FIG. 6

FIG. 4. THE RELATION OF TEST-BAR TO THE AMBIGUOUS 'B-13' for GROUPS I, II, III, AND IV

FIG. 5. THE RELATION OF TEST-BAR TO THE CLOSED 'B' FOR GROUP V

FIG. 6. THE RELATION OF *T*-FIGURE TO *I*-FIGURE FOR GROUP VI AND VII

[If *S*'s answer was 'no,' E then asked:] Do they differ in size? Is one or the other closer to you? [*S*'s answers were recorded together with any other report he wished to make, and then *E* said:] Now look at the fixation-point with both eyes—do not move your head. Do you see a difference now? [For *Ss* of Groups I and VI, *E* presented the I-card again and asked:] When you were looking at the 'x' on this card, were you always aware that the figure was a 'B'? [*S*'s responses were recorded and he was further encouraged to describe his previous impressions.]

The instructions to the *Ss* of Group II duplicated those of Group I; of Group IV, those of Group III; and of Group VII, those of Group VI, except that numbers (Fig. 2) were substituted for letters. The instructions to the *Ss* of Group V were appropriately simplified.

T-figures. The *T*-figures and their relation to the *I*-figures are shown in Figs. 4, 5, and 6. For purposes of comparison, *I*-figures are drawn in continuous lines and *T*-figures in dashed lines. Actually all were solid black-on-white figures.

Results. In Table I, *Ss* who gave various classes of response on the test are tabulated. *Ss* tallied under *FAE* (figural after-effect) include those who reported that the right hand *T*-figure was smaller, dimmer, or farther away than the left hand *T*-figure, or that the left hand *T*-figure was larger, brighter, darker, more vivid, or closer to *S* than the right hand *T*-figure. Reverse *FAE* refers to reports of the opposite relationships between *T*-figures. Two-way *FAE* refers to contradictory reports in which, for example, one figure is seen as larger than the other, but the latter is seen as daker or closer than the former. Reports of no difference indicate that even after questioning, *E* was unable to elicit a reply from *S* that he perceived any difference in the *T*-figures. The following conclusions may be drawn from these results.

(1) If, as a result of the instructions and the preliminary figures, the ambiguous B-13 is perceived differently, there would be a difference between the reports of the *Ss* of Groups I and II. For Group I the *I*-figure would be expected to close, thus creating a denser pattern than would the same *I*-figure for Group II. It would follow that greater *FAE*s should result for Group I than for Group II. This is, in fact, exactly what happens. Out of 40 *Ss* in Group I, *FAE*s are obtained for 33 *Ss*; out of 40 *Ss* in Group II, *FAE*s are obtained for 10 *Ss*. For these groups, $X^2 = 26.64$ $df. = 1$, $p < 0.001$, and the hypothesis is confirmed.

TABLE I

NUMBER OF SS WHO REPORTED FIGURAL AFTER-EFFECTS OR NO EFFECT

Group	N	Set	*I*-figure	*T*-figure	*FAE*	Two-way *FAE*	No diff.	Reverse *FAE*
I	40	letters	13	bar	33	3	1	3
II	40	numbers	13	bar	10	10	9	11
III	40	letters	blank	bar	7	4	17	12
IV	40	numbers	blank	bar	12	2	13	13
V	40	none	B	bar	29	4	4	3
VI	90	letters	13	13	54	12	13	11
VII	90	numbers	13	13	58	14	8	10

(2) What if set alone were responsible for the obtained difference in after-effects? What if it were immaterial whether the *I*-figure is perceived? If the sets established for *Ss* in Groups I and II are established for the *Ss* in Groups III and IV but if for Groups III and IV no *I*-figure is presented during the 60 sec. inspection-period, then the same difference that was found between Groups I and II would be expected between Groups III and IV. Out of 40 *Ss* in each of these latter groups, 7 in Group III and 12 in Group IV gave evidence of *FAE*s. For both groups, moreover, the whole pattern of responses by categories is remarkably similar. If the frequency of *FAE*s is compared in these two groups, the resulting fourfold table gives $X^2 = 1.73$, $df. = 1$, $p. = 0.19$. No significant difference between these groups can be claimed. There is thus no evidence that the instructions alone account for the difference between Groups I and II.

(3) Were the perceptual characteristics of the *I*-figure really altered by the sets of these *Ss*? If so, results for Group I would be expected to approximate results for Group V, who, without a definitely established set, inspected the actually closed 'B.' On the other hand, results for Group II would be expected to differ from those for Group V. It can readily be seen that this is exactly what occurs. *FAE*s were obtained for 29 *Ss* in Group V as compared to 33 in Group I and 10 in Group II. Furthermore, the whole pattern of Group V responses by categories approximates the pattern of Group I responses but bears no resemblance to Group II responses. For these groups, if all four categories of response are compared:

I vs V $X^2 = 2.20$, $df. = 3$, $p > 0.50$
II vs V $X^2 = 18.32$, $df. = 3$, $p < 0.001$

Thus the expected results were obtained.

(4) Another possible criticism remains: Could the set for numbers itself prevent *FAE*s? Neither Group II nor Group IV showed significant *FAE*s. Suppose that Group II had been presented with a *T*-figure located in the region perceived as "figure" for the previously inspected 13, that is, a *T*-figure simply of narrower form than the *I*-figure. Given such proper *T*-figure, would the ambiguous B-13 when perceived as a 13 produce *FAE*s? If so, Groups VI and VII should not differ. For these groups,

$$X^2 = 0.38, \quad df. = 1, \quad p > 0.50$$

Therefore, the set for numbers itself does not prevent figural after-effects.

The previous calculations are based on all *Ss*' responses while looking at the *T*-card with the eye that had been covered during inspection of the *I*-figure. Results do not differ significantly from those obtained after *E*'s first question. Binocular results also support the original hypothesis.

In every group an equal number of men and women were used. Frequencies are too low to permit a test of significance, but no substantial differences appear.

When *Ss* were asked, near the end of the experiment, whether they had perceived the *I*-figure during the period of satiation as presented, *i.e.* as a 'B' or as a '13,' the following percentages of *Ss* reported the alternative perception as having occurred at some time during satiation: Group I, 82.5; II, 25; VI, 62.2; and VII, 48.9. Thus only one-fourth of the *Ss* in Group II, given the set for numbers, reported perceiving the *I*-figure as 'B,' whereas over four-fifths of Group I, given the set for letters, reported perceiving the *I*-figure as '13.' Results for Groups VI and VII are in the same direction, but the difference between these groups is small. It can be concluded only that the ambiguous *I*-figure was apparently more readily seen as '13' than as 'B.'

From these data it might be supposed that the set for letters or for numbers had no effect. Such a supposition would be entirely unwarranted. *S*s could see the ambiguous figure in two ways—many did; but they *accepted* the figure as presented. More than that, they *insisted* the figure was as presented; for when any *S* reported having a contrary perception, he almost invariably added quickly, "But of course I *knew* it was a B (or a 13) all the time because you said so." The *S*s seemed incapable of doubt. Indeed, to *E* the remarkable quality about their sets was exactly the resulting cognitive certainty and assurance. The *I*-figure, when known differently, influenced the *FAE* differently.

EXPERIMENT II

The specific mechanism by which set operates to affect perception remains to be determined. One possibility might be that a given set occasions specific eye-movements; these eye-movements would then be presumed to determine the perception. In the case of the ambiguous 'B-13,' different eye-movements would be hypothesized for *S*s perceiving a 'B' than for *S*s perceiving a '13.' Two different peripheral patterns of stimulation would result, and thus, on this ground alone different after-effects would ensue.

If it can be shown, however, that set affects kinesthetic as well as visual perception, the eye-movement hypothesis would be somewhat inplausible for the visual case. It is assumed that the kinesthetic perceptions of an ambiguous object would be established through the *same* muscular movements. If the object were then perceived differently, depending on *S*'s set, the peripheral pattern could not be appealed to as the sole determinant of the figural after-effects.

To test this hypothesis, two objects were constructed with identical outside dimensions. One object was a solid wooden block; the other, two thin boards held together by dowels.

The problem was to convince some *S*s that they were handling the block and other *S*s that they were handling the open object. This was achieved by having *S*s handle either the block or the open object for 2 min., during which time they were asked to report any uses they could think of for the object in their hands. Such a procedure forced *S* to become familiar with the object he held. *S*'s eyes were open during the period he held the object, hence he could see as well as feel its solidity or its openness. At the end of 2 min., *S*, with eyes closed, moved his fingers back and forth on the sides of the block for 60 sec.

The kinesthetic after-effect (*KAE*) was determined by testing *S* before he handled either the block or the open object and again after the 60-sec. period. These tests were made by having *S* judge the width of a bar that was narrower than the space between the sides of the open object—the tests being carried out with the *S*'s eyes closed.

It was hypothesized that the *S*s who were set to perceive the *I*-object as the block should show greater kinesthetic after-effects than the *S*s who were set to perceive the *I*-object as open. The block would presumably seem denser than the open object just as the 'B' seemed denser than the '13.' Therefore, even though the *I*-object was the same for all *S*s, a difference in the kinesthetic after-effect was predicted depending on whether they believed the object to be solid or not.

Subjects. The *S*s for Experiment II, 60 students in introductory psychology, were again tested individually.

Materials. The wooden block was 3 × 5.2 × 2.9 in. Outside dimensions of the open object were identical; it consisted of two 5.2 × 2.9-in. sides, each ¼ in. thick, joined by two 2.5-in. dowels. The block was secured to a base during satiation.

A bar and a wedge were used as the test-objects.[3] The bar was 3 in. long and 1.5 in. in diameter; the wedge, 2 in. in length, widened from ¼ in. at its tip to 3.6 in. at its wide end and was calibrated at ½-in. intervals.

Procedure and instructions. *S* was seated in a chair between two stands; the one at his right held the bar and the one at his left, the wedge. When *S*s was comfortably seated, *E* gave him the following instructions:

> I wish you to judge the width of a bar with your fingers—without looking. The bar is here [pointing to the stand at *S*'s right] and the wedge there [pointing to the stand at *S*'s left]. Now touch the sides of the bar with your right thumb and middle finger and the wedge with your left thumb and middle finger. Then move along the wedge until you find the place that feels as wide as the bar. The bar is the same width along its entire length but the wedge widens or narrows as you move toward one or the other end. Is that clear? [*E* answered such questions as *S* raised and continued:] Close your eyes and do not open them until I tell you to do so. [*E* then placed *S*'s hands on the test-objects and said:] You may move your fingers back and forth on the bar, or wedge, or both. When you feel the same thickness with your left hand as you do with your right, announce it and I'll take a reading. I wish four readings.

S's thumb and middle finger were moved to the narrow end of the wedge at the beginning of every trial. Between successive trials the position of the wedge on the stand was slightly varied.

After four trials the bar and wedge were removed and *S* was told to open his eyes. *E* handed him the solid block (if *S* was in Group I) or the open object (if he was in Group II) and said: "Now will you tell me to what uses you think this can be put?" *E* allowed *S* 2 min. for answering. *S* handled the solid or open block with both hands during the time-interval, and *E* then said, "That's very good. Now I would like you to feel the sides of that with your thumb and middle finger with your eyes shut."

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[3] This testing apparatus was patterned after that used by G. S. Klein and David Krech, Cortical conductivity in the brain-injured, *J. Personal.*, 21, 1952, 126.

S closed his eyes. *E* placed the base on stand at *S*'s right and as *S* held out the block (or open object), *E* quickly secured the block to the base for Group I and pretended to secure the open object to the base for Group II. The block was already in place in the case of Group II, and *E* merely fumbled for a few seconds. Thus, for a very few seconds, *S*'s hands were empty.

E then placed *S*'s right thumb and middle finger on the sides of the block (3 in. apart) and pushed *S*'s hand back and forth twice. *E* said, "Feel the sides like this for 60 sec. Go." *E* clicked the stopwatch and timed *S* for 60 sec. During this interval, *E* placed the wedge on the stand at *S*'s left, and then said:

> Now keep your eyes shut and lift your hand [*E* removed base and block] and put it on this [*E* replaced original 1.5-in. bar] and put your other hand here [*E* placed *S*'s left hand on wedge in previous position] and let's get four readings of the width you are feeling now with your right hand.

TABLE II

MEAN SETTING BEFORE AND AFTER SATIATION OF POSITION ALONG WEDGE JUDGED EQUAL TO STANDARD BAR, AND MEAN DIFFERENCE BETWEEN PRE- AND POST-SATIATION MEASURES

	Group I (solid block)			Group II (open object)		
Ss	N	Pre-satiation	Post-satiation	N	Pre-satiation	Post-satiation
Men	15	32.37	27.54	15	29.97	28.42
Women	15	30.29	25.56	15	32.33	29.96
All	30	31.33	26.55	30	31.15	29.19
Mean diff.		−4.78			−1.97	

The earlier procedure on the 1.5-in. bar was then repeated.

Results. For both pre- and post-satiation trials, the mean of four settings was computed for each *S*. These settings had been read directly from the scale on the wedge. *S*'s kinesthetic after-effect is represented by the difference between the mean of his pre- and post-satiation settings.

For Groups I and II the difference is $t = 3.37$, $p < 0.01$, and it is clear that the predicted results were obtained.

Depending on whether *Ss* perceived the *I*-object as solid or as open, significantly different kinesthetic after-effects were obtained. Since, during satiation, perception of the *I*-object was apparently achieved for all *Ss* by the same movements, the difference in the *aggregate* perceptions must have been determined by the *Ss*' previous sets. Thus, for kinesthetic perception, set operates directly on the perception.

It should be noted that *Ss*' sets in Experiment II were established with open eyes. For 2 min., they both felt and saw the object they were later to 'perceive' with their eyes closed during satiation. Indeed, probably most of the *Ss*' knowledge was gained through vision. None of the *Ss* handled a set-object thoroughly but all so turned the set-objects that they gained many and various visual perspectives. Therefore, the cognitive set that affects a perceptual process apparently can be acquired by means of a sense-modality other than that strictly used in the perceptual process itself. In Experiment II, knowledge gained through vision seems to have influenced the kinesthetic after-effect.

CONCLUSION

From the results of this study we concluded that cognitive set is a determining factor in perception and that the theories based on the after-effect are inadequate to account for the influence of set on perception.

THE EFFECTS OF INSTRUCTIONAL 'SETS' ON LEARNING AND TRANSFER

By FRANCIS J. DI VESTA and KATHRYN BLAKE, Syracuse University

In Thorndike and Rock's much quoted study concerning the law of effect the assumption was made that awareness is reflected in a sudden increase in correct responses similar to that found in insightful learning.[1] In their experiment, *S*s made gradual improvement in rewarded responses. Accordingly, the interpretation was made that learning occurred not as a result of the awareness of the principle underlying the reward but as an automatic effect of rewards.

This conclusion has resulted in a series of investigations.[2] The variables of 'set' for speed,[3] motivation,[4] experimenter,[5] and kind of reinforcement[6] have variously been demonstrated as influencing learning in the absence of awareness as well as with the verbalization of the principle involved. Tatz, however, suggests that the level of awareness is related to the number of correct responses made in a typical study of learning without awareness.[7]

Many of the studies, have used different instructional sets. Accordingly, the present study was designed to demonstrate (1) the extent to which a 'set' to 'look for a principle' and verbal reinforcement facilitates learning; and (2) the extent to which searching for a principle transfers to facilitate performance on subsequent problems of a similar nature. The

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* Received for publication September 13, 1957. The assistance of Mr. James Lent in the collection and analysis of the data is gratefully acknowledged.

[1] E. L. Thorndike and R. T. Rock, Jr., Learning without awareness of what is being learned or intent to learn it, *J. exp. Psychol.*, 17, 1934, 1-19.

[2] For a survey of this literature up to 1947, see Leo Postman, The history and present status of the law of effect, *Psychol. Bull.*, 44, 1947, 489-563.

[3] R. L. Weiss, The influence of 'set for speed' on 'learning without awareness,' this JOURNAL, 68, 1955, 425-431.

[4] Leo Postman and P. A. Adams, Performance variables in the experimental analysis of the law of effect, *ibid.*, 67, 1954, 612-631.

[5] B. D. Cohen, H. I. Kalish, J. R. Thurston, and Edwin Cohen, Experimental manipulation of verbal behavior, *J. exp. Psychol.*, 47, 1954, 106-110; Leo Postman and R. F. Jarrett, An experimental analysis of 'learning without awareness,' this JOURNAL, 65, 1952, 244-255.

[6] Joel Greenspoon, The reinforcing effect of two spoken sounds on the frequency of two responses, *ibid.*, 68, 1955, 409-416; F. W. Irwin, K. Kaufman, G. Prior and H. B. Weaver, On 'learning without awareness of what is being learned,' *J. exp. Psychol.*, 17, 1934, 823-827; J. B. Sidowski, Influence of awareness of reinforcement on verbal conditioning, *ibid.*, 48, 1954, 355-360.

[7] Sherman J. Tatz, Symbolic mediation in 'learning without awareness,' paper read at the E.P.A. meeting on March 23, 1956 and provided the present author in a personal communication.

assumption here is that improvement in learning over a series of multiple, though similar, problems is a function of the number of previously learned problems. The 'sets' aroused by instruction are presumed to permit verbalizations which mediate adaptations to the problem in the form of hypotheses. This process is parallel to that of 'learning how to learn.'[8]

METHOD

Materials. Two sets of learning materials were used. On the original learning task (Task I) materials were adapted from the descriptions of those used by Philbrick and Postman in their verbal conditioning study.[9] Two hundred and sixteen different stimulus-words varying in length from 2 to 10 letters were selected from *The Teacher's Word Book.*[10] One word of each length was randomly assigned to 24 different blocks of 9 words each.

On the transfer-task (Task II) 216 stimulus-words representing verbs and adverbs were used, adapted after those used by Postman and Jarrett.[11] These words, too, were selected from *The Teacher's Word Book* and none was beyond the fourth grade level of difficulty. The words were randomly assigned to 24 blocks. To minimize systematic effects of order, the cards on which each word was typed were shuffled anew before every presentation to each *S*. Philbrick and Postman indicate that this task is more difficult than the preceding one.[12]

Experimental groups. Three experimental groups of *Ss* were used. The treatments received by each differed with respect (a) to whether they were informed that a principle was operative, and (b) to whether reinforcement was positive or random.

Instructions. The following instructions were given on Task I to the group (Group *RWP*) given (a) knowledge that some principle was operating, and (b) positive reinforcement.

> We have prepared a group of words and assigned numbers according to a principle or system, to each word. We are interested in seeing how many of these assigned numbers you can determine correctly when you are shown only the word. I will give you a series of cards. On each card there is a word. After you read each word I want you to tell me what number goes with that word. I have used numbers from 1 through 9, so you can choose from 1 through 9 for each word. There are 216 different words and we will go through the list just once. Give your responses as quickly as you can. Be sure to say each word before saying the number. I will say 'right' or 'wrong' after each response and I will record your response on this score sheet.

A second group (Group *RNP*), which was provided a 'set' to 'not look for a principle' but received accurate reinforcement, were given the following instructions.

> Perhaps you have heard of extra-sensory perception. *Ss* are asked to name items which they cannot possibly perceive in any ordinary sense of the word. If they are able to make hits more frequently than they would by chance alone, it

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[8] H. F. Harlow, The formation of learning sets, *Psychol. Rev.*, 56, 1949, 51-65.

[9] E. B. Philbrick and Leo Postman, A further analysis of 'learning without awareness,' this JOURNAL, 68, 1955, 417-424.

[10] E. L. Thorndike and Irving Lorge, *The Teacher's Word Book of 30,000 Words*, 1944, *passim.*

[11] *Op. cit.*, 245.

[12] Philbrick and Postman, *op. cit.*, 418.

> must be by extra-sensory perception. We are dealing with the same problem by a method somewhat different from the ones used before. We have prepared a group of words and assigned numbers to each word. We are interested in seeing how many of these assigned numbers you can guess correctly when you are shown only the words. I will give you a series of cards. On each card there is a word. After you read each word I want you to guess what number goes with that word. I have used numbers from 1 through 9 so you can choose from 1 through 9 for each word. There are 216 different words and we will go through the list just once. If extra-sensory perception exists you should be able to get more numbers right than you would by chance alone. If it does not exist you will merely be guessing the number that goes with each word and you should not make a score better than chance. Give your responses as quickly as you can. Be sure to say each word before saying the number. I will say 'right' or 'wrong' after each response and record your answer on this score sheet.[13]

A third group, (Group *PRR*), was made aware that some principle was operating but were administered reinforcement on a random basis. They were given the same instructions as Group *RWP*. The number of times *E* said 'right' or 'wrong' in each block was determined on the average number of correct responses given by *Ss* in a previously reported experiment.[14] The reinforcements were assigned at random within each block. Unlike the *Ss* in either of the other two groups, the *Ss* in Group *PRR* could not confirm their hypotheses by *E*'s reinforcement.

Task II followed completion of the Task I after a rest-period of 3 min. The *Ss* now were adminstered stimulus-words requiring sequential associations. All groups now received the following instructions.

> I will give you a series of cards. On each card is written a word. After each word you read, I want you to say the word or phrase which comes to your mind. Some of the words or phrases will fall into a category that I have decided to call 'right.' If the word or phrase you say falls into that category, I will say 'right.' If the word or phrase you say does not fall into my 'right' category I will say 'wrong.' I will record your responses on the score sheet. Be sure to say each word before you say the word or phrase. Answer as rapidly as you can.

Principle of reinforcement. On Task I, for those groups who received reinforcement, the response reinforced by *E*'s saying 'right' was a number equal to the number of letters in the stimulus-word *minus* one; all other numbers were designated as incorrect and followed by *E*'s saying 'wrong.' As soon as the *Ss*, in the groups receiving other than randon reinforcement, gave evidence of responding at a level significantly better than by chance (4 correct responses in any one block) they were asked to state the basis on which their responses were being made. If their principle was incorrect, the inquiry was repeated at the end of all succeeding blocks until they gave the correct principle. Once the principle was correctly stated no further inquiries were made but the presentation of the stimulus-cards continued until the criterion of two successive errorless trials was reached. The group receiving random reinforcement was asked for a principle on each trial following the fifth.

On Task II, the sequential, associative response to the stimulus-word, *e.g.* across—*the street* throw—*the ball,* was reinforced by *E's* saying 'right'; any other response was incorrect and was followed by *E* saying 'wrong.' After the second block of 9 words, *S* was asked the basis on which he was selecting responses to the stimulus-words. The inquiry was repeated after each succeeding block until the principle was correctly stated. Following the adequate statement of the principle, *S* continued to respond to the words until two successive errorless blocks were completed.

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[13] These instructions are only slightly varied from those used by Postman and Adams, *op. cit.*, 614.

[14] Philbrick and Postman, *op. cit.*, 421-423.

Scoring. Both responses and principles stated by *S* were recorded by *E*. Protocols were scored by two *E*'s for reliability.

Experimenters. Two graduate students in an advanced course in learning served as *E*.

Subjects. Fifty sophomore students in the introductory psychology course served as *S*s. Participation in one experiment was a requirement of the course. These *S*s 'volunteered' for this experiment without cognizance of the nature of the tasks and they were assigned randomly to each of the three groups. Thirty-six *S*s were in Group *RWP*, 32 in *RNP*, and 32 in Group *PRR*. They were further assigned on a random basis to each of the *E*s.

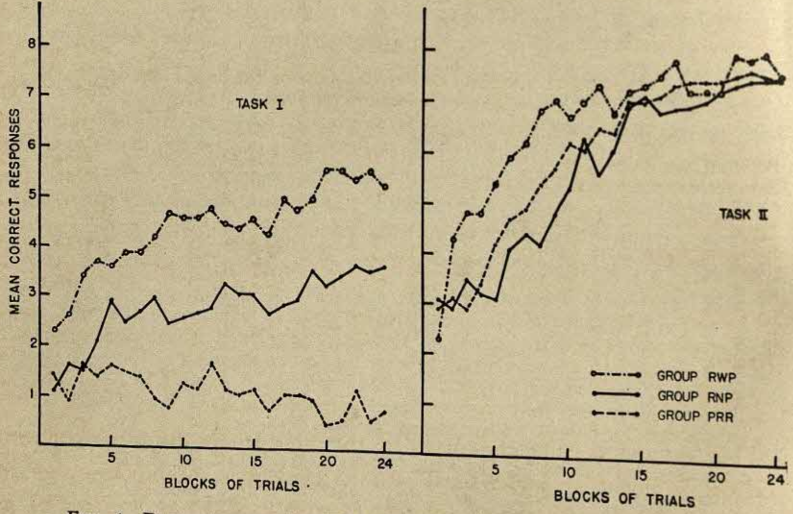

FIG. 1. PERFORMANCE FOR ALL GROUPS ON TASK I AND TASK II

RESULTS

Task I. The first question is: Are the variations in instructional sets reflected in differences in learning rate? It was hypothesized that Group *RWP* would make significantly more correct responses than Group *RNP* and that Group *PRR* would make no improvement.

The performance of each of the three experimental groups on the first learning task are presented in Fig. 1. Included in this figure are the mean number of correct responses for *S*s in Groups *RWP, RNP*, and *PRR* on each of the 24 blocks. As may be seen, the learning curve for the Group *RWP* begins slightly higher than either of the other two groups and remains approximately parallel with the Group *RNP*. As one would expect, Group *PRR*, our 'control' gives no evidence of increase and the curve is flat.

The significance of these trends was tested by analysis of variance using Lindquist's Type III design.[15] To facilitate analysis and achieve equal numbers in each cell, two *S*s were discarded on a random basis from Group *RWP* for this analysis. One *S* for each *E* was so eliminated to achieve an *N* of 16 in each group. The data for the analysis are presented in Table I. This analysis indicates: (a) that the groups do differ significantly on their over-all performance; (b) that there are inter-trial differences among the 24 blocks; and (c) that the curves for the three groups differ in their vertical displacement or degree of slope as is evident in the blocks by groups interaction. Inasmuch as two different *E*s were used it was desirable to test the effects of the *E*s. No differences existed between them in either the main effects or in the interactions.

TABLE I

Analysis of Variance of Performance of the Ss in Each of Three Conditions on Task I

Source	*df.*	MS	*F*
Between conditions (C)	2	891.31	7.34*
Between experimenters (E)	1	178.14	1.47
C×E	2	101.90	<1.00
Error	42	121.35	
Between blocks of trials (B)	23	8.88	4.42*
B×C	46	5.32	2.65*
B×E	23	2.37	1.18
B×C×E	46	1.82	
Error	966	2.01	
Total	1151		

* P<0.01.

*S*s in the three groups who did *not* learn the principle were compared to determine whether the experimental conditions had a differential effect on initial learning. There were 9 *S*s in Group *RWP*, 14 in Group *RNP*, and 16 in Group *PRR* who did not learn the principle after 24 blocks. The performance of these individuals is shown in Fig. 2. There is a decided difference between the performance of the *S*s in each of these groups. The *S*s who had an opportunity to learn show improvement. The curve for the *S*s who were aware that some principle was operating (Group *RWP*) shows more improvement than for those who were not aware of a principle behind the reinforcement (Group *RNP*). Again, both of these curves indicate improvement whereas none was evident in Group *PRR* where it was not possible to learn because of random reinforcement.

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[15] E. F. Lindquist, *Design and Analysis of Experiments*, 1953, 281-284.

The analysis of variance for repeated measurements on these data is presented in Table II.[16] This analysis indicates: (a) that the groups differ significantly in their over-all performance; (b) that there are no significant inter-trial (inter-block) differences; and (c) that the interaction between blocks and groups is significant indicating a difference in the slopes of the three curves. Accordingly, even though the Ss do not have a set to 'look' for the principle, there is an improvement in performance.

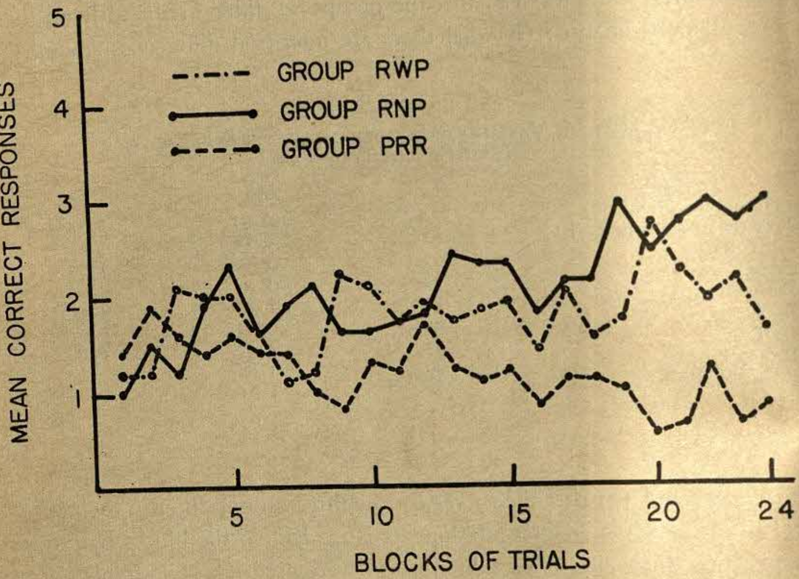

FIG. 2. PERFORMANCE OF UNSUCCESSFUL *S*S IN ALL GROUPS ON TASK I

Task II. A second major question was: Will 'learning how to learn' the principle be reflected in more rapid gains on Task II? It was hypothesized that Group *RWP* would learn the principle more rapidly on the new task than would either of the other two groups.

The second learning task was given to each *S* immediately following the first task, interrupted only by a 3-min. rest-period. The second learning task was exactly the same for all the Ss.

All groups reached a higher level of performance on Task 2 than did

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[16] A. L. Edwards, *Experimental Design in Psychological Research,* 1950, 284-302. This analysis corresponds to Lindquist's Type I design.

Group *RWP* on the first task. The *S*s in all groups started at an initial point higher than that on Task I. There is also a steep increase in the number of correct responses for the first 12 blocks, followed by a slight gradual improvement over the last 12 blocks. In this respect the learning curves are distinctly different from those found on the first learning task where, when learning occurred, the curves showed a gradual improvement over the 24 blocks. The mean number of correct responses for each group on each of the 24 blocks is graphically shown in Fig. 1.

TABLE II

ANALYSIS OF VARIANCE OF PERFORMANCE SCORES OF Ss IN EACH OF THREE CONDITIONS WHO FAILED TO REACH THE CRITERION ON TASK I

Source	*df.*	MS	*F*
Between conditions (C)	2	88.26	4.76†
Between Ss in same group	36	18.54	
Between blocks of trials (B)	23	1.48	1.07
C×B	46	2.68	1.94*
Pooled Ss×B	828	1.38	
Total	935		

† $P<0.05$.
* $P<0.01$.

TABLE III

ANALYSIS OF VARIANCE OF PERFORMANCE OF THE THREE GROUPS OF Ss ON TASK I

Source	*df.*	MS	*F*
Between groups (G)	2	55.84	<1.00
Between experimenters (E)	1	15.82	<1.00
G×E	2	172.51	<1.00
Error	42	194.76	
Between blocks of trials (B)	23	107.06	25.74*
B×G	46	2.82	<1.00
B×E	23	1.28	<1.00
B×G×E	46	1.71	<1.00
Error	966	4.16	
Total	1151		

* $P<0.01$.

An analysis of variance on thees data using the Type III design is given in Table III. In this analysis there are indications (a) that the groups do not differ significantly in their over-all performance; (b) that there are no differences between experimenters or in the interactions involving experimenters; (c) that there is a significant inter-block difference reflecting the improvement made by all *S*s over all the blocks; and (d) that the interaction between blocks and groups is not significant indicating that the slopes of the three curves are parallel.

It will be recalled that the *S*s of Group *RWP* was controlled on Task I;

that they not only were asked to learn the principle but also had opportunity to learn 'how to learn' it. One purpose in the experiment was to determine the extent that such learning would affect performance on Task II. Accordingly, the hypothesis was that Group *RWP* would perform with a significantly greater number of correct responses on Task II than the other two groups—Groups *RNP* and *PRR*. To test this hypothesis

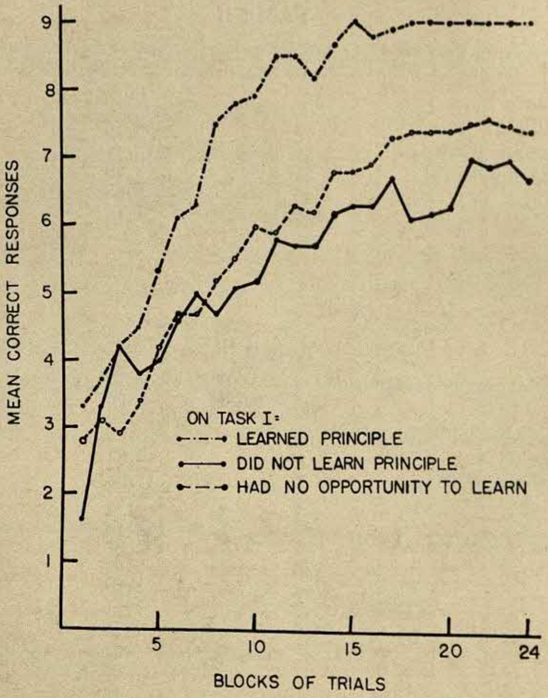

FIG. 3. PERFORMANCE ON TASK II AS A FUNCTION OF PERFORMANCE ON TASK I

the one-tailed Mann-Whitney test was used. The difference in the average scores over the first three trials in this comparison was significant at the 5% level ($z = 1.8$). A similar comparison was made over Trials 4–12 inclusive. The difference for these trials was not significant at the 5% level *via* the one-tailed *t*-test; a *t* of 1.66 was obtained whereas a *t* of 1.68 is required for this level of significance. Performance on the last 12 trials did not differ significantly ($p = > 0.10$) in these comparisons.

The performance of *S*s on Task II was also analyzed according to the success of their performance on Task I. One group consisted of the *S*s who had learned the principle successfully on Task I. There was 11 such

Ss, 9 from Group *RWP* and 2 from Group *RNP*. A second group was composed of those who had opportunity to learn the principle on Task I, but were not successful. There were 23 *Ss*; 9 from Group *RWP* and 14 from Group *RNP*. A third group was composed of those who had no opportunity to learn the principle on Task I, this was the entirety of Group *PRR*. All of the *Ss* who had learned the principle on learning Task I, were successful in learning Task II. The slope of the learning curve increases sharply, then more gradually until maximal performance is reached at the eighteenth block. Neither of the other two groups reached maximal performance. The *Ss* who had no opportunity to learn the principle started at a point lower than those who did not learn the principle but had an opportunity to learn. At the eighth block the former group excelled over the latter and remained superior in performance through the twenty-fourth block. Fig. 3 shows, for the successive blocks, the mean number of correct responses for these three groups.

TABLE IV

ANALYSIS OF VARIANCE OF PERFORMANCE OF Ss ON TASK II GROUPED ACCORDING TO SUCCESS OF PERFORMANCE ON TASK I

Source	df.	*MS*	*F*
Between groups (G)	2	332.59	1.9
Between Ss in same group (Ss)	47	175.71	
Between blocks of trials (B)	23	110.08	36.1*
G×B	46	5.96	1.9*
Ss×B	1081	3.05	
Total	1199		

* $P<0.01$.

The analysis of variance for repeated measurements on the same *S* for these groups are shown in Table IV. The results of this analysis indicates: (a) that the groups do not differ significantly on their over-all performance; (b) that there are significant inter-block differences; and (c) that the curves for the three groups differ in their slopes.

DISCUSSION

The performance of *Ss* on Task I provides evidence for the differential effects on learning of three combinations of (a) verbal reinforcement and (b) knowledge that *a* principle or *some* principle was the basis for the reinforcement. The *Ss* who had knowledge that a principle was being used for reinforcement (Group *RWP*) learned at a more rapid rate than did those who were not so instructed (Group *RNP*). The *Ss* in the latter group were informed that they would be 'guessing' the correct answers.

Even under these conditions, however, learning was still accomplished. The control group (Group *PRR*) under the condition of random reinforcement made no improvement. On the other hand, this group did have opportunity to search for the principle and had practice at stating the principle as had Group *RWP*. Group *PRR*, in fact, stated many principles with a higher degree of variation and complexity than either of the other two groups.

A comparison of the *S*s in each of these groups who did not learn the principle indicates that all but the control group improved in their performance. *S*s from Groups *RNP* and *RWP* made similar improvement. If we use the definition of 'learning without awareness' as being that 'the learner learns without knowing that he has learned and without caring whether he has learned or not,' it is very unlikely that this experiment will provide evidence for or against 'learning without awareness.' Certainly, the *S*s in all of our groups were motivated, at least by instruction, to try to find the correct principle, the correct answers, or to outguess *E*. It is assumed that *S*s in each group were aware of some criterion of performance even though they were unable to verbalize the correct principle or meet the criterion of two blocks of trials without error for adequate performance.

The performance of the *S*s on Task II was intended to determine whether a set to 'learn how to learn' would transfer from Task I to affect performance. The evidence indicates that the group that had a set to search for the principle *and* was reinforced performed significantly better than either of the other two groups, even though its performance on the first block of trials in Task II was initially lower than the other two groups.

In this experiment it was desirable to keep conditions between groups as comparable as possible, varying only instructional 'set.' The *Ss* in Group *RNP*, however, may have inferred that a principle was involved from the question: "On what basis are you responding?," used in the initial learning task, even though this point was not emphasized in the instructions. In future experiments, in which this design is used, it would be advisable to control altogether the implication that a principle might be the basis for a response being correct.

SUMMARY

On the basis of findings in previous investigations of learning without awareness it was hypothesized that instructional 'sets' would be differentially reflected in initial learning and in transfer to facilitate subsequent performance on similar though more complex tasks.

On Task I, *S*s were asked to respond to 24 blocks of 9 words each with a number from 1 to 9. *S*s in one group were instructed that a principle (the number of letters in a word minus one) was the basis for *E*'s saying 'right' or 'wrong' to a response. Ss were not informed of the principle. This group was labeled Group *RWP*. *S*s in a second group were instructed that *E* was testing the success of extra-sensory perception (Group *RNP*). The responses of both groups were accurately reinforced in a positive or negative fashion. A third group was informed that a principle was the basis for *E*'s saying 'right' or 'wrong' to a response, but reinforcement was given on a random basis (Group *PRR*).

Task II was a more difficult one than the first task. The *S*s were required to respond to the stimulus-words with sequential associations. No mention was made of a principle operative in the reinforcement or of extra-sensory perception. All groups received accurate reinforcements.

Analysis of the data indicates that on Task I Group *RWP* made more correct responses than Group *RNP*. The group receiving random reinforcement made no improvement. Unsuccessful *S*s in the first group improved more rapidly than did the unsuccessful *S*s in the second group.

On Task II, the *S*s in Group *RWP* improved more rapidly over the first four trials than those in either of the other two groups. This difference was maintained, although not significantly, through Trial 12 and disappeared in the last 12 trials. The *S*s who performed successfully on Task I were also successful on Task II. Where the curves on Task I showed gradual improvement, the performance on Task II reflected the typical "insight" curve with increasing gains over the first trials and decreasing gains on the last trials.

The findings are interpreted as providing evidence that instructions permit verbalizations which mediate adaptations to the learning task by the formulation of hypotheses. The 'sets' provided are motivational in that they provide a basis for associations necessary to 'learning how to learn.'

ATTITUDE AND DISTANCE-ESTIMATION AS VARIABLES IN SIZE-MATCHING

By NOËL JENKIN and RAY HYMAN, Harvard University

In size-distance judgments, in which physically equal matches are made under conditions of free binocular regard and adequate cues of depth, responses tend closely to approximate the physical size of the objects judged despite changes in their distance.[1] In contrast, matches made in the absence of cues of depth tend to approximate a size that subtends the visual angle of the object judged.[2] A third type of judgment, henceforth to be described as 'analytic,' is one in which *S* is provided with adequate depth-cues but instructed to compare the areas of space projected at a given distance by the two stimulus-objects, *i.e.* to judge equality in terms of the visual angle of the retinal or photographic images. Under the analytic attitude, a match is generally chosen which is intermediate in size between the first (objective, cue available) and the second (cue-reduced) situations.[3] The difference in mean size of the matches under the first and the third situations is attributable to a change in attitude produced by the instructions. Previous researches have been concerned with the degree and statistical significance of this difference and with the size-distance relationship under the two conditions.[4] A question which has remained unanswered is: To what degree, if any, is one type of judgment dependent upon the other?

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[1] A. S. Gilinsky, The effect of attitude upon the perception of size, this JOURNAL, 68, 1955, 173-192; A. H. Hastorf and K. S. Way, Apparent size with and without distance cues, *J. gen. Psychol.*, 47, 1952, 181-188; B. E. Holaday, Die Grössenkonstanz der Sehdinge bei Variation der inneren und äusseren Wahrnehmungsbedingungen, *Arch. f. d. ges. Psychol.*, 88, 1933, 419-486; J. L. Singer, Personal and environmental determinants of perception in a size-constancy experiment, *J. exp. Psychol.*, 43, 1952, 420-427.

[2] E. L. Chalmers, Monocular and binocular cues in the perception of size and distance, this JOURNAL, 65, 1952, 415-423; Hastorf, *op. cit.*, 185-186; A. H. Holway and E. G. Boring, Determinants of apparent visual size with distance variant, this JOURNAL, 54, 1941, 21-37; William Lichten and Susan Lurie, A new technique for the study of perceived size, this JOURNAL, 63, 1950, 280-282.

[3] Gilinsky, *op. cit.*, 179-185; Holaday, *op. cit.*, 419-486; R. B. Joynson, The problem of size and distance, *Quart. J. exp. Psychol.*, 1, 1949, 119-135; Singer, *op. cit.*, 422-427; R. H. Thouless, Phenomenal regression to the real object, *Brit. J. Psychol.*, 21, 1931, 351-355.

[4] Singer, *op. cit.*, 422-427; Gilinsky, *op. cit.*, 173-192.

Thouless, employing the tetrad difference-equation claimed to demonstrate the presence of a general factor among intercorrelations from measures of shape-, size-, and brightness-constancy.[5] This was interpreted as implying that individuals vary in the degree to which they tend to experience "phenomenal regression," to use Thouless' terms. 'Constancy' or 'phenomenal regression' is thus considered as a unitary concept which certain people possess or certain circumstances evoke in greater or lesser degree. Such a view is termed a fallacy by Sheehan, who suggested the presence of methodological errors in Thouless' work.[6] Her own study showed much smaller correlations among different types of 'constancy' than those of Thouless.[7]

From Thouless' work it could reasonably be predicted that the general 'tendency to constancy' would manifest itself in a positive correlation between judgments under objective and analytic conditions. Sheehan's contribution, on the other hand, gives no reason to believe that such a relationship exists. This unsettled issue is important for at least two reasons. First, several experiments using size-distance judgments as a dependent variable have used instructions to *S* which are neither clearly analytic nor clearly objective in character, but which stress a 'phenomenal' response; *i.e. S* is asked to call 'equal' when the stimulus-objects 'look' the same size.[8] Martin and Pickford have shown that when given such instructions some *Ss* may spontaneously adopt an objective attitude (set for the perception of physical size), while others may adopt an analytic attitude (set for the perception of stimulus or angular size).[9] If, therefore, objective and analytic matches are independent of each other, it can be seen that a serious source of error can be introduced into experiments using 'phenomenally-oriented' instructions.

Secondly, a theoretical issue of some importance lies in the question: To what extent does *S* utilize retinal size as a cue in making an objective size-match? Brunswik suggested that *S* used it but little; *i.e.* "distal rather than proximal focussing" took place.[10] Using two *Ss* and a representative sample of familiar objects, he showed that estimates of physical size and of distance were both considerably more accurate than estimates of projected (angular) size.[11] Bolles and Bailey subsequently demonstrated that objective judgments under similar conditions show a constancy of size that is equally as high without mediation from the visual system as it is with use of the visual system.[12] For familiar objects, therefore, sensory information is not necessary to the making of accurate objective judgments. "Distal focussing of perception" can evidently be interpreted as verbally mediated and non-visual categorization of familiar objects.

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[5] Thouless, Individual differences in phenomenal regression, *Brit. J. Psychol.*, 22, 1932, 216-241.

[6] M. R. Sheehan, A study of individual consistency in phenomenal constancy, *Arch. Psychol.*, 31, 1938, 1-95.

[7] *Ibid.*, 20 ff.

[8] For example; H. E. Gruber, The relation of perceived size to perceived distance, this JOURNAL, 67, 1954, 411-426; Edward Lovinger, Perceptual contact with reality in schizophrenia, *J. abnorm. soc. Psychol.*, 52, 1956, 87-91; H. L. Raush, Perceptual constancy in schizophrenia, I. Size constancy, *J. Pers.*, 21, 1952, 176-187; Singer, *op. cit.*, 422-427.

[9] T. M. Martin and R. W. Pickford, The effect of veiling glare on apparent size relations, *Brit. J. Psychol.*, 29, 1938, 92-103.

[10] Egon Brunswik, Distal focussing of perception: Size-constancy in a representative sample of situations, *Psychol. Monogr.*, 56, 1944, (No. 254), 12.

[11] *Ibid.*, 12-28.

[12] R. C. Bolles and D. C. Bailey, Importance of object recognition in size constancy, *J. exp. Psychol.*, 51, 1956, 222-225.

In the case of objects which are unfamiliar or which possess for *S* a low degree of familiarity, one would expect retinal size to function as a cue to physical size. The study of individual differences in size-distance judgment is one method whereby this general problem may be clarified. Granted that an analytic size-match does not usually represent an accurate match of the visual angle it is still possible to reason that the analytic response determines, at least to some degree, the estimate of physical size.[13] Hence *S*s who overestimate the projected size of a standard under analytic attitude would be expected also to overestimate the physical size of the standard under the objective attitude, and contrariwise with *S*s making underestimates.

To obtain evidence on this general problem, the present experiment was designed to employ intercorrelation and factor analysis of individual differences in size-distance judgment under the two attitudes. In order to avoid the possibility of judgments under objective conditions being nonvisual it was decided to use unfamiliar objects.[14] Sampling of individuals, rather than situations, as in Brunswik's experiment, was considered to be a more meaningful approach to the problem.

A supplementary aim was to obtain evidence on the relationship of distance-estimates to size-judgments under the two attitudes. From the hypothesis of size-distance invariance, it was predicted, since visual angle was constant for all *S*s, that individual differences in both types of size-estimates would be reflected also in distance-estimates.[15] Thus individuals underestimating the size of a distant object would also underestimate its distance, and contrariwise for those overestimating size.

METHOD

Apparatus. The apparatus for obtaining measures of size-distance judgment consisted of a movable white equilateral triangle which was raised and lowered through an aperture against a black mounting board. From *S*'s viewing position, the triangle when fully lowered was obscured below the aperture. A pulley and counterweight enabled it to be exposed in graduated degree above the aperture, as indicated by a scale at the rear. *E* sat behind the board, thus screened from *S*'s view, and manipulated the size of the triangle, which at its largest setting measured 27.5 cm. from base to apex. Two white standard-triangles measured respectively 8.5 and 6.5 cm. in height. One at a time, these standard-triangles were presented approximately at *S*'s eye-level and directly to his left, while the movable comparison-object remained directly in front of him at the same height as the standard. Lines projected upon *S* from the two stimuli would thus have made an angle of 90°. Between *S* and the comparison-object a row of chairs was interposed which reached a height well below

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[13] See Footnote 3.

[14] As indicated by Bolles and Bailey, *op. cit.*, 224-225.

[15] F. P. Kilpatrick and W. H. Ittelson, The size-distance invariance hypothesis, *Psychol. Rev.*, 60, 1953, 223-231.

S's eye-level. No objects were interposed between *S* and the standard-triangle. Daylight was excluded and the room was artificially lit for the experiment.

Subjects. The *Ss*, 77 Harvard freshmen, were randomly selected. All participants claimed normal vision or vision approximately corrected to normal by means of eyeglasses. They were remunerated for their services at the usual rates.

Procedure. Each *S* was tested individually in a 1-hr. session. A group of four size-distance judgments was first obtained under conditions established by 'objective' instructions. *S* was told to make the match with reference to the physical size of the stimuli and to give the judgment 'equal' only when he thought the two would measure the same amount if tested by a ruler. The first size-distance judgment was obtained with the comparison-triangle 30 ft. in front of *S* and the 8.5-cm. standard-triangle 15 ft. to his left. One ascending and one descending series were run under these conditions. The standard was then placed at a distance of 2 ft. from the bridge of *S*'s nose when S's head was turned directly to the left, and two more judgments were obtained. *S* and the standard-triangle were then so placed that *S* was 15 ft. in front of the comparison-object and the standard was 12 in. to his left. After *S* had made two more judgments, the standard was moved to a position 15 ft. to his left, thus making the stimuli equidistant from him, and two further judgments were obtained.

A second group of four judgments was then obtained under conditions now established by the 'analytic' instructions. *S* was asked this time to ignore distance and what 'knowledge' he might have of the sizes. His task was to select from the distant comparison the size which would be just covered by the standard-triangle if the latter were suspended in his forward line of vision and at its present distance. This definition of 'retinal' or 'visual angle' equality was further illustrated by referring to the areas of space each triangle would occupy if seen together through an hypothetical window-frame a short distance from *S*. The 6.5-cm. standard was placed in position and four pairs of judgments were obtained at the same distances and in the same order as for objective instructions.

From 60 of the *Ss* it was possible to obtain, following the size-distance judgments, estimates of the distance from them, in feet, of the comparison-object. For the purposes of obtaining this estimate, *S* was returned to his initial position, 30 ft. from the adjustable triangle, which was set at zero. Judgment was thus made of the distance from *S* to the black mounting board, with no cue supplied by any arbitrarily determined setting of the triangle.

RESULTS

A preliminary examination of the data suggested that some of the measures needed to be transformed that their distributions would have shapes closer to normality and to those of the remaining measures. Accordingly, a logarithmic transformation was applied to the following measures: 'objective,' 30 ft./15 ft. and 15 ft./1 ft; 'analytic,' 30 ft./15 ft. and 15 ft./15 ft.

The reliabilities of the constancy-measures were estimated by intercorrelating the parallel sets of scores obtained from the ascending and descending trials. The eight resulting coefficients, adjusted by the Spearman-

Brown prophecy formula, all had values between 0.77 and 0.96.

The transformed scores were intercorrelated by the product-moment method and the resulting coefficients are shown in Table I. Two orthogonal factors were extracted from these correlations by the centroid procedure. The residuals, after the extraction of two factors, were judged to be random error. The factors were rotated to orthogonal simple structure and the resulting loadings are given in Table II.

TABLE I

CORRELATIONS AMONG MEASURES OF SIZE-DISTANCE JUDGMENT

(N=77)

Measures*	O 30/15	O 30/2	O 15/1	O 15/15	A 30/15	A 30/2	A 15/1
O 30/2	.62						
O 15/1	.42	.66					
O 15/15	.23	.34	.58				
A 30/15	.14	.20	.13	.20			
A 30/2	−.02	−.17	−.12	−.12	.63		
A 15/1	−.03	−.14	−.08	−.08	.54	.88	
A 15/15	.24	.26	.19	.20	.10	−.10	−.05

* 'O' signifies that judgments were obtained under objective instructions, and 'A' signifies that they were obtained under analytic instructions.

TABLE II

FACTOR ANALYSIS OF SIZE-DISTANCE JUDGMENTS

Measures	Centroid		Rotated		h^2
	I	II	A	B	
30 ft./15 ft., Objective	.55	.37	.66	.03	.44
30 ft./ 2 ft., Objective	.60	.55	.81	−.08	.66
15 ft./ 1 ft., Objective	.60	.52	.79	−.06	.63
15 ft./15 ft., Objective	.48	.36	.60	.00	.36
30 ft./15 ft., Analytic	.63	−.43	.25	.72	.58
30 ft./ 2 ft., Analytic	.46	−.82	−.12	.93	.88
15 ft./ 1 ft., Analytic	.47	−.77	−.09	.90	.81
15 ft./15 ft., Analytic	.27	.24	.36	−.03	.13

It is clear from the results of this analysis that objective size-distance judgments are independent of analytic size-distance judgments. Factor A is an 'objective' factor with only a small loading (0.25) on one of the analytic measures (30 ft./15 ft.). The other two analytic measures (30 ft./2 ft. and 15 ft./1 ft.) have negligible loadings on Factor A. It is noteworthy that objective judgments at the equidistant position have a substantial loading (0.60) on Factor A and that 'analytic' judgments at the equidistant position have a smaller loading on the same factor. An equidistant judgment under analytic instructions is essentially the same task as under objective instructions, but there was, of course, a difference in the physical size of the standards being judged (objective, 8.5 cm.; analytic, 6.5 cm.). From this evidence it appears that objective size-distance judgments have something in common with size-matches where distance is *not* a variable. Factor B is equally clearly an 'analytic' factor on which the objective and the equidistant measures have virtually zero loadings.

The verbal estimates of the distance from *S* of the comparison-object (from the 30-ft. position) yielded for the 60 *S*s a mean of 30.2 ft. and an *SD* of 7.06 ft. These measures were intercorrelated with the size-judgments at the 30-ft./15-ft. distance, for objective and analytic conditions respectively. The size-distance scores from the three disparate distances were converted to standard scores and then averaged separately for objective and analytic conditions. These combined measures were then intercorrelated with the distance-estimates. Since plots of the size-judgments against distance-estimates showed a departure from linearity of regression, both distributions were split at the respective medians to give fourfold contingency tables. Phi-coefficients were computed from these tables, and signs were added showing the direction of the relationships. These coefficients are given in Table III, together with significance-estimates based on the Chi-square values. These statistics give a better description of the relationship than do product-moment coefficients, the values of which in every instance were somewhat smaller than the Phi-coefficients.

TABLE III

PHI COEFFICIENTS AND SIGNIFICANCE-TESTS BETWEEN SIZE-JUDGMENTS AND DISTANCE-ESTIMATES

(N=60)

	Size-judgments	30 ft. distance-estimates
Objective	30 ft./15 ft.	$-.30$ $P<0.02$
	Mean of 3	$-.27$ $P<0.05$
Analytic	30 ft./15 ft.	$.20$ $P<0.20$
	Mean of 3	$.30$ $P<0.02$

It will be seen that under objective conditions there was a statistically significant relationship between estimated distance and size-matches, in the direction of a distant object's being judged as relatively small and relatively remote, or as relatively large and relatively near. Under analytic conditions, a relationship was also present in the case of the averaged size-judgments, but the direction was opposite to that found for objective conditions. The relationship of analytic size-judgments to estimated distance was toward the distant object being regarded as relatively large and relatively remote, or relatively small and relatively near.

DISCUSSION

The present results failed to find any general 'tendency toward constancy' which manifested both in the objective and analytic size-judging situation. Knowledge of an individual's performance on one kind of task evidently does not enable one to predict what he will do on the other task. One implication to be drawn from these results is the importance of unequivocal instructions in any experiment where size-distance judgment is a dependent variable. If some *S*s in a given experiment assume that their task is an analytic one when *E* intends it to be an objective one, or vice versa, their responses will not be comparable with those of other *S*s who have correctly understood the task. As Joynson points out, an object may 'look' a certain angular size and may also 'look' a certain absolute size.[16] Since the present results have shown that one kind of 'apparent size' is not dependent upon the other kind, 'look'-instructions or any instructions which do not specify the task in terms of physical referents, may lead to mixed attitudes among the *S*s so instructed.[17]

The present results are consistent with Brunswik's concept of "focussing upon the distal object." Assuming that the analytic judgment represented an *S*'s best attempt to respond in terms of proximal stimulation, it would appear that the objective match had no relationship to the perceived magnitude of the (retinal) stimulus. As Woodworth and Schlosberg point out in discussing the geometry of visual size and depth, "visual angle, retinal size or projected size is the one term of the equation that is stimulus-bound, and hence the keystone of the equation."[18] It is surprising, therefore, that the judgment of this projected size was not positively related to the judgment of objective size and the question arises as to what factors in the situation did determine the objective response.

Examination of the data suggests some clues which may lead to solution of this problem. First, the equidistant judgments had positive loadings on the objective factor. This means that in the least ambiguous of the situations presented, where identical proximal sizes would correspond to identical distal sizes, *S*s nevertheless varied in the matches they chose in a manner corresponding to the variation reflected also in their objective size-distance matches. For example, an overestimate at the equidistant position tended to be accompanied by an overestimate at the 30 ft./15 ft. position. This suggests strongly that the objective judgments were more the outcome of *S*'s subjective frame of reference and his assumptions about the absolute size of the object, than the outcome of a stimulus-situation. For example, S_A may have thought the standard to be a larger object (in absolute terms) than did S_B, and this impression manifested itself in larger matches at all distances, even including the equidistant position. The cognitive events involved in this impression of absolute magnitude were so compelling as to overshadow completely the nature of the relevant sensory information. This statement may seem paradoxical, since it can be argued that *S* must first take account of the sensory data before arriving at his cognitive impression of absolute size. Nevertheless, it may be maintained that the results are better accounted for by the view that *S* does not at first respond to areal magnitudes, sensorily given, but instead obtains an immediate total impression of shape, area, brightness, whiteness, texture and distance, which configuration of properties is categorized as 'an equilateral triangle, 3, 4 or 5 in. high.' After the initial categorization, *S* continues at subsequent placements of the standard to judge it in the same terms, varying his response only within narrow limits, despite the wide variations in projected size.

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[16] Joynson, *op. cit.*, 123-124.

[17] See Footnote 8.

[18] R. S. Woodworth and Harold Schlosberg, *Experimental Psychology*, 1954, 484.

The conclusion concerning lack of dependence of objective judgments upon apprehension of the projected size is further reinforced by considering data from the distance-estimates. Under analytic conditions, the relationship between estimated size and estimated distance was in the direction which would be predicted by the size-distance invariance hypothesis. Since visual angle was constant at any given position for any given observer, it would be expected that if S_A judged the distance as shorter than did S_B, the former would judge the size as being smaller than would the latter. Such a tendency was in fact found to be the case, and this result has some interest in and of itself since the size-judgments were obtained by a matching technique but the distance-judgments by the method of verbalization in terms of linear units. What is more remarkable, however, is the fact that the relationship between estimated distance and size-judgments in the objective situation was statistically significant but in the opposite direction to that found for analytic judgments, and in evident contradiction of the hypothesis of size-distance invariance.[19]

While this matter clearly requires further study, a tentative explanation may be offered. In the analytic situation, where attention is directed to the sensory aspects of the situation, the expected relationship between perceived size and distance holds good with regard to individual differences. In the objective situation, however, sensory reality tends to be replaced by opinions or beliefs. One general belief seems to be that there is a relationship between nearness and bigness or farness and smallness. Hence individuals who make a judgment without attending to projective size and who form an impression of relative 'farness' tend also to respond with a size-match which is relatively small. This, of course, implies that the size-distance invariance relationship is contrary to 'common sense,' and indeed, the writers have found that *S*s only with difficulty grasp the notion that if visual angle is constant, a decrease in apparent distance results in a decrease in apparent size.

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[19] Kilpatrick and Ittelson, *op. cit.*, 223-231.

The present finding recalls Gilinsky's experiment in which analytic size-matches decreased and objective size-matches increased as distance became greater.[20] By a different method, the present investigation has again drawn attention to the different properties which objective and analytic size-distance judgments respectively display. It now appears not only that the matches made under these two conditions differ in extent, as a function of physical distance, but also that they are statistically independent of each other. A further possibility is that they are related to estimated distance in different ways. It is concluded that analytic and objective size-distance judgments are the outcome of different and independent processes, and that an explanation for constancy of size of unfamiliar objects is not to be found in the view that size is deduced from a knowledge of distance and an estimate of projective size.

SUMMARY

An experiment was conducted in which 77 freshmen were required to make three different size-distance judgments and one equidistant size-judgment under two instruction-produced attitudes, analytic or retinal, and objective or distal. Factor analysis of the results showed that judgments from the two attitudes were independent of each other. Verbal estimates of distance showed small correlations with the size-matches. The relationship was negative for analytic judgments, as expected from the hypothesis of size-distance invariance, and positive for objective judgments. Suggestions are made concerning the nature of the apparently different and independent processes leading to judgments of the two types.

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[20] Gilinsky, *op. cit.*, 173-192.

LINGUISTIC MANIFESTATIONS OF BILINGUALISM

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Social scientists are beginning to recognize that bilingualism offers a unique opportunity for the study of language-behavior. Second-language acquisition, in contrast to the infant's learning of his first language, permits closer experimental observation, as well as phenomenological analysis by the learner himself, of the development of linguistic skills. In this sense the bilingual condition places one type of language-acquisition in slower motion for the observer. Before social scientists can take full advantage of bilingualism, however, the phenomenon must be systematically defined and its influence on verbal behavior assessed.

Analysts of the acquisition of a second language have used the term 'automatic' to describe the linguistic behavior of those using a language with fluency in contrast to the hesitancy of others beginning the study of a second language.[1] As one develops skill, he becomes less dependent on translation and reacts automatically in the new language. When we use a language automatically, it appears, we do not attend to the language-process itself but to the organization and appropriateness of content.

The chief characteristic of automatic behavior is speed of response, a commonly accepted index of habit-strength. In an earlier study with English-French bilinguals we have made use of this speed-variable as a behavioral measure of bilinguals' comparative 'automaticity' in their two languages.[2] The actual technique involved the use of eight finger-keys, the stems of which were differently colored, an exposure-apparatus which directed *S* to depress a particular key with directions appearing randomly in either of two languages, and a chronoscope measuring speed of response. The direction 'left, red,' for example, is presented and *S*'s speed of response is measured; later in the series, *S* is directed in French to push the same key. After 12 practice-trials, 32 directions are given and *S*'s comparative speed of response in his two languages is determined.

The measuring-technique is a behavioral analogue of linguistic automaticity as described by language-theorists. With respect to reliability, two measures of automaticity were obtained from 16 *S*s on different days and the product-moment correlation of mean difference-scores (French speed minus English speed) was 0.74.[3] The technique distinguishes *S*s whose language-histories suggest probable differences

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[1] H. B. Dunkel, *Second-Language Learning*, 1948, 46 ff.; J. Warshaw, Automatic reactions in practical foreign language work, *Mod. Language J.*, 9, 1924, 151-158.

[2] See W. E. Lambert, Measurement of the linguistic dominance of bilinguals, *J. abnorm. soc. Psychol.*, 50, 1955, 197-200.

[3] From a comparison of the first and last 16 responses of a larger group of *S*s it appears that reliability can be increased by giving much more practice; the last 16 responses are typically more stable and indicate more distinctly the comparative extent of experience with the two languages.

in their comparative linguistic skills and it also correlates significantly with S's comparative associational fluencies in their two languages.[4]

Making use of this measure, bilinguals can be conveniently grouped as *dominants,* those showing a statistically significant difference in speed of response between their two languages, and *balanced,* those showing similar speed of response in both native and second languages. Earlier research focused attention on those linguistic characteristics which differentiate the dominant from the balanced bilinguals, allowing us to describe the changes which occur as one develops skill in the use of a second language.[5] Various developmental stages of skill have been isolated by analyzing the associations *Ss* give to stimulus-words presented in the two languages. These stages of development, which may be considered as successive steps in the acquisition of a second language, have indicated that a vocabulary-skill, as defined by a cluster of tests, is the easiest to develop, while a factor involving acquaintance with the second culture, defined by a different grouping of measures, constitutes the most resistent barrier to full mastery of the second language.

The purpose of the present study is to develop further a series of behavioral measures of bilingualism.

Procedure. The *Ss* used in the present investigation were 43 English-French bilinguals, all college- or university-level students who had not only studied both languages but had had some experience speaking in both. This restriction on speaking ability was introduced to eliminate those extremely dominant in one language whose inclusion would unduly emphasize differences between balanced and dominant *Ss*.

The *Ss* were recruited by asking members of the French Department of McGill University for students who appeared bilingual and by contacting other departments for known bilingual students. These *Ss*, who were paid for their time, willingly gave names of other appropriate *Ss*. Each *S* was tested for a total of 3 to 4 hr., distributed over different days. The directions and procedures were read by each *S* and were summarized orally by *E*. The directions were given in English, but the procedural examples were always repeated in French.

Thirty *Ss* met our criteria for bilingual balance and 13 were dominant in either French or English. For the purpose of the present analysis, the *Ss* were considered as varying in extent of bilingualism along a continuum ranging from dominance in one language through balance to dominance in the other language. For each *S*, the mean reaction-time to English directions was subtracted from the mean time of responding to French directions. The *S* with the largest difference-score fell at one dominant extreme, depending on which language was dominant, and the *S* with the smallest difference-score fell nearer the balance point on the continuum. Pearson product-moment correlations were calculated between the degree of bilingualism and other linguistic measures to be described below. 'Degree of bilingualism' refers to the difference-scores which reflect the direction and the magnitude of the difference between reaction-times to French and English directions, indicating in which language *S* responds more quickly. Thus, the perfectly balanced bilingual would obtain a difference-score of zero, and the person more proficient in French would have a negative score.

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[4] Lambert, *op. cit.,* 199.

[5] Lambert, Developmental aspects of second-language acquisition, *J. soc. Psychol.*, 43, 1956, 83-104.

Results. (1) Speed of word-recognition. In the light of studies relating recognitive thresholds to familiarity of words, our first speculation was that extent of bilingualism should manifest itself in perceptual as well as response-processes.[6] As a test, each *S* viewed tachistoscopically 20 English and 20 French words presented in a shuffled order. Thresholds of recognition were determined by the common procedure of increasing the time of presentation until a word was correctly reported. The words varied in content between languages but were matched for length and frequency of occurrence. The *Ss* were given four practice-words and were encouraged to guess or to spell the word if it was difficult to pronounce. It was our prediction that the closer bilinguals approach balance in the test of speed of responding, the more likely they are to have similar recognitive thresholds for words in their two languages. Average thresholds for each language were determined and difference-scores between-language (French exposure-time minus English exposure-time) computed for all *Ss*. The product-moment correlation between degree of bilingualism and comparative recognitive thresholds is 0.46, significant at beyond the 1% level. It appears, then, that the degree of bilingualism is reflected in the perceptual processes.

(2) *Facility in word-completion.* In an earlier study, a different group of bilinguals were asked to give continuous free associations to French and English stimulus-words with the restriction that associations be made in the same language as the stimulus-word.[7] A high positive correlation, $r = 0.82$, between degree of bilingualism and degree of bilingual fluency was obtained. Since such a high correlation had been found, it seemed advisable to examine further the relation of bilingualism to associational fluency. Word-completion tasks, such as crossword puzzles, call for an ability to make active use of one's vocabulary. We predicted that the closer a bilingual approaches balance, the more nearly equal will be his use of his vocabularies in both languages. As a test, we presented *Ss* with a series of two-letter sequences which are common beginnings for words in both languages, such as 'vi', common to 'ville' or 'victory.' The *Ss* were requested to make as many completions of each stimulus as possible in a 60-sec. period and were encouraged to make their completions in both languages. Differences between the total number of English and French responses were computed for each *S*. As predicted, there was a positive correlation between degree of bilingualism and comparative word-completion facilities; $r = 0.58$, significant at beyond the 1% level.

(3) Facility in word-detection. We speculated further that bilingualism would express itself in the facility of finding embedded English and French words. The *Ss* were instructed to find English or French words in a series of letters, *e.g.* DANSONODEND where *dans, ans, de, en* are meaningful French words, *no, nod, node, ode, end* are meaningful English words, and *an, son, on* could be meaningful in either language depending upon how they are pronounced. In constructing these stimulus-series of letters, attention was given to the commonality of words embedded, their number in each language, and their position in the series. After a practice-example, where the *Ss* were instructed to read from left to right, four test-stimuli

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[6] D. H. Howes and R. L. Solomon, Visual duration threshold as a function of word probability, *J. exp. Psychol.*, 41, 1951, 401-410; K. A. Preston, The speed of word perception and its relation to reading ability, *J. gen. Psychol.*, 13, 1935, 199-203.

[7] Lambert, *op. cit.*, 1955, 83-90.

were presented tachistoscopically for four consecutive 1-sec. intervals. The score assigned each *S* was the percentage of English words detected minus the percentage of French words detected. We predicted that comparative facility in the detection of words would correlate positively with degree of bilingualism. It did, with an $r = 0.42$, significant beyond the 1% level.

(4) Facility in reading. We reasoned that bilingualism should express itself in the comparative speed of reading words in both languages. Whereas word-recognition was measured by the time for a stimulus-word to take on meaning, reading-speed involves the time to make verbalized responses to words above the recognitive threshold. Twenty commonly-occurring English words intermixed with their French equivalents were presented one at a time in a Rantchburg exposure-apparatus. The *Ss* were asked to read each word as quickly as possible. The verbal response was picked up by a sensitive microphone in circuit with a chronoscope. The time taken to start each response was measured in hundredths of a second.[8] As predicted, the comparative facility in reading (French reaction-time minus English reaction-time) was positivley correlated with degree of bilingualism, with $r = 0.46$, significant beyond the 1% level.

(5) Verbal-response set. French-English bilinguals are often confronted with stimulus-words common to both language-systems, such as 'silence,' 'chance,' 'important,' which have quite different pronunciations. An individual dominant in English would be likely to perceive and read these words in English and a French dominant would be likely to read them in French. We presumed that the more closely an individual approaches bilingual balance, the less would he be set to respond in one language rather than the other. Given a series of such stimulus-words, he should respond equally often in both languages. To test for this type of verbal-response set, 14 stimulus-words which are spelled the same in both languages were randomly placed in the longer list of French and English words described in the immediately preceding section. It will be recalled that the *Ss* were instructed to read these words as quickly as possible. For the 14 stimulus-words common to both languages, note was taken of whether a French or an English pronunciation was given to them and difference-scores (the number of English minus the number of French responses) were presumed to indicated in which language, if either, an *S* was set to respond. As predicted, the degree and direction of response-set was positively correlated with the degree of bilingualism; $r = 0.45$, significant beyond the 1% level.

(6) Facility in translation. Bilinguals, by definition, have learned two symbols for each referent and there should be some functional connection between the bilingual's two sets of symbols. We argued that bilingualism should express itself in the facility of switching from one symbol to the other; the more balanced a bilingual, the more similar should be his translation times into French and English. To measure this

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[8] Although the voice-key was constructed for this investigation by an electronics specialist, pretests indicated that the results would depend on the types of words used as well as *S*'s manner of pronunciation. We chose words for both languages which start with explosive consonants and the *Ss* were trained to enunciate clearly, but the results were still not as precise as desired. Nevertheless, we are confident that mean scores for 20 stimulus-words in each language are comparable for the present purpose.

facility in translation, the procedure used for determining skill in reading, as described above, was extended. After the Ss had read the 40 English and French words, the list was presented again and they were asked to translate each word into its other-language equivalent. Speed of translation was measured in terms of the resulting reaction-times. The score assigned each S was the average time for translation from English to French minus the average time for translation from French to English. The correlation between degree of bilingualism and comparative speed of translation is not significantly different from zero, with $r = 0.18$.

Because our prediction was not supported by the data derived from facility in translation, we looked more carefully at the logic underlying the process of translation as well as the data themselves. Not all the Ss showed the same direction of difference in both reading and translating. In fact, 23 Ss showed a greater average speed of reading in one of their languages and also a greater average speed of translation into that language, but 20 Ss with a greater average speed of reading in one language were faster translating into their *other* language. The obvious assumption is that one would be faster translating into his dominant language (or, if balanced, his native language) since response in the dominant language is the more automatic, and since the response takes more time than perceiving. It appears, however, that a good proportion of bilinguals are more skilled in translating into the acquired language.

We can only speculate about the reasons for this phenomenon until it is systematically studied. In brief, our speculation is that an individual may be passive or active in the acquisition of a second language. If passive, he may learn to translate into his dominant language and thereby transform unknown symbols, when encountered, into known ones. An active learner may attempt to reduce the number of unknown symbols and learn to translate into the acquired or less-dominant language. The passive learner, dominant in English, would look up the word 'chien' when he encounters it, while the hypothetical active learner would ask himself how the French say or write 'dog,' and look it up. The former would use a French to English and the latter an English to French dictionary. Our guess is that differences in attitudinal and motivational variables underlie the two types of orientation. The data on hand, of course, were not collected with this idea in mind, but we are encouraged to note that the five Ss who show a greater mean speed of reading in French and who have a greater mean speed of translation into English are all French natives who have come to an English university for training. For these Ss, acquiring English in an active manner is of clear instrumental value.

In conclusion, it appears that bilingualism has a pervasive influence throughout various aspects of verbal behavior. It is exhibited in thresholds of word-recognition, facility for word-completion and word-detection, response-sets, and speed of reading, but not in speed of translation. The closer an individual approaches bilingual balance, the more he will be able to perceive and read words in both languages with similar speeds, to associate in both languages with similar fluency, to make active use of his vocabularies in both languages, and to be set to verbalize in both languages. If he is dominant in French, let us say, on one of these measures, then he will be dominant in French on all of them.

When consideration is given to the potential usefulness of these findings for measuring bilingualism, the question of the independence of the various measures arises. Table I gives the intercorrelations of all seven measures.

The range of correlations is generally small, except for the measures involving translation, and an examination of the correlation-matrix indicates that a single factor is sufficient to describe all associations. Factor loadings, computed by the centroid method, are presented at the right of Table I. Although a second factor was extracted, it is doubtful that this factor is amenable to any meaningful interpretation. It seems appropriate to conclude that the seven measures developed here are common tests of the relative strength of English and French in this group of bilinguals. The factor loadings indicate that word-completion and response-set are direct measures and easily administered indices of bilingualism which are convenient substitutes for measurement by automaticity.

TABLE I

Correlation-Matrix of Bilingual Measures

(All correlation are based on an N of 43.)

Measures	Measures						Centroid factor loadings	
	2	3	4	5	6	7	I	II
(1) Automaticity	.46*	.45	.58	.46	.18	.42	.68	.04
(2) Recognition-Speed		.40	.58	.45	.12	.38	.65	.09
(3) Response-Set			.68	.68	.06	.46	.75	.22
(4) Word-Completion				.53	.35	.65	.88	−.22
(5) Reading-Speed					.02	.31	.68	.44
(6) Translation-Speed						.15	.27	−.37
(7) Word-Detection							.66	−.26

* With 40 *df*., an *r* of 0.30 is significant at the 5% level of confidence and an *r* of 0.39 is significant at the 1% level.

SUMMARY

English-French bilinguals were given a series of tests to determine the degree of correlation among various aspects of their linguistic behavior. It was found that their relative superiority in French or English is manifested in comparative thresholds for word-recognition, facility in word-completion and word-detection, response-sets, and speed of reading, but not in speed of translation. Intercorrelations indicate that a single dimension underlies these various measures, suggesting that bilingualism is reflected in many aspects of linguistic behavior.

COMPLEXITY OF RESPONSE AND TIME OF INITIATING RESPONSES IN RELATION TO AGE

By STEPHEN GRIEW, University of Bristol, England

Despite the considerable amount of research in the field of aging, which has centered around the study of reaction-time, little or no attention appears to have been paid to the possible existence of a relationship between age and complexity of response and time of initiating the response.[1] This may be because of the scarcity of evidence supporting the view that reaction-time is influenced by a complexity of conditions. The findings most frequently quoted suggest that there is no such influence, but they are based, in the main, upon experiments with young *Ss*—those in their 'teens or twenties.[2] If a relationship of this sort is demonstrated in an older population, it may prove possible to interpret more easily some of the age-changes in reaction-time which are at present difficult to account for in terms of the complexity of the responses involved. The information may also throw light upon the problem of determining which aspects of central organization deteriorate with age.

This paper concerns two experiments in which the effect of complexity of response on the time of initiating the responses was studied, and in which a special point was made to determine age-changes in performance.

Apparatus. The apparatus was designed to allow manipulation in a simple perceptual-motor task of three independent variables: (1) level of stimulus-choice; (2) level of response-complexity; and (3) task-continuity. The general layout of the apparatus is shown in Fig. 1. It consisted of a display of eight signal-lights, placed at equal distances on the circumference of a semicircle of 5-in. radius. Upon the appearance of one of these signal-lights, *S* was required to move a stylus, which normally he held in contact with a brass disk at the center of a similar semicircular array of eight brass targets, to the appropriate target and back again as quickly as possible. The central disk and the targets were 1¼ in. in diameter, and the radius of the array of the targets was 8 in.

The level of stimulus-choice was varied by presenting series of 10 signals drawn randomly from 2, 4, and 8 lights, respectively. When less than the total number of lights were in use, the others were covered, and the only targets exposed to view were those which corresponded with the exposed signal-lights. The three levels of choice involved were one of two (½), one of four (¼), and one of eight (⅛).[3]

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* Received for publication September 30, 1957. The author is indebted to the Nuffield Foundation, London, England, for financial support of the experiments reported here.

[1] Wayne Dennis, Age and behavior: A survey of the literature, Randolph Field, Texas: *USAF Sch. Aviation Med.*, 1953; A. T. Welford, Psychological aspects of ageing, in W. Hobson (ed.) *Modern Trends in Geriatrics*, 1956.

[2] J. S. Brown and H. T. Slatter-Hammel, Discrete movements in the horizontal plane as a function of their length and direction, *J. exp. Psychol.*, *39*, 1949, 84-95; L. V. Searle and F. V. Taylor, Studies of tracking behavior: I. Rate and time characteristics of simple corrective movements, *ibid.*, 38, 1948, 615-631.

[3] If the signal-lights are numbered 1 to 8 from left to right, as seen by *S*, Numbers 4 and 5 are exposed at choice-level one-half, 3 to 6 at one-quarter, and 1 to 8 at

Complexity of response was varied by presenting *S* with two types of target. Each target consisted of an upper removable disk, a second disk with a 3/16-in. hole in its center, and, beneath this, a small lower disk. Fig. 2 shows a cross-sectional view of a target.

When the upper disk was in position, *S* was required to touch it with the point of the stylus before returning to the central position. This was the first level (L^1) of complexity of response studied. It will hereafter be referred to as L_1. For the second level (L_2), the upper disk was removed and the one with a hole was exposed. In this case, *S* was required to touch the disk, slide the point of the stylus into the hole, and hit the lower disk before returning to the central position. The lower part of Fig. 2 indicates the paths of the point of the stylus in making responses according to L_1 and L_2. It is clear that both types of response begin with an outward movement, and that the precise direction of this movement depends upon the position of the signal. To this extent, the beginning of each response depends upon successful interpretation of the signal. The two types of response vary in what has to be done upon arrival at target.

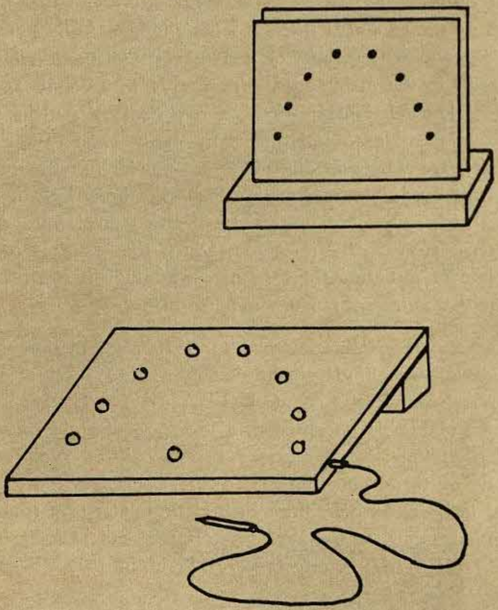

FIG. 1. GENERAL LAYOUT OF THE APPARATUS

The experiments also concerned two levels of task-continuity. In both, the three levels of stimulus-choice and the two levels of complexity of response were combined to give six experimental conditions. At each condition, *S* responded to a series of 10 signals. In Experiment I, signals were presented discontinuously, at intervals of

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one-eighth. A preliminary experiment indicated that results obtained when employing different combinations did not differ significantly from those obtained using the combinations given above.

about 6 sec. (About 2 sec. before each signal, an auditory warning was given.) Experiment II was concerned with continuous performance. In this part of the study, signals occurred immediately upon *S*'s return to the central position, *i.e.* upon the completion of the previous response. In both experiments, signal-lights were extinguished as soon as responses commenced, that is when the stylus broke contact with the central disk at the start of the initial outward movement. The dependent variable, the time elapsing between the appearance of a signal and the commencement of the response, was recorded electromagnetically. This interval is referred to as the time of initiating the response (*TIR*).

In each experiment, 12 *Ss* (20–26 yr. of age) and 12 *Ss* (50–57 yr. of age) took part. They were randomly drawn from a large population of industrial workers of roughly similar occupations and background. All were right-handed. Prior to the experiments, the *Ss* were given, for practice, two series of trials under each experimental condition. Practice was separated from the experimental trials by a rest of about 10 min. The six experimental conditions involved in each experiment were attempted by different *Ss* in different orders, given by the rows of a 6 × 6 Latin Square, and replicated twice in each age-group.

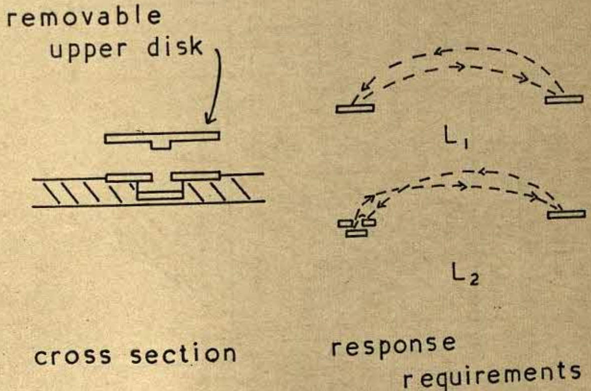

Fig. 2. Cross-Sectional View of Target, and *S*'s Responses in L_1 and L_2

Results. In computing mean *TIRs*, the first two trials in each series were disregarded as a preliminary experiment had indicated that they were invariably accompanied by significantly longer *TIRs*. For the eight remaining trials, there was no significant 'order-effect.' Very few errors occurred as only about 2% of the responses of the *Ss* in both age-groups were incorrect. Where errors occurred, the associated *TIRs* were disregarded.

The results of the two experiments are shown graphically in Fig. 3. Each point on the graphs is the mean of some 96 separate *TIRs*. Summaries of analyses of variance performed on the mean *TIRs* of the individual *Ss* are given in Tables I and II. As would be expected, the effect of level of stimulus-choice is highly significant in all cases. The effect of complexity of response is significant in both discontinuous and continuous performance with the older *Ss*, but in neither case with the younger *Ss*. The significant effects of complexity of response with the older *Ss* reflect significant differences between mean *TIR*s at L_1 and L_2 at all levels of choice, with the exception of the difference between the mean *TIR*s at choice-level one of four in discontinuous performance. Here, however, the difference is very nearly significant ($t = 1.98$, $df. = 22$).

Discussion. The non-significant effects of complexity of response in younger *S*s are in line with previous findings. The significant effects of complexity of response in older *S*s appear to reflect a relationship between complexity and latency of response which has not been reported previously. The results suggest that a certain amount of detailed preparation for response must be taking place during the period of initiation in older *S*s, over and above the interpretation of signal upon which the initial outward movement depends. There is an indication, in fact, that the manipulative part of the response is being prepared during the period of initiation. As this manipulative part of the response is contingent upon the initial outward movement, it seems likely that its preparation, during the initiation of response, must be taking place after the interpretation of the signal.

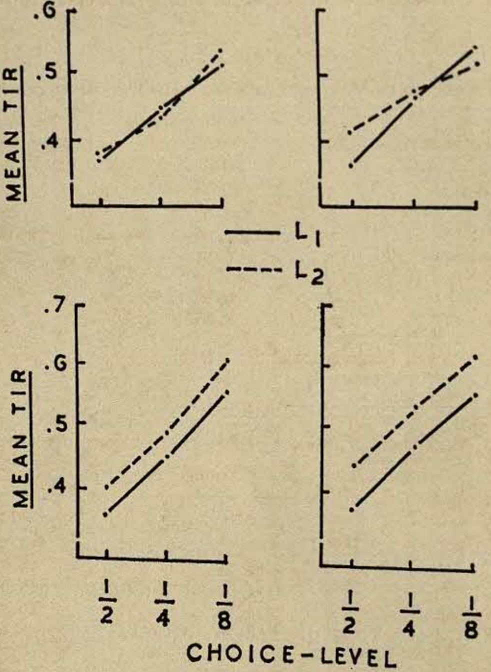

FIG. 3. MEAN TIME OF INITIATING RESPONSES (*TIR*)
Upper left: younger *S*s, discontinuous task
Upper right: younger *S*s, continuous task
Lower left: older *S*s, discontinuous task
Lower right: older *S*s, continuous task.

The fact that this seems not to be occurring in the performance of younger *Ss* suggests first, that, in their case, interpretation of signal and preparation for response occur simultaneously during the period of initiation. In view of the ultimate contingency of manipulation upon signal intrepretation, however, it seems unlikely that manipulation could be prepared until the interpretation of signal is completed.

A more likely explanation of the younger *Ss'* results is that the manipulative part of the response is prepared during the course of the outward movement to target, and that there is a breakdown with age in the ability to prepare for this manipulation while monitoring the outward movement. This would account for the apparent preparation for response by older *Ss* during the period of initiation and is in line with a current view that, with older *Ss*, the central processes responsible for monitoring movement become increasingly important limitations upon performance.[4] It is hoped that further information bearing upon this interpretation will be obtained from an experiment involving the examination of high-frequency chronocyclegraphic records of patterns of movement during performances of the type involved in this study.

TABLE I

SUMMARY OF ANALYSES OF VARIANCE OF RESULTS OF DISCONTINUOUS PERFORMANCES

Source of variance	df.	Younger Ss		Older Ss	
		MS	VR	MS	VR
Order of presentation	5	58.57	.40	31.15	1.22
Error among Ss	6	144.67		25.57	
Level of choice (C)	2	1381.96	154.24†	2618.39	190.57†
Complexity of response (R)	1	6.72	.75	308.32	22.44†
Interaction (C×R)	2	61.10	6.81*	6.06	.44
Trials	5	6.80	.76	1.55	.11
Error within Ss	50	8.96		13.74	

* Significant at 1% level; † Significant at 0.1% level.

TABLE II

SUMMARY OF ANALYSES OF VARIANCE OF RESULTS OF CONTINUOUS PERFORMANCES

Source of variance	df.	Younger Ss		Older Ss	
		MS	VR	MS	VR
Order of presentation	5	60.70	.97	30.16	.91
Error among Ss	6	62.73		33.03	
Level of choice (C)	2	1184.61	80.81†	1840.04	185.30†
Complexity of response (R)	1	34.70	2.37	729.74	73.49†
Interaction (C×R)	2	88.05	6.01*	2.16	.22
Trials	5	15.17	1.03	6.79	.68
Error within Ss	50	14.66		9.93	

* Significant at 1% level; † Significant at 0.1% level.

Summary. Two experiments are reported in which the relationship between complexity of response and initiation of response is studied in a simple perceptual-motor task involving an element of choice. In both continuous and discontinuous performance the effect of increased complexity of response on the performance of older *Ss* is to increase significantly the period of initiation at all levels of choice. This increased latency of response does not occur in younger *Ss*. These results seem to indicate that the manipulative part of the response is prepared by older *Ss* during the period of initiation. This may be due to a breakdown with age in the ability to monitor one movement while preparing for another.

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[4] Welford, *op. cit.*, 72.

MANIFEST ANXIETY-LEVEL, VERBAL RESPONSE-STRENGTH, AND PAIRED-ASSOCIATE LEARNING

By ALEXANDER M. BUCHWALD, Indiana University

This article deals with relationships among scores on the Manifest Anxiety Scale (*MAS*), verbal response-tendencies, and paired-associate learning.[1] These relationships are important in connection with the hypothesis that *S*'s score on the *MAS* reflects his level of drive. This hypothesis, combined with the postulates of Hull's theory, has been used to generate predictions concerning the relative goodness of performance of highly anxious and less anxious *Ss*, where the terms 'highly anxious' and 'less anxious' refer to *Ss* who make high and low scores, respectively, on the *MAS*. On this basis it has been predicted that highly anxious *Ss* will perform better than less anxious *Ss* when correct responses have greater habit-strengths than incorrect responses, and more poorly than less anxious *Ss* when incorrect responses have greater habit-strengths than correct responses.[2]

The first of these two propositions has recently been subjected to empirical testing in two experiments in which *Ss* learned paired-associates consisting of pairs of synonymous adjectives.[3] In both studies anxious *Ss* performed better than less anxious *Ss* on these lists. It is not, however, entirely clear that these experiments adequately test the hypothesis. For one thing, it seems somewhat dubious that the use of synonymous adjectives guarantees greater habit-strengths for correct responses than for *all* other responses. This is particularly so because in word-association data antonyms are frequently the most commonly given responses to adjectives.[4]

In addition to the question concerning the habit-strengths of correct responses, there is another source of difficulty which is of greater importance. There is no evidence to indicate that the correct responses had equal probabilities of occurrence for the two groups at the start of learning. That is, it is possible that the obtained differences reflect greater initial habit-strengths, for correct responses, or smaller habit-strengths for competing responses, for the anxious *Ss* rather than differences in drive-level during the course of learning. This possibility is further suggested by the fact that on a list of word-pairs on which highly anxious *Ss* performed better than less anxious *Ss* throughout the course of learning, the greatest difference in the mean

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* Received for publication November 13, 1957.

[1] J. A. Taylor, A personality scale of manifest anxiety, *J. abnorm. soc. Psychol.*, 48, 1953, 285-290.

[2] K. W. Spence, I. E. Farber, and H. H. McFann, The relation of anxiety (drive) level to performance in competitional and noncompetitional paired-associate learning, *J. exp. Psychol.*, 52, 1956, 296-305; Taylor, Drive theory and manifest anxiety, *Psychol. Bull.*, 53, 1956, 303-320.

[3] Spence, Farber, and McFann, *op. cit.*, 296-305; K. W. Spence, Taylor, and Rhoda Ketchel, Anxiety (drive) level and degree of competition in paired-associates learning, *J. exp. Psychol.*, 52, 1956, 306-310.

[4] W. A. Russell, and J. J. Jenkins, *The Complete Minnesota Norms for Responses to 100 Words from the Kent-Rosanoff Word-Association Test*, *Technical Report* No. 11, 1954.

proportion of correct responses between the groups occurs on the first trial for which data are shown.[5]

The present paper reports two experiments. Experiment I is a comparative study of word-association responses. Although designed primarily to permit selection of word-pairs for Experiment II, it provides data suitable for testing the hypothesis that highly anxious and less anxious *S*s differ in regard to verbal response-tendencies. Experiment II is a comparison of paired-associate learning of highly and less anxious *S*s on a list of word-pairs in which (a) correct responses have markedly greater habit-strengths than any other responses at the start of learning, and (b) the initial probabilities of correct responses are approximately equal for both groups of *S*s. Experiment II was designed to test the hypothesis that under these conditions highly anxious *S*s will learn more rapidly than less anxious *S*s.

EXPERIMENT I

Materials. Forty stimulus-words were selected from the Kent-Rosanoff word-association list. The commonest response to each of these words had been given by at least 33% of a norm group.[6] The words were mimeographed in a single list arranged in a randomly determined order.

Subjects. A total of 346 students enrolled in the introductory psychology course responded to the word-list. Scores on the *MAS* were available for most of these students. Thirty-nine men and 23 women had scores of 23 or above on the *MAS* (highly anxious *S*s), and 39 men and 21 women had scores of 10 or below (less anxious *S*s).[7] Only the data from the high and low groups of *S*s were analyzed.

Procedure. In a regular classroom session the students were asked to read each word on the list and respond with the first word thought of. After all members of the class had indicated that they were finished, each person was asked to write his name and sex on the blank. No mention of the *MAS* was made in connection with this study.

Responses to each stimulus-word were tabulated for four groups of *S*s; highly anxious *S*s and less anxious *S*s, of each sex. The tabulation was carried out by an individual who was unacquainted with the purpose of the study. All word-forms were tabulated separately except for plural and singular forms which were tabulated together, and except for obvious misspellings.

Results. Highly and less anxious *S*s gave the same response as the commonest response to a given stimulus-word for 39 of the 40 words.[8] For each sex separately these commonest responses tended to be given by more less anxious than highly anxious *S*s. This was true of 24 words for men and 27 words for women *S*s, while the reverse (commonest word given more frequently by highly anxious *Ss*) occurred for 11 words in each sex-group.

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[5] Spence, Farber, and McFann, *op. cit.,* 300.

[6] Russell and Jenkins, *op. cit.*

[7] These represent the 80th and 20th percentile points for the students at Indiana University. See I. G. Sarason, Effect of anxiety, motivational instructions, and failure on serial learning. Unpublished doctoral thesis, Indiana University, 1955.

[8] Jenkins and Russell, Associative clustering during recall, *J. abnorm. soc. Psychol.,* 47, 1952, 818-821; W. A. Russell and L. H. Storms, Implicit verbal chaining in paired-associate learning, *J. exp. Psychol.,* 49, 1955, 287-293.

A 'commonality'-score was obtained for each *S* by counting the number of most common responses he had given. For each sex separately, the commonality scores for high- and low-anxious *Ss* were thrown together and divided at the median. The two fourfold tables yielded a combined $\chi^2 = 7.4$ with $df. = 2$, which corresponds to $p < 0.03$ for the hypothesis that level of anxiety is unrelated to commonality-score. For each sex separately, the less anxious *Ss* tend to give fewer of the commonest responses than highly anxious *Ss*, but while the χ^2 for men is significant at the 1% level, that for women is not significant. This may stem from the lack of a difference between highly and less anxious women or it may be largely a result of the relatively small number of women *Ss* available.

Discussion. These results clearly indicate that, at least for men, anxious *Ss* tend to give a greater variety of word-associations than do less anxious *Ss*. Since previous studies strongly suggest that word-association responses are indicators of more general verbal response-tendencies, the present results suggest that before verbal learning is compared for highly and less anxious *Ss* the material to be learned must be tested for equality of response-tendencies between the two groups.

EXPERIMENT II

Materials. Thirteen pairs of words, each consisting of a stimulus-word and its most commonly given response, were selected on the basis of data obtained from the men in Experiment I. For each word-pair the same response had occurred most frequently among both highly anxious and less anxious men in Experiment I, and the *t*-value for differences between the proportions of highly and less anxious *Ss* giving the commonest response was equal to, or less than, 1.0 for each word-pair. The responses used had been given, on the average, by 57% of both the highly and less anxious men in Experiment I, and each response had occurred at least twice as frequently as the next most common response to the same stimulus-word.

Subjects. The *Ss* were 56 men, students in the introductory course in psychology. None of these *Ss* had participated in Experiment I. Half of them (highly anxious group) had *MAS* scores at or above 23, the others (less anxious group) had *MAS* scores at or below 10. All *Ss* had taken the *MAS* in class sometime prior to this experiment. The *Ss* were informed that they had been randomly selected and were asked to serve in an experiment. No mention of the *MAS* was made in connection with this experiment.

Procedure. Word-pairs were presented to *S* by means of a Hull-type memory-drum. A 2-sec. exposure was used both for stimulus-words alone and for stimulus-response pairs, while a 4-sec. period was used between successive repetitions of the list. The list was arranged in three randomly determined orders which were presented cyclically until *S* had correctly anticipated all the responses on two successive repetitions of the list.

Results. The principal analysis in this experiment was carried out using data in the form of the number of trials to reach criterion. Results of a pilot study had indicated that this might be a more sensitive measure in this experimental situation than would number of errors. These data are shown in Table I. Because of the shape of the distributions, conventional statistical tests could not be used. Application of the multiple-hypergeometric distribution to these data yielded an exact probability of 0.055 on the null hypothesis that the two distributions were drawn from the same population.[9]

Examination of the data shown in Table I suggests that the major source of discrepancy between the two distributions is a tendency for the scores made by less anxious *Ss* to cluster around the low end of the distribution to a somewhat greater extent than do the scores made by highly anxious *Ss*. But whatever the exact nature of the difference is, the data do not support the hypothesis that anxious *Ss* learn more rapidly than less anxious *Ss*. The distributions of errors appear somewhat more similar than the distributions of trials to reach criterion, but again the less anxious *Ss* show a slightly smaller mean number of errors over all trials as well as a smaller mean number of errors on trials after the first.

TABLE I

Trials to Reach Criterion in Learning Paired Associates

No. of Ss	Trial criterion reached									Mean
	2	3	4	5	6	7	8	9	10	
Highly anxious	5	11	4	3	3	0	1	0	1	3·93
Less anxious	8	6	11	0	2	1	0	0	0	3·46

Discussion. The results of this experiment indicate that less anxious *Ss* learn slightly more rapidly than highly anxious *Ss* when they are required to learn a list of paired-associates in which correct responses have high initial habit-strengths and when the word-pairs are selected so that neither group of *Ss* has an initial advantage. This, of course, is counter to the result predicted on the basis of the hypothesis that manifest anxiety is directly related to level of drive when this hypothesis is combined with the assumption that response-occurrence depends on a multiplicative function of drive and habit-strength. Even if the reader chooses to accept the null hypothesis on the basis that the *p*-value is borderline, the data do not confirm the prediction. Neither the direction of the mean difference between groups, nor the greater skewness of the highly anxious trial to criterion-data, is such as to support the possible argument that the low power of the statistical test used has obscured a mean difference in favor of the highly anxious *Ss*. The same holds for the possible argument that the materials used rendered the task too easy so that the highly anxious *Ss* could not outstrip the less anxious *Ss*. In this latter connection it should be noted that the requirement that correct responses have greater habit-strengths than incorrect responses at the start of learning necessitates the use of material of a low level of difficulty and consequently makes small differences between means highly likely.

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[9] Computations were performed on the equipment of the Indiana University Research Computing Center. Drs. Julius Blum and Marshal Wrubel programmed the problem for the computer.

Since the prediction that highly anxious *Ss* would perform better than less anxious *Ss* was not confirmed, it suggests either that the hypothesis that manifest anxiety reflects level of drive, or that the postulated interaction drive-strength, is inadequate. Since there is a growing body of evidence which is difficult to reconcile with the hypothesis that *MAS* scores reflect drive-levels, the reformulation of manifest anxiety in terms other than drive would seem more desirable.[10]

SUMMARY

Two studies were performed. In the first, word-associations (40 stimulus-words) were collected from a total of 78 men and 44 women, who were classified as of 'high' and 'low' anxiety upon the basis of their scores on the *MAS*. Less anxious *Ss* tended to give more of the most commonly given responses than did highly anxious *Ss*, but the difference was significant only for the men.

In the second study, two groups ($N = 28$ each) of men, selected on the basis of *MAS* scores, learned a list of paired-associates to criterion. Word-pairs were so chosen on the basis of word-association data that the preëxperimental probabilities of correct responses were (a) high, and (b) approximately equal for the two groups. Distributions of trials to criterion differ at a significance-level of 0.055, with the anxious *Ss* tending to learn more slowly.

The implications of these results for research with groups selected on the *MAS*, and for the hypothesis that anxiety reflects level of drive was discussed.

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[10] See, for examples, A. M. Buchwald, and H. G. Yamaguchi, The effect of change in drive level on habit reversal, *J. exp. Psychol.*, 50, 1955, 265-268; Eli Saltz and A. J. Hoehn, A test of the Taylor-Spence theory of anxiety, *J. abnorm. soc. Psychol.*, 54, 1957, 114-117.

RETINAL SUMMATION AND INHIBITION

By S. M. Luria, University of Pennsylvania

The low brightness-thresholds and the poor acuity in the periphery of the retina have been ascribed to the phenomenon of retinal summation, which was first discovered in the periphery. When it was later found in the fovea,[1] it posed the problem of how summation was overcome there to render possible the high foveal acuity. The concept of retinal inhibition was subsequently introduced to explain the phenomenon.

Supporting evidence for this explanation was reported in 1931 by Graham and Granit.[2] In their study they determined first the critical flicker frequency (*CFF*) of two lights (*A* and *B*) of equal intensity; the *CFF* of each individually was 55.8 c.p.s. Then the two lights were presented together and the *CFF* was again determined; it was now 60.6 c.p.s. for each light. Next they dimmed one light (*A*); its *CFF* in isolation was now reduced to 54.7 c.p.s. When the second light (*B*) was presented at its original intensity with Light (*A*), the *CFF* of Light (*B*) was 55.8 c.p.s. This value, as predicted, is higher than the *CFF* of Light (*A*) alone and lower than the value of Light (*B*) when presented with undimmed Light (*A*). The *CFF* of the dimmed light (Light *A*) was found under these conditions to be 53.9 c.p.s., a drop of 1.5% from the value of this light when dimmed and measured by itself. Thus they found that, if two lights of different intensities are presented together, a depressive effect results in the *CFF* of the dimmer light instead of the usual summative effect. They did not, however, find this depressive effect in the periphery.

In proposing an explanation, the authors concluded that there is in the retina an inhibitory pathway especially well developed in the fovea. They proposed that the system of amacrine cells (whose function is obscure) was the inhibitory mechanism since these cells are more prominent in the fovea than elsewhere in the retina.[3] In this way they explained the high visual acuity in the fovea despite the summation over lateral channels.

Shortly thereafter, in 1936, Fry and Bartley found that the depressive effect could still be found with one light outside the fovea, and further that, as the separation between the two lights increased, the intensity of the light must be increased to produce a drop.[4] They concluded that the *CFF*-reversal depended on the spatial separation of the lights and not on a foveal-peripheral distinction.

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* Received for publication December 9, 1957. This paper is based on a portion of a doctoral dissertation submitted to the University of Pennsylvania. The writer is now at the U. S. Naval Medical Research Laboratory, New London, Connecticut.

[1] R. Granit, Comparative studies on the peripheral and central retina: I. On interaction between distant areas in the human eye, *Amer. J. Physiol.*, 94, 1930, 41-50.

[2] C. H. Graham and R. Granit, Comparative studies on the peripheral and central retina: IV. Inhibition, summation and synchronization of impulses in the retina, *Amer. J. Physiol.*, 98, 1931, 664-673.

[3] S. L. Polyak, *The Retina*, 1940, 297-308.

[4] G. A. Fry and S. H. Bartley, The effect of steady stimulation on one part of the retina upon the critical flicker fusion in another, *J. exp. Psychol.*, 10, 1936, 351-356.

Beitel has also obtained inhibitory effects outside the fovea under conditions of dark-adaptation.[5]

It is difficult to reconcile these findings with Graham and Granit's explanation. The question arises, furthermore, as to why Graham and Granit did not obtain depression in the periphery as they used in their peripheral study a separation of 8′ of a visual angle, the same width as their widest separation in their foveal study. In view of the lack of acuity in the periphery and the intensity of light they employed, there can be little doubt that their *Ss* were unable to differentiate the two lights. If this is true—and Graham and Granit themselves raise the question[6]—then it is not difficult to see why they did not find the effect. It would appear reasonable that the *Ss* must be able to see the two lights as distinct from each other before accurate *CFF* readings can be obtained, and if such a precaution is taken, the depressive effect should be evident.

METHOD AND PROCEDURE

Apparatus. The intermittent light stimulus was produced with an episcotister, a half-sectored disk, attached to a motor. The current for the motor was taken from a constant voltage regulator and fed through a variac. The variac permitted adjustments of voltage to be read in ½-v. steps. The speed of the episcotister was calibrated with a photocell and a Berkeley electronic counter. With this equipment, the number of light pulses passing through the episcotister was counted for 10 periods of from 30 to 60 sec. The resulting values have standard deviations which do not exceed 0.3 v., which corresponds to about 0.15 flashes per second.

A 1000-w. bulb was used as the source of illumination. The light was focused by a lens through the episcotister into the viewing screen. Between the lens and the episcotister and also between the episcotister and the screen were placed cardboard shields with small apertures which served as baffles to reduce the amount of stray light being reflected through the system. A voltmeter was so hooked into the electrical system that periodic checks could be made to see whether the amount of current passing through the bulb was constant.

The intensity of the light and the size of the aperture of the viewing screen could be regulated and varied by placing filters on the back of it and frames of different sizes on the front of it. The neutral-density filters were cut from photographic film which had been exposed to varying degrees of light-intensity. The film was developed and cut into appropriate sizes and shapes and taped to glass plates. The apertures themselves were so cut that, when viewed from a distance of 47 in., they subtended a visual angle of 1° 40′. For the foveal condition, they were separated by a strip whose visual angle was 4′ wide. For the peripheral condition, the frame was so cut that the separation subtended 30′ while the apertures again had a diameter of 1° 40′, total diameter in the periphery being 2° 10′. Between the filters and the frames, a piece of ground glass was placed to diffuse the light.

A 'light-box,' with a window measuring 3 × 8 in. which was covered with frosted glass, was placed 18 in. from *S*. A red light was used as a peripheral reference-point; a chin-rest, to stabilize *S*'s head-movements; and a second motor to mask the sound of the episcotister during the experiment were used.

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[5] R. J. Beitel, Inhibition of threshold excitation in the human eye, *J. gen. Physiol.*, 14, 1936, 31-61.

[6] Graham and Granit, *op. cit.*, 669.

Procedure. The *Ss* (seven graduate students and one instructor in psychology at the University of Pennsylvania) were dark-adapted for 10 min. and then brought up to about the same level of light-adaptation by gazing at the light-box for a period of 4 min. Then the light-box was darkened and the stimulus-light turned on. *S* was told that the task was to inform *E* when the semicircular lights, either singly or in pairs, were flickering or fused. Both conditions were demonstrated. Next *S* placed his head in the chin-rest and the light was presented flickering slightly above the fusion threshold; when *S* indicated that the light appeared fused, he was told to move his eyes slightly to one side. The light could now be seen as flickering. It was pointed out that this demonstration emphasized the necessity of suppressing extraneous movements during the judgments since they affect the *CFF*. Several practice-trials were given and *S* told to develop criteria for judging flicker or fusion; he was told he might set up his own criteria, but that he must adhere to them consistently for the remainder of the experiment. When *S* indicated he was

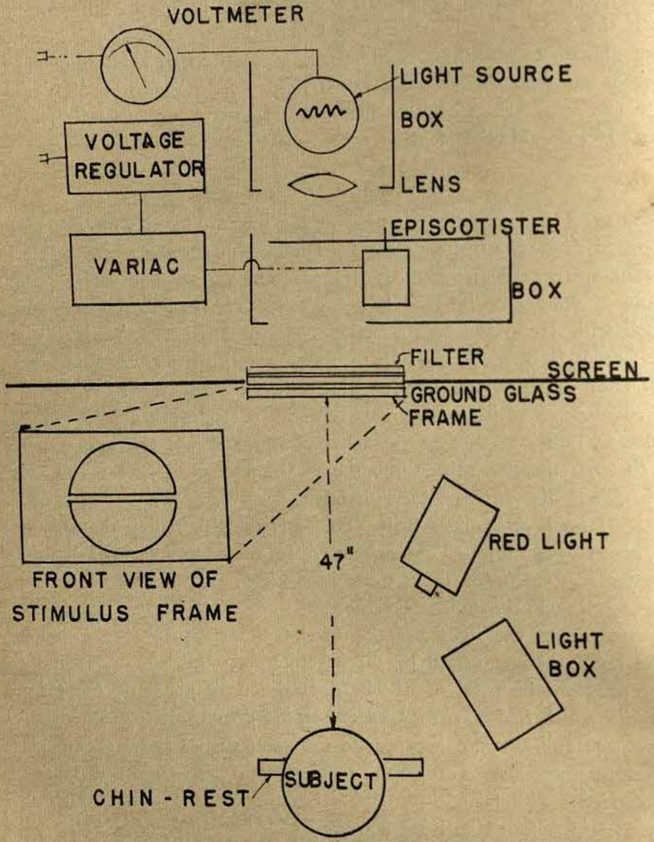

FIG. 1. DIAGRAMMATICAL ARRANGEMENT OF THE APPARATUS

ready to start, the *CFF* was determined for the various conditions by the method of limits. Five ascending and descending readings were taken and the *CFF* was the average of these 10 readings. At the start of each new condition, five practice-trials were given to provide an opportunity for adaptation to the new level of intensity. The conditions were presented in varying order with each *S*. All judgments were made monocularly. For the foveal conditions, the point of fixation was the center of the stimulus-object whose *CFF* was being determined. For the peripheral conditions, *S* was instructed so to fixate that the red light would fall on the blind spot and be invisible. Practice was given until *S* indicated that he could maintain this fixation without undue difficulty. Then the same procedure as for the foveal determinations was carried out.

TABLE I

CFF VALUES FOR THE VARIOUS FOVEAL AND PERIPHERAL CONDITIONS

	S	Bright semicircle*	Dimmed semicircle	Bright full circle	Dim half-circle†	Bright half-circle
Fovea	*EF*	41.8	40.9	44.8	40.5	43.2
	MH	39.6	39.7	42.0	37.3	40.4
	RS	40.8	40.5	45.5	39.0	40.8
	WA	38.7	37.0	39.8	35.8	39.3
	GM	40.8	39.5	43.0	38.6	41.1
	HG	40.2	39.5	41.7	38.5	40.5
	SL	52.5	50.7	52.8	49.9	52.1
	WAS	38.3	35.7	42.6	33.0	39.0
	Av.	41.6	40.6	44.0	39.3	42.0
Periphery	*EF*	36.7	34.5	40.8	33.4	38.3
	MH	43.0	41.5	46.6	39.8	45.0
	RS	38.0	37.5	41.3	32.5	38.5
	WA	48.3	46.0	51.3	44.0	49.0
	GM	43.8	41.1	48.3	41.0	47.0
	HG	40.3	39.9	43.7	38.0	43.3
	SL	49.3	45.7	53.5	42.5	53.4
	WAS	45.2	42.2	51.1	38.5	48.5
	Av.	42.5	41.1	47.1	38.9	45.4

* "Semicircle" refers to a semicircle presented in isolation.
† "Half-circle" indicates a semicircle in the presence of a second semicircle.

The peripheral angle of regard was 13° from the fovea. This setting was obtained by determining before the experiment began the position at which three people could distinguish the two semicircles at a separation of 30′. It was desired to ensure that the lights were falling on the retina far enough from the fovea that the cone-density was the same everywhere the light impinged. According to Østerberg's data, this condition is satisfied at 13° from the fovea.[7]

RESULTS AND DISCUSSION

The results for both foveal and peripheral conditions are given in Table I. The first column gives the values obtained with one semicircle at full intensity; the average *CFF* was 41.6 c.p.s. for foveal conditions and 42.5 for peripheral. The second column gives the *CFF* for the semicircle dimmed by about 50% as expected, this value is lower than the first reading. The third column gives the *CFF* for a semicircle at full intensity when presented together with another semicircle at the same level of brightness; these values are higher than the first readings. The fourth column gives the *CFF* obtained for a dimmed semicircle in the presence of another semicircle of original intensity. This is the critical reading. It will be noted that in every case the depressive effect is evident, both in the foveal and peripheral conditions. The last column gives the readings for the semicircle at original intensity when presented with the dimmed semicircle. These values are lower than the readings obtained in the presence of the other bright semicircle but higher than the values for the semicircle in isolation.

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[7] G. Østerberg, The topographic layers of rods and cones in the human retina, *Acta Ophthal. Suppl.*, Suppl. 6, 1935, 1-102.

The results of the present experiment lead to the rejection of Graham and Granit's explanation. Though Brooks and Eccles, in 1947, published a theory of inhibition similar to Graham and Granit's,[8] Brock, Coombs, and Eccles, in a more recent study, state that explanation in terms of inhibition involves postulates for which there is no evidence and conclude that the hypothesis is untenable.[9] This study indicates that there exists no sharp distinction between the fovea and the periphery of the retina in regard to inhibition and that an explanation of foveal acuity cannot rest on the assumption of a special inhibition process in the fovea.

SUMMARY AND CONCLUSIONS

Graham and Granit's study in which they found that the *CFF* of a light was depressed, in the presence of a brighter light, in the fovea but not in the periphery, was repeated. We discovered, when conditions were obtained in which the two lights could be differentiated at all times, that the depressive effect was obtained in the periphery as well as in the fovea. We concluded, therefore, that there exists no special inhibitory system in the fovea and that an explanation of the high foveal acuity cannot rest on the assumption of a foveal-peripheral distinction.

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[8] C. McC. Brooks and J. C. Eccles, An hypothesis of central inhibition, *Nature*, 159, 1947, 760-764.

[9] L. G. Brock, J. S. Coombs, and J. C. Eccles, The nature of the monosynaptic excitatory and inhibitory processes in the spinal chord, *Proc. Roy. Soc.*, 140, 1952, 170-176.

THE EFFECT OF STRYCHNINE SULPHATE ON MAZE-LEARNING

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In 1917, Lashley reported that injections of strychnine sulphate accelerated the rate of maze-learning in rats.[1] Lashley's conclusion was, however, only partially supported by his data. In his first experiment, rats which were given daily injections of strychnine sulphate made significantly fewer errors in learning a Watson maze than did water-injected controls, but, when the experiment was repeated with older and heavier animals, the strychnine group did not do better than the controls. Since all experimental animals received the same amount of strychnine, regardless of weight, the results could be interpreted to mean either that the results of the first experiment were due to 'chance' or that the dose used in the second experiment was too small for the heavier animals. A further investigation of the problem is reported here.

Subjects. The *S*s were 76 naïve rats of a heterogeneous strain maintained at the University of California. There were 68 males and 8 females, all about 80 days old when training was begun. The females were divided among the three experimental groups and one of the control groups.

Apparatus. A four-unit, eight-cul Lashley III alley-maze was employed. Each alley was 4 in. wide, 5 in. high, and 36 in. long. The entire maze was painted gray. The floor was marked with white lines 2 in. from both sides of each choice-point. The *S*s were pretrained in an 18-in. runway leading from the starting box of the maze to the goal-box.

Design. The *S*s were divided into seven groups—three experimental and four control—which were matched for age and weight. Those in the experimental groups received daily intraperitoneal injections of a 0.5% solution of strychnine sulphate. Group I ($N = 11$) received 0.66 cc. of solution per kg. of body-weight; Group II ($N = 11$) received 1.3 cc./kg.; and Group III ($N = 11$) received 2.0 cc./kg. In terms of milligrams of strychnine per kilogram of body weight, the dosages were 0.33, 0.66, and 1.0, respectively. The average lethal dose for this strain of rats was found to be about 2.2 mgm. per kg. of body-weight. Thus our dosages were about ⅙, ⅓, and ½ of the average lethal amounts.

Group IV ($N = 11$) had the needle inserted into the peritoneal cavity but no solution was injected. Groups V ($N = 11$) and VI ($N = 11$) were injected with the same amount of normal saline solution per kilogram of body-weight as the animals in experimental Groups I and III, respectively. Group VII ($N = 10$) was not injected, nor was the needle inserted.

Preliminary training. A hunger cycle was established by allowing the *S*s access to

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* Received for publication May 1, 1957. The work reported here was done in the psychological laboratory of the University of California at Berkeley.

[1] K. S. Lashley, The effect of strychnine and caffeine upon rate of learning, *Psychobiol.*, 1, 1917, 141-170.

TABLE I

MEAN NUMBER OF ERRORS AND RUNNING TIMES FOR EACH OF THE RESULTS FOR THE EXPERIMENTAL AND CONTROL GROUPS

Measure	Group										
	Experimental groups				Control groups					t (expt'l. vs control)	p (expt'l. vs control)
	I	II	III	I–III	IV	V	VI	VII	IV–VII		
Trials to criterion	19.0	20.8	21.4	20.4	25.3	35.4	26.6	19.6	26.8	2.95*	<0.02
Initial errors (Trials 2–15)	16.3	21.1	21.4	19.5	23.5	28.2	25.0	19.6	24.1	2.18	<0.05
Initial errors to criterion	18.2	25.1	24.7	22.7	31.1	45.3	35.3	24.6	34.1	3.19*	<0.01
Total errors (Trials 2–15)	19.0	32.1	26.7	25.9	35.0	40.7	40.3	26.9	35.7	2.60	<0.02
Total errors to criterion	20.9	36.2	30.5	29.2	43.8	58.8	52.4	32.2	46.9	3.34*	<0.01
Mean of median running times (last 5 trials of pretraining)	6.4	6.2	6.1	6.2	6.0	7.6	5.0	5.2	5.9	.49	
Mean of median running times (last four errorless trials)	10.0	9.7	8.8	9.5	9.2	7.1	6.5	6.7	7.3	1.97*	
% weight-loss (days 1, 2, 3)	11.0	12.5	13.5	12.3	11.0	10.4	10.6	12.7	11.2	1.04	

* Degrees of freedom were adjusted according to Welch's method since the requirement of homogeneity of variance was not met in these comparisons (see H. M. Walker and J. Lev, *Statistical Inference*, 1953, 157–158).

food (wet mash) for only 30 min. per day (in the home cage). On the fourth day the *Ss* were injected for the first time, placed in a retaining cage for 10 min., and then allowed only 15 min. access to the mash. On the fifth day, the *Ss* were injected, placed in a retaining cage for 10 min., and then allowed to feed for 5 min. in the goal-box of the maze. They were then given 10 min. of additional feeding in the home cage. (This additional feeding was allowed on each subsequent day.) There followed four days of training in the runway during which the animals were then given a total of 18 trials—three on the first day and five on each of the next three days. Throughout the experiment, every *S* (except those in Group VII) was injected and placed in a retaining cage for 10 min. before the daily trials.

Maze-training. On the tenth day, each *S* was given two trials in the maze. The number of errors per trial and the time per trial were recorded. An error was scored each time *S*'s head was extended beyond one of the white lines on the floor of the maze. There were three trials in the maze on the next day, and on each succeeding day there were five trials, with 30sec. of feeding in the goal-box following each run. The criterion of learning was five out of six errorless runs, but each animal was given at least 15 trials. The animals were weighed before and after each day's trials.

Results. The results are shown in Table I. Analyses of variance were made separately for the experimental groups (Groups I, II, III) and for the control groups (Groups IV, V, VI, VII) for each of the measures. Since none of the *F*-ratios was significant, the pooled results for all 43 control animals could be compared with the pooled results for all 33 strychnine animals. As may be seen in Table I, the combined strychnine groups required fewer trials to reach criterion, made fewer initial and total errors to criterion, and made fewer initial and total errors on Trials 2-15. For each of these measures, the difference between the combined strychnine groups and the combined control groups was significant beyond the 5% level of confidence.

There was a tendency for the non-injected control group (Group VII) to make fewer errors than the injected controls (Groups IV, V, and VI). For total errors to criterion, the difference was significant at the 5% level. No confidence is placed in this finding, however, in view of the after-the-fact nature of the test, the failure of differences in other measures to reach significance and our failure to replicate the finding in subsequent experiments.

The mean percentage of weight-loss during the first three days of maze-training was calculated for each group. As Table I shows, the mean for the combined control groups was not significantly higher than that for the combined experimental groups. For all animals, the correlation between percentage of weight-loss and total errors on Trials 2-15 was only +0.07. As can be seen in Table I, the two combined groups did not differ significantly in speed of running.

Discussion. The results of this experiment support the conclusion that strychnine sulphate enhances maze-learning. There are several possible hypotheses which might account for this result. It is known that strychnine exerts excitatory effects on the central nervous system.[3] *In vitro* studies of the physiological effects of strychnine have shown that, in low concentrations, strychnine reduces synaptic resistance and prolongs the period of latent addition at the synapse.[3] It is possible that strychnine facilitates learning by increasing the efficiency of transmission in the central nervous system. Platt has reported that injections of di-isopropyl fluorophosphate (*DFP*) facilitate discriminative learning in rats.[4] This finding is particularly relevant to the above hypothesis since, in low doses, *DFP* also is known to have an excitatory affect upon neural activity.[5]

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[3] L. S. Goodman and A. Gilman, *The Pharmacological Basis of Therapeutics,* 1955, 331.

Krech, Rosenzweig, and Bennett have reported a relationship between brain-cholinesterase (*ChE*) and discriminative learning.[6] They suggest that the *ChE*-activity provides an index of the transmission-efficiency of the central nervous system, and that transmission-efficiency is correlated with the capacity for adaptive behavior.

There is some evidence, however, to support the view that strychnine may enhance performance rather than learning. In man, strychnine increases visual acuity[7] and efficiency of motor performance.[8] Zalmanson has reported that local application of strychnine to the cerebral cortex enhances the performance of a conditioned withdrawal-response in dogs.[9] In his original study, Lashley also reported that strychnine improved the performance of an already learned maze-habit when strychnine and distilled water were injected on alternate days, but Miles did not find such an effect.[10] Since strychnine has been used as a tonic in medicinal practice to increase appetite,[11] it is possible that strychnine may improve performance by affecting motivation. If so, some effect on weight might be expected, since the animals had access to food only a few minutes each day. The data do not support this hypothesis. Further doubt is cast on the motivational interpretation by the fact that running speeds were not affected.

Summary. Three groups of rats injected with different amounts of strychnine sulphate and four control groups were trained on a Lashley three-alley maze. The maze-performance of the experimental animals (measured in terms of trials and errors) was superior to that of the controls, although no differences in weight-loss or running time were found. The significance of these findings was discussed.

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[3] P. Heinbecker and S. H. Bartley, Manner of strychnine action on nervous system, *Amer. J. Physiol.*, 125, 1939, 172-187.

[4] C. E. Platt, The effects of subcutaneous injections of di-isopropyl fluorophosphate (*DFP*) on the rate of learning a discrimination problem by albino rats, unpublished doctoral thesis, Ohio State University, 1951.

[5] D. Grob, A. M. Harvey, O. R. Langworthy, and J. L. Lilienthal, The administration of di-isopropyl fluorophosphate (*DFP*) to man: III. Effect on the central nervous system with special reference to the electrical activity of the brain, *Bull. Johns Hopkins Hosp.*, 1947, 81, 257-266; C. J. Hylander, The medical division of the Army Chemical Corps and its research activities, *Chemical Corps Medical Division Reports*, No. 200, July, 1949 (as cited by Platt).

[6] David Krech, M. R. Rosenzweig, and E. L. Bennett, Dimensions of discrimination and level of cholinesterase activity in the cerebral cortex of the rat, *J. comp. physiol. Psychol.*, 49, 1956, 261-268.

[7] W. H. White (ed.), *Textbook of Pharmacology and Therapeutics*, 1901, 283.

[8] W. McDougall and M. Smith, *The Effects of Alcohol and Some Other Drugs During Normal and Fatigued Conditions*, 1920.

[9] A. N. Zalmanson, Conditioned defense reflexes after local poisoning of the cortical motor centers with strychnine and cocaine, *Vyschaya Nervnaya Deyatel'nost, Moscow.* GIZ, 1929, 39-48 (*Psychol. Abstr.*, 8, 1943, 21).

[10] W. R. Miles, Drug effects measured by acquired patterns of response, *Amer. J. Physiol.*, 90, 1929, 451.

[11] W. H. White, *op. cit.*, 288; H. N. Wright, and M. Montag, *A Textbook of Materia Medica, Pharmacology, and Therapeutics*, 1942, 124.

INTERDEPENDENCE OF SUCCESSIVE AFFECTIVE JUDGMENTS

By WARREN W. WILLINGHAM, U. S. Naval School of Aviation Medicine

When a person is required to guess blindly in two-choice situations, there is convincing evidence that his successive responses are not independent. If he responds 'heads' on one trial, he is more likely than not to respond 'tails' on the next trial.[1] Thus, there is a tendency to alternate responses more often than is dictated by chance. Attempts to explain this phenomenon of alternation have followed one of two lines of reasoning. Skinner offered one explanation which we may refer to as 'guessing habits.' In his re-analysis of Goodfellow's data, Skinner said, "A tendency is set up in the growing child, through readily observable processes of conditioning, which opposes repetition.[2] . . . The present data [Goodfellow's] demonstrate the additional fact that alternation is more likely to occur after failure to alternate and less likely after previous alternation. This may plausibly be attributed to the conditions of reinforcement which establish the tendency to alternate."[3] The second explanation might be termed 'misconception of chance' and was first referred to in the present context by Goodfellow.[4] This explanation is based on the very common 'maturity fallacy' in gambling.[5] Gamblers often have *l'idée fixé* to the effect that if one of two events of equal probability occurs, it is more likely to be followed by the other, since the proportions must eventually balance.

When we come to the point of predicting behavior, it may be seen that these two explanations have quite different logical consequences. If the tendency to alternate in a two-choice situation is due to a misconception of chance, then we expect this alternation of reponses only when the subject (*S*) perceives the situation as involving random, correct responses. On the other hand, taking Skinner literally, we would assume that in any choice-situation our behavior is governed to some extent by this basic contingency of response. Furthermore, it would become mandatory to tie in this mechanism with existing theories of behavior.

If this tendency toward alternation is due to habits of guessing, it would certainly show up in the constant psychophysical methods when discriminations are subliminal. We might reasonably make the same prediction on the basis of *S*'s misconception of chance since it is highly probable that *S* rightly assumes that *E* has 'mixed up' the correct answers. A bias toward alternation in psychophysical data has

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* Received for publication October 7, 1957. The opinions and conclusions expressed here are those of the author. They are not to be construed as reflecting the views of the Navy.

[1] L. D. Goodfellow, A psychological interpretation of the results of the Zenith radio experiments in telepathy, *J. exp. Psychol.*, 23, 1938, 601-632; The human element in probability, *J. gen. Psychol.*, 23, 1940, 201-205; B. F. Skinner, The processes involved in the repeated guessing of alternatives, *J. exp. Psychol.*, 30, 1942, 495-503; R. L. Soloman, A note on the alternation of guesses, *ibid.*, 39, 1949, 322-326.

[2] *Op. cit.*, 499 f.

[3] *Idem*, 500.

[4] Goodfellow, *op. cit.*, *J. gen. Psychol.*, 23, 1940, 201.

[5] H. C. Levinsen, *The Science of Chance*, 1950, 33.

been reported several times[6] with some exceptions,[7] which may be due to explanations offered by Senders.[8] In the area of affective judgment, however, these explanations predict different results. If we ask *Ss* whether they like or dislike geometric designs, we would expect some alternations in this two-choice response on the basis of 'guessing-habits,' since this hypothesis presupposes a basic mechanism of alternative responses. On the basis of 'misconception of chance' we would not, however, expect any tendency to alternate responses of 'like-dislike' since there are no correct responses which could be randomly ordered. Furthermore, interdependence of responses in psychophysical measurement has been the subject of considerable recent research. Consequently, the question whether this tendency to alternate carries over into affective judgment is of interest in its own right. This experiment was designed for the dual purpose of clarifying the mechanism of alternation and delineating its effect.

METHOD AND PROCEDURE

When examining a series of responses of 'like-dislike' based on affective judgments, it is impossible to determine to what extent the alternations represent a tendency to alternate and to what extent they represent the intercorrelations between successive items. A large number of alternations between a successive pair of items may simply mean the items are negatively correlated. Consequently, it is necessary to approach the problem obliquely. Given two items, *A* and *B*, we administer them to one group in the order *A–B*. We administer the same items to another group in the order *A–X–B;* that is, with another item interspersed. A tendency to alternate will attenuate r_{AB} when the order is *A–B*, but this same tendency will elevate r_{AB} when the order is *A–X–B*. This basic notion is incorporated here in a somewhat more complex design.

Material. The stimulus-objects presented the *Ss* were geometric designs pasted on white 4 × 6-in. cards. The design on each card consisted of two patches of color differing in one or more of the following dimensions: hue, saturation, shape, and size. There was a total of 25 cards in the series with the actual test-cards embedded as Numbers 16, 17, 18, and 19. The test-cards were so selected that the intercorrelations would probably be low. It was necessary that the material used have so little affective value that the tendency to alternate would show even if it were of little magnitude. The material fitted the requirements quite well. Even though practically all of the *Ss* took the task seriously, they frequently commented that they felt little or no basis for making their judgments. Such a condition maximizes the effect of the bias to alternate responses.

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[6] Leon Arons and F. W. Irwin, Equal weights and psychophysical judgments, *J. exp. Psychol.*, 15, 1932, 733-756; S. W. Fernberger, Interdependence of judgments within the series for the method of constant stimuli. *ibid.*, 3, 1920, 126-150; M. G. Preston, Contrast effects and the psychophysical judgments, this JOURNAL, 48, 1936, 389-402; Contrast effects and the psychometric functions, this JOURNAL, 48, 1936, 625-631; Soloman, *op. cit.*, 326; W. W. Willingham, Interdependence of successive comparative judgments, Project NM 14 02 11, Sub-task 12, Report No. 2, U. S. Naval School of Aviation Medicine, Pensacola, Fla., May 21, 1957.

[7] J. E. Conklin and Roberta Sampson, Effect of stimulus-determination on response- interdependence at threshold, this JOURNAL, 69, 1956, 438-442; Virginia Senders, Further analysis of response sequences in the setting of a psychophysical experi- ment, this JOURNAL, 66, 1953, 215-228; Virginia Senders and Ann Sowers, Analysis of response sequences in the setting of a psychophysical experiment, this JOURNAL, 65, 1952, 358-374.

[8] Senders, *op. cit.*, this JOURNAL, 66, 1953, 266.

Testing. All of the possible permutations (24) of the 4 test-items were embedded in the series and administered to groups of 50 *Ss*. The *Ss* involved were 1200 Naval aviation cadets or aviation officer candidates. They were tested in groups of 20 to 30.

Instructions. The following instructions were given the *Ss*:

> We are making an extensive analysis of human judgment. Obviously, judgment plays a major role in any complex task. I am going to show you a number of cards. I want you to look at the whole design on the card and decide whether you like it or dislike it. Use any standards you wish in making these judgments. There are no right or wrong answers. I only want your opinion. I will call out the number of each card. Give me your first impression by putting down an 'L' for like or a 'D' for dislike. Make a choice on every card.

RESULTS

The percentage of 'like' responses for Cards *A, B, C,* and *D* were 25, 62, 59, and 34, respectively. The tetrachoric intercorrelations between the cards ranged from slightly negative to moderately positive. Fig. 1 shows the variations in the intercorrelations as a function of the number of intervening cards. In the case of each intercorrelation, the figure shows three coefficients based on groups of 600, 400, or 200 *Ss* (for Positions 0, 1, and 2 respectively). For example, 600 *Ss* judged the cards in sequences such that *A* and *D* were adjacent. Considering these *Ss*, we see from Fig. 1 that r_{AD} is about 0.50. If there were a tendency for the *Ss* to alternate or perseverate successive judgments, the intercorrelations would show either a consistent rise followed by a drop, or vice versa. No such tendencies are evident in Fig. 1. Some variation does occur, the significance of which cannot be tested. Considerable scrutiny did not, however, reveal any explanation for the variations other than chance. This view is supported by the data as presented in Table I. The first line represents all possible sequences of responses in terms of alternations (A) or

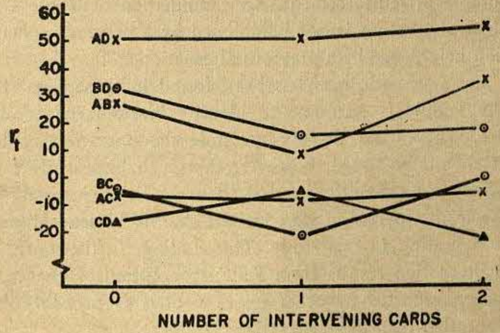

FIG. 1

TABLE I

FREQUENCIES OF ALL POSSIBLE SEQUENCES OF ALTERNATION (A) AND NON-ALTERNATION (O)

Response-sequence	OOO	OOA	OAA	OAO	AAA	AAO	AOO	AOA
No. of Ss	181	137	137	154	130	151	163	147

repetitions (0). Since the cards are presented in every possible permutation, we can expect a theoretical frequency of 150 for each order if there are no biases operating, if the average intercorrelation is near zero, and if the over-all percentage of 'like' is near 50. The two latter conditions are reasonably well met and the Chi-square test of goodness of fit is insignificant at the 5% level. This result is consistent with an assumption that the response was not biased. The variations from 150 which do occur are in general in line with the slight preference for the response 'dislike' and the slightly positive over-all average intercorrelation.

Another check on whether biases toward alternation or repetition are present in the data lies in a finding of Skinner.[9] In his re-analysis of Goodfellow's data, Skinner pointed out that the base-level of alternation was low (53%), but after a repetition the tendency to alternate was very high (75%). Skinner explained this in terms of reinforcement, but the finding also fits in quite logically with the hypothesis of 'misconception of chance.' It simply means that the 'maturity fallacy'—the occurrence of a chance event decreases the probability of that event on the succeeding trial—is much stronger after two identical responses. Since the effect was so large in Goodfellow's data, we would certainly expect it to appear here; that is, if the effect is attributable to reinforcement. An examination of Table I shows, however, that the level of alternation is not heightened by a previous repetition. Nor is the level of repetition heightened by a previous alternation.

No evidence of any systematic bias has been found in these data. It is believed that the size of the sample is adequate to identify biases large enough to be of practical significance. These data do not preclude the operation of biases at the individual level. It should be noted, however, that such biases affect validity only through the attenuation of reliability, and such a condition is easily remedied by increasing the *N*. From the foregoing we conclude that no biases—either to alternate or to repeat—were operating in this study. This finding, furthermore, supports the view that frequent alternation results from *S*'s misconception of chance, *i.e.* that the bias toward alternation reported by other experimenters is probably due to a specific experimental set rather than to a generalized tendency.

SUMMARY

This study, an investigation of the bias between alternatives of the affective judgments 'like-dislike,' was undertaken to determine: (1) whether the bias operates in the field of affective judgments, and (2) whether the bias to alternate between 'two-choice' judgments is due (a) to a generalized tendency, or (b) to a misconception of chance.

We would expect to find a bias toward alternation between affective judgments if the bias is due to a generalized tendency; we would not expect to find it if it is due to a misconception of chance since there are no objectively correct responses in the affective field and *S* would consequently not be set to presuppose a chance-order.

Responses of 'like-dislike' by 1200 *Ss* to four designs were analyzed. No evidence of a bias was found. We concluded, therefore, (1) that the bias between alternatives does not operate in situations involving affective judgments, and (2) that the bias between alternatives is probably due to a misconception of chance rather than to a generalized tendency to alternate.

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[9] Skinner, *op. cit.*, 498.

MANIFEST ANXIETY AND THE LEARNING OF SYLLABLES WITH DIFFERENT ASSOCIATIVE VALUES

By LUCIANO L'ABATE, Michael Reese Hospital, Chicago, Illinois

The role of manifest anxiety in the learning of nonsense-syllables as paired-associates is still obscure. Spence predicted that a high level of anxiety would interfere in the learning of paired-associates under conditions where there might be a considerable amount of competition among responses. He and his associates have reported findings that support this view.[1] On the other hand, there also have been negative results that cannot be easily ignored.[2] Part of the difficulty, of course, lies in the uncertainty about the rate of learning when stimulus- and response-terms possess various degrees of associative value.[3] The early report of McGeoch, unpublished research of Sheffield, and a recent paper by Noble and Stockwell suggest that verbal learning is more rapid when the associative value of each stimulus- and response-term is high. By pairing syllables from the extremes of associative value, it is possible to obtain combinations yielding different rates of acquisition.

It should be noted that McGeoch was the only experimenter to use nonsense-syllables of different associative value. Sheffield, as well as Noble and Stockwell, employed combinations of nonsense-syllables and familiar three-letter words or adjectives possessing degrees of meaning not clearly defined.

If it is valid to assume that associative value is a measure of previous and, therefore, competitive learning, it should be possible to predict from Spence's theory that a high level of drive and high associative value should result in a slow rate of acquisition, because of the interference of previously acquired associative tendencies. This is the argument advanced by Montague.[4] When the associative value is low, on

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[1] J. A. Taylor and J. P. Chapman, Anxiety and the learning of paired-associates, this JOURNAL, 68, 1955, 671; K. W. Spence, I. E. Farber, and H. H. McFann, The relation of anxiety (drive) level to performance in competitional and non-competitional paired-associates learning, *J. exp. Psychol.,* 52, 1956, 296-305; K. W. Spence, J. A. Taylor, and Rhoda Ketchel, Anxiety (drive) level and degree of competition in paired-associates learning, *ibid.,* 306-310.

[2] S. J. Korchin and Seymour Levine, Anxiety and verbal learning, *J. abnorm. soc. Psychol.,* 54, 1957, 234-240; Luciano L'Abate, Transfer and manifest anxiety in paired associate learning, *Psychol. Rep.,* 2, 1956. 119-126; Eli Saltz and A. J. Hoehn, A test of the Taylor-Spence theory of anxiety, *J. abnorm. soc. Psychol.,* 54, 1957, 114-117.

[3] J. A. McGeoch, The influence of associative value upon the difficulty of nonsense syllable lists, *J. genet. Psychol.,* 37, 1930, 421-426; F. S. Sheffield, The role of meaningfulness of stimulus and response in verbal learning, unpublished doctoral thesis, Yale Univer., 1946; C. E. Noble and F. E. Stockwell, Stimulus vs. response meaningfulness (*m*) in paired-associate verbal learning, *Amer. Psychologist,* 12, 1957, 425.

[4] E. K. Montague, The role of anxiety in serial rote learning, *J. exp. Psychol.,* 45, 1953, 91-96.

the other hand, then a high level of drive should produce better performance than a low level, because of the lack of competing responses. The study reported here was conducted, therefore, to explore three points, namely: (a) whether anxious *Ss* perform better than less anxious *Ss* in a paired-associates learning; (b) whether anxious *Ss* perform relatively better whenever the associative value of the stimulus-response pairings decreases from high to low; and (c) to obtain more precise evidence concerning the relationship between rate of verbal learning and the different associative values of the two elements in paired-associates learning.

Method. (a) Subjects. The *Ss* of this study were 65 undergraduate students from classes in general psychology. They were selected on the basis of their scores on the upper and lower quartiles of the distribution obtained on the Taylor Scale of Manifest Anxiety. They were randomly assigned to four groups (Fig. 1). Twelve *Ss* had been dropped from this sample for failure to learn three paired-associates within the first 10 learning-trials. Most of these drop-outs had been assigned to an extremely difficult condition, where the stimulus- and response-terms possessed zero associative value.

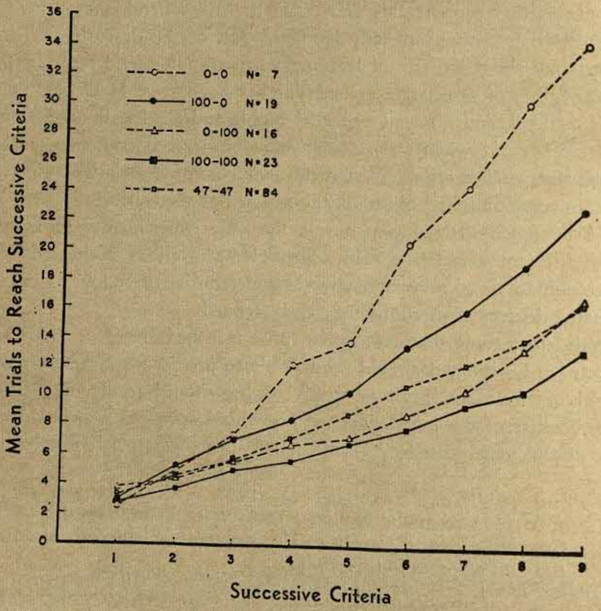

FIG. 1. MEAN TRIALS TO REACH SUCCESSIVE CRITERIA AS A FUNCTION OF DIFFERENT ASSOCIATIVE VALUES

(b) Stimulus-materials. Nonsense-syllables from Hilgard's tables were grouped in four lists of nine paired-associates, in conformance to the conventional criteria outlined by the same author.[5] The four lists had, as stimulus- and response-terms,

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[5] E. R. Hilgard, Methods and procedures in the study of learning, in S. S. Stevens (ed.), *Handbook of Experimental Psychology*, 1951, 541-546.

syllables chosen for their associative value as follows: List 100-100; List 0-100; List 100-0; List 0-0. Each *S* was assigned at random to memorize one of these lists by the anticipation-method. Neither the *S* nor the *E* was aware of the anxiety-level of each *S*. The same standard procedure was followed as in a previous experiment by this author.[6] The learning-criterion was one errorless repetition of the whole list.

Results. Two measures of learning were used in the analysis of these results: (a) mean number of trials to reach one errorless repetition of the nine paired-associates (Table I); and (b) Underwood's transformation, consisting of the mean number of trials to reach the successive criteria of learning one and more paired-associates (Fig. 1).[7] Both of these measures were found to correlate highly ($r = 0.92$). Additional data from the previously mentioned study, nonsense-syllables of 47-47 associative value, are included in the figure for comparison.[8]

Learning becomes progressively more difficult for the four lists as the associative value decreases. An analysis of variance yielded a highly significant *F*-ratio for the four lists ($F = 23.25$, $p < 0.001$). No other main effect or interaction was found to be significant. Thus our results for anxiety contradicted Montague's results. If however, the learning data of the author's previous experiment are added to the present data, a significant interaction between level of manifest anxiety and sex is found ($F = 4.40$, $p < 0.01$). By the use of Lindquist's formula, it is possible to test for the effect of anxiety on learning for either sex.[9] In the case of 35 high-anxiety and 41 low-anxiety men, the difference in means (21.28 and 18.71 respectively) reaches a barely acceptable level of statistical significance ($F = 2.87$, $0.05 < p < 0.10$), while the difference is not statistically significant between the high- and low-anxiety women ($F = 1.50$). Anxious men learn more slowly (in a greater number of trials) than less anxious ones. These effects are independent of the list used and are contrary to our expectations.

TABLE I

Mean Number of Trials to Reach Criterion of Learning

Sex	Anxiety-level	Associative Values							
		100–100		0–100		100–0		0–0	
		Mean	*SD*	Mean	*SD*	Mean	*SD*	Mean	*SD*
Men	high	16.75	9.22	13.50	3.31	24.71	5.79	35.00	—
	low	15.50	15.75	15.50	5.43	18.33	4.93	38.67	13.65
Women	high	8.33	4.14	20.00	5.10	21.67	6.44	28.00	—
	low	13.00	6.17	17.80	3.64	23.00	3.60	31.00	7.81

In both studies, such differences as were found in women were in the opposite direction. Anxious women on the average learned in a smaller number of trials

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[6] L'Abate, Transfer and manifest anxiety in paired-associates learning, doctoral thesis, Duke Univer., 1956.

[7] B. J. Underwood, *Experimental Psychology: An Introduction*, 1949, 394 ff.

[8] L'Abate, *Psychol. Rep.*, 119-126.

[9] E. J. Lindquist, *Design and Analysis of Experiments in Psychology and Education*, 1953, 213.

(17.57 and 19.50 for both studies) than less anxious ones (19.28 and 21.20 for both studies). The composite results of this experiment and the previous one indicate that men behaved contrary to our prediction, while women conformed to the predictions but not at an acceptable level of significance. Furthermore, these results fail to support hypotheses derived from the Taylor-Spence theory of anxiety, since the associative values of the syllables did not interact with anxiety in our results.

Summary. This experiment was conducted to verify two hypotheses concerning the role of manifest anxiety in learning material with different associative values. Although it was found that learning becomes more difficult as associative value decreases, neither of these hypotheses was supported. A sex-difference was found when all lists were considered. Anxious men learned more slowly than less anxious men, while anxious women learned more rapidly than less anxious women.

ASSESSING COMMUNICATION WITH THE SEMANTIC DIFFERENTIAL

By MELVIN MANIS, VA Hospital, Ann Arbor, Michigan

The statement of an opinion is often motivated by the communicator's desire to make his views known to the recipient. This suggests that, at one level, the effectiveness of such statements can be measured in terms of the similarity between (1) the communicator's views and (2) the recipient's conception of the communicator's views. In the present study, these two variables were quantified by means of the semantic differential.[1]

The semantic differential consists of a series of seven-step bipolar rating-scales (such as good-bad, strong-weak, honest-dishonest) which can be used to quantify the connotations associated with a variety of concepts (for example, jazz, labor unions, baseball). While theoretically one can conceive of an almost limitless series of bipolar scales, many of the scales correlate highly with one another. Osgood and Suci have utilized factor-analytic procedures to reduce 50 such descriptive scales to three relatively independent factors: an evaluative factor (good-bad), a potency-factor (strong-weak), and an activity-factor (active-passive). Of these, the first factor is by far the most important and accounts for more non-error variance than the other two combined.[2]

Method. (1) Communicators. Five undergraduate students wrote short passages explaining their views on (a) college fraternities and (b) the University of Pittsburgh. After returning their messages to the *E,* these communicators rated each of the message-topics, on nine scales of the semantic differential (see below). These ratings were used to quantify the communicators' views.

(2) Recipients. The two sets of messages were distributed in two successive class sessions of an undergraduate psychology course. The students (henceforth called recipients) read each message and then predicted how the writer had described his topic on the nine rating-scales. These predictions served to quantify the recipients' conceptions of the views held by each communicator.

The data that are presented below were obtained from the 30 recipients who were present at both testing-sessions.

(3) Scales. Three relatively 'pure' scales, as determined by Osgood and Suci's factor analysis, were used to estimate each of the three dimensions of connotative meaning.[3] Factor I (evaluation) was represented by good-bad, honest-dishonest, and fair-unfair; Factor II (potency) was represented by strong-weak, large-small, and

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* Received for publication January 30, 1958. This study was conducted under a PHS Career-Teacher Grant at the University of Pittsburgh. My thanks are due John Coury, who assisted in collecting and analyzing the data.

[1] C. E. Osgood, G. J. Suci, and P. H. Tannenbaum, *The Measurement of Meaning,* 1957.

[2] Osgood and Suci, Factor analysis of meaning, *J. exp. Psychol.,* 50, 1955, 325-338.

[3] *Ibid.,* 331.

heavy-light; Factor III (activity) was represented by hot-cold, fast-slow, and active-passive.

The nine scales were presented in a fixed randomized order, with the scales representing each factor so rotated that some ran from left to right and others from right to left.

(4) Measuring communication. Communication on each topic was studied one dimension at a time.[4] First, to increase the stability of measurement, the three scales representing each dimension were summed; this was done for (a) each recipient's predictions and (b) each communicator's ratings. For each dimension, Kendall's *tau* was then used to measure the correlation between each recipient's predictions for the five messages and the five communicators' ratings; these *taus* constituted the measure of communicative efficiency.[5] For example, if on Dimension I, a recipient's predictions for the five college fraternity messages showed a high positive correlation with the five communicators' corresponding ratings, this indicated that the communicators had been relatively successful in transmitting their respective evaluations of fraternities to this recipient; *i.e.* they had succeeded in informing him of their respective scale-positions on the good-bad continuum. Similarly, if on Dimension II, a recipient's predictions correlated highly with the communicators' ratings for this dimension, this meant that the recipient had successfully inferred how *potent* (strong vs. weak) each of the communicators felt fraternities to be.

TABLE I

MEAN *TAUS* BETWEEN RECIPIENT-PREDICTIONS AND COMMUNICATOR-RATINGS

Dimension	College-fraternities		University of Pittsburgh	
	uncorrected *taus*	partial *taus*	uncorrected *taus*	partial *taus*
I. good-bad	.63*	—	.38*	—
II. strong-weak	.33*	.13	.02	−.02
III. active-passive	.39*	−.08	.39*	.18†

* $p<0.01$. † $p<0.02$.

Results. Table I presents, for the mean recipient, the *taus* between his five predictions and the communicators' ratings; *taus* are reported separately for each message-topic and each dimension of meaning. A modified version of Wilcoxon's paired ranks test was used to evaluate the hypothesis that the *taus* within each cell were not significantly greater than chance expectancy, *i.e.* zero.[6]

(1) Uncorrected taus. First consider those portions of Table I that deal with the uncorrected *taus*. On Dimension I, for the messages about college fraternities, the mean recipient's predictions correlated 0.63 with the communicators'

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[4] A somewhat similar procedure for measuring communicative efficiency has been utilized by Nebergall in an unpublished doctoral dissertation. See R. F. Nebergall, An experimental study of rhetorical clarity, Unpublished doctoral thesis, University of Illinois, 1956.

[5] M. G. Kendall, *Rank Correlation Methods*, 1948, 1-36.

[6] S. Siegal, *Nonparametric Statistics for the Behavioral Sciences*, 1956, 75-83.

ratings ($p < 0.01$). The uncorrected correlations for Dimensions II and III of the college fraternity messages were also significantly greater than zero. In short, for this message-topic, communication was successful on all three dimensions of meaning. For the messages about the University of Pittsburgh, communication was successful on Dimensions I and III, but not on Dimension II.

(2) Partial taus. An examination of the data revealed that the communicators' ratings on Dimensions II and III (strong-weak and active-passive) were not independent of their ratings on Dimension I (good-bad); similarly, the recipients' predictions for Dimensions II and III were correlated with their predictions for Dimension I. Partial *taus* were therefore computed, providing a measure of each recipient's predictive accuracy on the last two dimensions, after the influence of his predictions for Dimension I had been statistically removed.[7]

In Table I, the columns that are labelled "partial *taus*" represent the residual predictive power for the mean recipient after this corrective procedure had been applied. Neither of the partial *taus* for Dimension II differs significantly from zero; when the influence of the predictions on Dimension I (good-bad) was eliminated by statistical means, the average recipient could not predict the communicators' ratings for Dimension II (strong-weak) with better than chance accuracy. The partial *taus* for Dimension III (active-passive) of the University-of-Pittsburgh messages *were* significantly greater than zero, indicating that for these messages, communication on the third dimension was *not* solely dependent on the recipients' Dimension I (good-bad) predictions. The results for the college fraternity messages were not, however, significant.

In summary, a comparison of the uncorrected *taus* and the partial *taus* in Table I suggests that the accuracy of the original predictions for Dimensions II and III may have been a result of the recipients' assumption that, if a communicator seemed to approve of his topic, he was also likely to consider it 'strong' and 'active.' When the consequences of this assumption were controlled by statistical means, predictions on Dimensions II and III were markedly impaired.

Conclusions. This study suggests that the semantic differential can be profitably employed in assessing the communication of *evaluative* attitudes (good vs. bad). In measuring the communication of *non-evaluative* attitudes, *i.e.* potency and activity, the results were, however, less satisfactory, primarily because these dimensions were not independent of evaluation for the topics that were used. It is quite possible, of course, that the choice of other non-evaluative dimensions would have yielded more promising results. It may be that rather than sampling the factors that emerged from the Osgood and Suci analysis, the selection of non-evaluative scales should be guided by the factors that are found to be most important in determining the connotative meaning of the particular topic chosen for experimental use.

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[7] Kendall, *op. cit.,* 99-104.

APPARATUS

SKIN-DRILLING: A METHOD OF DIMINISHING GALVANIC SKIN-POTENTIALS

By BRIAN SHACKEL, Feltham, England

In the technique of electro-oculography the position and movements of the eyes are recorded by detecting the changes in DC potential upon the adjacent skin surface arising from the eyeball itself. Any other DC potentials generated in the body or skin, detected at the same time, seriously diminish the accuracy of recording. As part of a larger study on electro-oculography it was therefore found necessary to reduce these interfering potentials; indeed, it was required to find ways of converting the skin if possible into a completely inert, 'noise-free,' conducting medium.

Previous workers have shown that superficial scarification of the epidermis will eliminate diffusion-potentials and the electro-dermal response of Ebbecke[1] and that it will diminish skin-resistance and the electro-dermal response of Féré—the galvanic skin response (*GSR*) by the resistance method.[2] It is common practice in electro-encephalographic (*EEG*) and other electro-physiological recording to scrape the skin in some way to lower its source impedance and diminish the noise-level. The effect of breaching the skin on the electro-dermal response of Tarchanoff (the *GSR* by the potential method) does not seem, however, to have been thoroughly investigated.

Rothman describes the work of Rein which suggests that "by superficial scarification of the epidermis, the skin becomes an electrically neutral nonpolarizing medium, with free exchange of ions in all directions."[3] No quantitative data, however, are given relating to this effect, and it has not been possible to consult the original paper by Rein. Rothman also quotes, and in general seems to agree with, other workers who conclude that the source of such polarization-potentials is the area of the stratum lucidum and stratum granulosum, the boundary area between cornified and non-cornified epidermis.[4]

Thus it seems that eroding the outer epidermis and breaching this boundary layer, without invading the dermis and drawing blood, should give a significant improvement in the zero stability of recordings, in the isolation of wanted from unwanted signals, and in the reproducibility of results. Practical methods of breaching the skin to diminish diffusion-potentials and skin-resistance were therefore investigated; the effect of the final method adopted upon the electro-dermal response of Tarchanoff (*EDR–T*) was subsequently studied.

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* From the Psychological Research Laboratory, EMI Electronics Ltd., Feltham, Middlesex, England.

[1] S. Rothman, *Physiology and Biochemistry of the Skin*, 1954, 13.

[2] C. P. Richter, Physiological factors involved in the electrical resistance of the skin, *Amer. J. Physiol.*, 88, 1929, 596-615; Eric Farmer and E. G. Chambers, Concerning the use of the psycho-galvanic reflex in psychological experiments, *Brit. J. Psychol.*, 15, 1925, 237-254.

[3] Rothman, *op. cit.*, 13. [4] Rothman, *op. cit.*, 12, 27 and 32.

Sandpaper was the first abrasive tried. The scarification was hardly superficial; pain from the heat generated by the necessary friction was very considerable. Eventually a method was developed which erodes the skin over an area no larger than 1/16 in. in diameter, which is almost instantaneous, and which, above all, is completely painless.

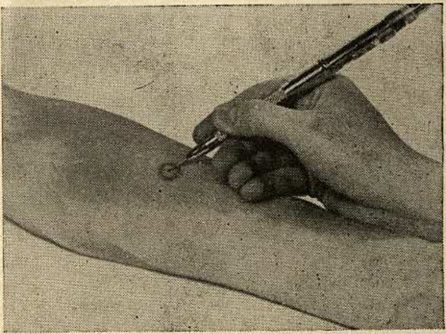

FIG. 1. METHOD OF HOLDING THE DRILL STEADY FOR ACCURATE WORK

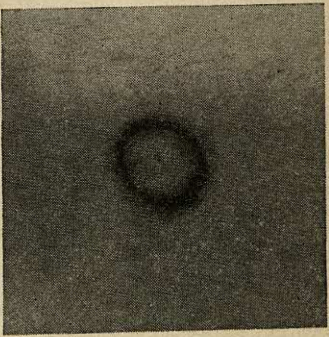

FIG. 2. A SATISFACTORY SKIN-DRILLED HOLE

Method. A spherical dental burr, covered with industrial diamonds (obtainable as Dica straight round burr No. 102), is held in a chuck or handpiece of a laboratory dental machine which rotates between 2000–5000 rpm.—(speed is not critical). The skin of the area to be 'drilled' is first cleaned with an antiseptic and then, with the handpiece held like a pen (see Fig. 1), the rotating burr is applied lightly to the skin three or four times until a shallow, circular depression is made in the epidermis. To ensure steady control, the operator should rest his hand near the site to be drilled.

The correct depth of the hole is indicated by the slight glistening of tissue and cell fluid in the depression. This is best tested by viewing the site in line between, and at equal angles with, the eye and a window or a bright light. If the drill hole is not deep enough, it reflects the same amount of light to the eye as the surrounding skin and so does not glisten. If it is deep enough, if the boundary layer of the epidermis has been reached, the stratum malpighi and the fluid now visible reflect significantly more light. Sometimes this indication is not easily seen, but one rapidly learns with a little practice the appearance of a satisfactorily drilled hole. About a dozen separate trials, some of them carried deliberately far enough to draw a spot of blood, should suffice to learn the technique.

The tiny mark left on the skin disappears very quickly. Indeed, it is advisable to mark the spot as shown in Fig. 2. Before applying electrodes the area is swabbed with acetone or alcohol; after the test is finished and the electrolyte jelly is removed, an antiseptic is again applied and a little lanolin cream is rubbed into the spot.

There are two criteria, apart from the visual inspection, which help confirm that the necessary depth has been reached: (1) Swabbing with acetone before applying the electrodes usually gives a slight tingling or stinging sensation; (2) when using accurate electrodes, a DC amplifier and recording system, the absolute potential difference between a pair of electrodes on properly drilled holes is seldom more than 3 mv. and is usually less than 1 mv.

It should be emphasized that the method is painless and that blood should not be drawn. For accurate DC-recording the absence of blood is important; it would be useless to have a method of stabilizing other potentials which itself caused troublesome concentration-potentials by diffusion of blood into the contacting electrodes.

Results: (1) Skin-resistance. 'Measurements of skin-resistance' were taken with an Avometer Model 8 using non-polarizing silver silver-chloride electrodes and 1% saline jelly electrolyte. Two areas on the upper inside forearm were thoroughly drilled first. Then measurements were taken against them of skin-areas at the wrist and on the palm. The wrist- and palm-areas were drilled and the measurements repeated. A similar test was made on the forehead. The results are given in Table I.

TABLE I

SKIN-RESISTANCE

(All readings in kilohms.)

Area	Before drilling	After drilling
Wrist	30– 60	2– 7
Palm	75–150	5–15
Forehead	12– 25	2– 5

(2) Electro-dermal response of Ebbecke (EDR–E). The electro-dermal response of Ebbecke is a potential difference of up to 10 mv. elicited by mechanical, thermal or electrical stimulation of the skin itself.[5] In the normal recording situation this 'noise' signal is most likely to arise from small movements by the *S* which wrinkle or displace the skin.

Two areas on the upper inside forearm were drilled first for the 'neutral' electrodes. Two types of rubber suction cup electrode were used, one with a long metal pole which actually presses on the skin along with the jelly when in place, and one with a short metal pole which contacts the body only via the saline jelly electrolyte.[6] One electrode of each type was placed on the drilled areas, the other, 'active,' electrode of each pair was placed on the wrist. The potentials from the skin were detected with a high input-impedance DC amplifier and recording system.[7]

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[5] Rothman, *op. cit.,* 21.

[6] Brian Shackel, A rubber suction cup surface electrode with high electrical stability, *J. appl. Physiol.,* 13, 1958, 153-158.

Two methods of eliciting the (*EDR—E*) potentials were used: (1) *E* pressed and released the 'active' electrodes at the wrist, 10 times, once every 2 sec. and with a force of about 8–12 oz.; (2) *S* rotated his wrist and forearm (pronation and supination) through an angle of about 90°.

After the potential-changes resulting from these stimuli had been recorded, the electrodes at the wrist were removd, the skin-areas were drilled, and the electrodes were replaced. The stimuli were repeated, special care being taken to ensure that the pressure exerted on the electrodes by *E* was not less than before.

Seven *S*s were used in the test, at least nine readings being taken from each test-electrode on each *S* under each condition. The results are given in Table II.

The skin-drilling technique will therefore give a threefold to fourfold improvement in stability and freedom from *EDR-E* 'noise' when detecting other types of potential from the body. Moreover, it is to be noted that the absolute potential generated by quite considerable displacement of the electrodes and limb, or by a moderate pressure upon the electrodes, seldom exceeds 100 μv. if the skin has been drilled.

TABLE II

POTENTIALS OF THE ELECTRO-DERMAL RESPONSES OF EBBECKE—*EDR-E*

N=at least 68 for every mean; all readings in millivolts.

	Variable pressure on electrodes				*S* rotates wrist			
	short electrodes		long electrodes		short electrodes		long electrodes	
	before drilling	after drilling	before drilling	after drilling	before drilling	after drilling	before drilling	after drilling
Mean	0.30	0.07	0.22	0.06	0.40	0.09	0.29	0.09
Ratio of means	4.3:1*		3.7:1†		4.4:1†		3.2:1†	

* Difference between means significant at $P = <0.02$.   † Difference between means significant at $P = <0.01$

The two types of electrode were used to see if one might be better than the other, but the differences between them are not significant here. For other reasons the electrode with the short metal pole is to be preferred for the accurate recording of DC potentials from the body.[8]

(3) Electro-dermal response of Tarchanoff—(EDR—T). It was not

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[7] Brian Shackel, R. C. Sloan and H. J. J. Warr, An eye movement measuring apparatus, *Electronics*, 31, 1958, 36-39.

[8] Shackel, *op. cit.*, 153-158.

initially expected that this skin drilling technique would diminish the *EDR-T* extensively. An anaesthetic method had already been proved satisfactory for this purpose,[9] and during the anaesthetic test it had been found that puncturing the palmar skin and administering intradermal injections of 0.5 cc. 1% NaCl solution did not greatly diminish the *EDR-T* detected by surface electrodes from the area. The effect on the palmar *EDR-T* of drilling the skin was tried, however, and the result was immediately encouraging and a full test was made.

Two areas on the inside upper forearm were first drilled and one of each pair of electrodes was placed there. The other of each pair was placed on the inner aspect of the palm. Ten signals were then given, five each intermingled of two types of stimulus, a deep breath held for a short while by *S*, and a loud bang from a wooden clapper not within *S*'s field of view. This first half of the test was to show that approximately the same potential response came from both palmar areas.

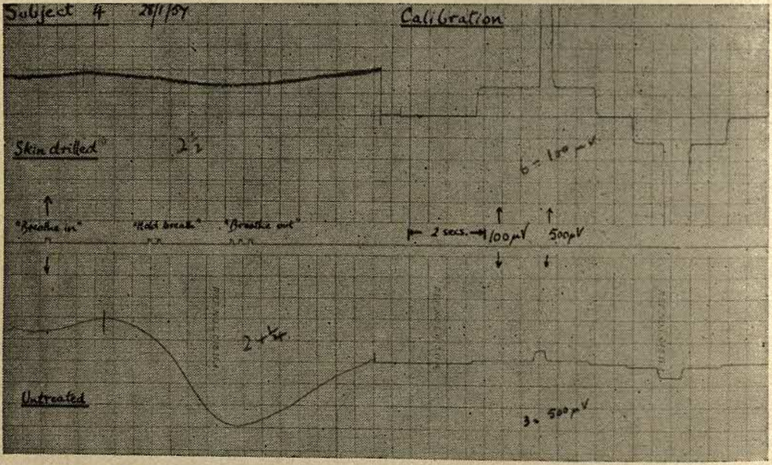

FIG. 3. A RECORD OF *EDR-T* FROM UNTREATED AND FROM SKIN-DRILLED PALMAR SKIN

One palmar area was then drilled and five signals of both types of stimulus were again given. From this second half of the test, to assess the effect of the skin-drill, we have the *EDR-T* potential from two areas, the one drilled and the other untreated, detected at the same instant in response to the same stimulus. An actual record is shown in Fig. 3, with an *EDR-T* of over 4 mv. from the untreated palm, but only 0.04 mv. from the drilled area. Four *S*s were used; the results are given in Table III.

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[9] Shackel, The effect of certain drugs upon the Galvanic Skin Response, *Brit. J. Derm.*, 70, 1958, 155-165.

Discussion. It is thought that the general discussion by Rothman on the diminution of diffusion potentials and the *EDR-E* may be extended and amplified to explain the diminution of the *EDR-T*. The hole made through the epidermis acts as a direct conducting pathway into the body; the saline jelly electrolyte 'short circuits' to the body via this hole the outside of the epidermis and the inside of the sweat ducts and sweat glands, over the whole area covered by the contacting electrode, and thus depolarizes the respective membranes.

The diminution of the *EDR-E*, 3:1 or 4:1, is relatively small compared with the 20:1 or so reduction in *EDR-T*. This may have been because there was only a very rough control of the stimulus-intensity in the *EDR-E* test. In varying the pressure of the electrodes on the areas drilled, *E* deliberately aimed to err on the heavy side, but it is not thought that an error to the extent of pressing five times harder can have occurred. Moreover, the results from *S*s' rotating their wrists 90° are in substantial agreement; the control on this stimulus also was not precise but again a fivefold discrepancy can hardly have occurred.

TABLE III

POTENTIALS OF THE ELECTRO-DERMAL RESPONSES OF TARCHANOFF—*EDR-T*

N=20 for every mean; all readings in millivolts.

	Both palmar areas untreated				One palmar area drilled			
	Deep breath		Noise		Deep breath		Noise	
	area 1 (normal)	area 2 (normal)	area 1 (normal)	area 2 (normal)	area 1 (normal)	area 2 (drilled)	area 1 (normal)	area 2 (drilled)
Mean	3.61	3.57	4.69	4.65	3.01	0.14	3.78	0.16
Ratio of means	1.01:1		1.01:1		21:1*		23:1†	

* Difference between means significant at $P=<0.01$. † Difference between means significant at $P=<0.001$

Alternatively there may be some difference in type or extent between the potentials detected in this test and the true *EDR-E*. Ebbecke and others record a potential change of the order of several millivolts lasting for 10 sec. or more in response to 'moderately intense' stimuli.[10] The potential changes recorded in our test were only in the region of ¼–½ mv. and were directly related to the displacement or change of pressure on the electrode. It is thought, nevertheless, that probably our potentials may be regarded as a form of *EDR-E* to a less intense stimulus. This would not explain, however, why the same diminution was not achieved by the skin-drill as with the *EDR-T*, particularly as Ebbecke found that his potential reaction was abolished by 'paralyzing' the epidemis with chloroform, which damages the epidermis and thus depolarizes it.[11]

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[10] Summarised in Rothman, *op. cit.*, 21.

Perhaps, therefore, what has been called *EDR-E* in our test may really be a combination of a skin-response and an electrode-response, and should be called an 'electrode disturbance' potential. The skin-response portion would be a small *EDR-E* and would be diminished greatly by the skin-drill. The electrode-response portion would depend on the physical behavior of the electrode system; if the chemical system of electrode and jelly electrolyte, which is in contact with the skin under a certain pressure, is sensitive to displacement or change of pressure, then it will produce the same order of potential in both parts of the *EDR-E* test for the same physical disturbance. If therefore what has here been called *EDR-E* potential were, in fact, made up of about 80% skin-response and about 20% electrode-response, and if the skin-response were reduced to 1/20 by skin-drilling, as the *EDR-T* is, but the electrode-response remained unchanged, the total response after skin-drilling would be about 24% of the original, which is the order of the potential actually recorded in the reduced response. Whatever the exact reason for the apparently less effective diminution of 'electrode disturbance' potentials, it is none the less noteworthy that for practical purposes, when investigating other types of potential, 'noise' generated by quite large movements of limb or electrode may be diminished at least to one third or one quarter by drilling the skin.

The value of this technique in the field of electro-oculography has been found to be considerable. Using the skin-drill, and the carefully prepared electrodes which are equally essential, it has been found possible to record the required DC-signals with no detectable interference from the skin, and with the average zero-drift from the electrodes about 50 µv. per min. By contrast it has been found, by analyzing the incidental results of zero drift during a survey on 120 *Ss*, that without the skin-drill the average zero-drift rate is more than 9 µv. per sec., equivalent to 550 µv. per min.

By means of this technique it is possible for workers investigating the *EDR-T* to ensure that they are recording the response from one area of the skin only, with hardly any contaminating voltage superimposed from the area of the neutral electrode, and without having to adopt unpleasant methods such as hypodermic needles for the neutral electrode.

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[11] Rothman, *op. cit.*, 21.

It is presumed that pain from the customary methods of diminishing 'skin-noise' occurs with a blunted syringe needle by direct stimulation of pain sensory endings and with sandpaper from the heat generated by friction. It is thought that the skin-drill is painless because of the rapid penetration by the high-speed cutting tool and the delicate control of the depth of erosion which can be achieved. If *S* experiences any pain at all, *E* is negligent in drilling far too deeply or by using a blunt drill. The technique has already been used satisfactorily and without difficulty on 20 schizophrenics by another investigator.[12]

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[12] P. H. Venables, personal communication from the Institute of Psychiatry, Maudsley Hospital, 1957.

NEW USES FOR THE OSCILLOSCOPE AS AN INSTRUMENT OF RESEARCH AND DEMONSTRATION

By Ralph H. Turner, Oberlin College

While the oscilloscope is commonly used in the psychology laboratory for obtaining visible representation of auditory stimuli, it can also be made to serve a number of other purposes. The standard oscilloscope with vertical and horizontal amplifiers and a sweep generator is readily available and comparatively low in price. A 5-in. screen or larger is desirable although not necessary. With this apparatus, the following phenomena may readily be demonstrated.

(1) The field-concept of movement as a resultant or vector of two simultaneously applied forces. The horizontal centering knob is turned to illustrate horizontal movement of the point-source of light as produced by one hand. The vertical centering knob is turned to illustrate movement in the vertical direction as produced by the other hand. Both knobs are turned simultaneously and at the same rate to illustrate a 45° diagonal movement as a vector or resultant of these two equal forces. Turning one knob faster than the other alters the angle of the resultant pathway and demonstrates movement under conditions where the component forces are simultaneous but unequal.

(2) A method for measuring bi-manual perceptual-motor coördination. With the horizontal centering knob held in one hand and the vertical centering knob in the other, the point-source of light may be made to follow any pathway if the knobs are properly coördinated. Paper can be taped over the oscilloscope-screen with a star pattern or other narrow pathway cut for the purpose. The length, shape, and complexity of the pathway can be varied and learning-curves based upon time, errors, or both, can be readily generated. By attaching handles to the knobs, the essential nature of the task can be changed from coördination of the fingers to coördination involving the arms.

(3) How sine waves are generated by rectilinear motion. With the point-source of light positioned near the left edge of the screen, the vertical centering knob is turned in rapid alternation to produce vertical rectilinear movement. Rotation of the horizontal centering knob 'sweeps' this rectilinear motion in a horizontal direction and produces a wave-pattern which can be made to approximate a sine curve with a little practice.

(4) How lower stimulus-thresholds are determined by psychophysical methods. If the knob controlling intensity has no pointer, it is replaced with one that has. A cardboard scale is so placed behind this pointer that zero represents the extreme counterclockwise position (no visible stimulus) and the highest scale reading corresponds to the extreme clockwise position (most intense stimulus). The intensity-control can be varied systematically to produce an alternating ascending and descending intensive series that includes subthreshold values.

(5) Changes in visual sensitivity or dark-adaptation as a function of time. Using the modification of the apparatus just described, the observer (*O*) adjusts the in-

tensity until the spot can just be seen and *E* takes a reading of the position of the knob. *O* must be thoroughly light-adapted for several minutes at a high level of illumination and determinations begun immediately in a completely darkened room. With practice, five determinations can be made each minute.

(6) The greater sensitivity of the peripheral retina. *O* fixates a mark near the center of the scope and so adjusts intensity that he can just see the spot peripherally. The centering knob is then used to sweep the dot slowly through the fixation-point and out the other side. The spot will disappear as it enters the fixated area and appear again as it emerges from this area.

(7) Mapping the blind spot. *O* views with his right eye a fixation-point at the edge of the tube at 10 o'clock from a distance of 9 in. Tissue paper is taped directly on the glass screen, and *O* slowly moves the spot to the right until it just disappears. *E* marks the location on the tissue paper. *O* then continues turning the knob in the same direction until the spot emerges from the blind area and backs it up until it just disappears. The position is again marked. The spot is then moved up or down one eighth of an inch and the horizontal scanning repeated, the process being continued until the map of the blind spot is complete. *O* can plot his own blind spot by alternating the role of *E* and *O*.

(8) A dynamic reversible perspective figure. The vertical input is connected to the 60 ~ terminal and the sweep generator turned on. Synchronization is placed in the internal position, the vertical input switch in the high position, the sweep frequency-selector at about 1000 or 2000 and the vertical and horizontal gain adjusted so that a pattern about 2 × 2 in. in outline is produced. The frequency-vernier is adjusted until a stable pattern is obtained composed of a series of almost horizontal lines, half of which are slightly tilted to the right and half to the left. The frequency-vernier is then so adjusted that these lines move in a vertical direction and give the impression of a very slowly rolling cylinder composed of approximately 30 lines. By turning the synchronization control to the proper position, some lines can be made to project horizontally beyond the others and the cylinder can then be seen as having an open right end pointed slightly toward or away from *O*. This constitutes a reversible perspective figure in that the open right end of this slowly rotating cylinder sometimes appears pointed toward *O* and sometimes away from *O*. It constitutes a *dynamic* reversible perspective figure because the cylinder rolls toward or away from *O* depending upon whether the open end of the cylinder is seen as pointed toward him or away from him.

(9) An 'electronic kinephantoscope' which produces a moving ambiguous figure. The output of an audio generator producing 55–65 sine waves per second is connected to the vertical input of the scope. The horizontal input terminal is connected to the 60 ~ terminal. The audio generator-frequency is shifted slowly in the vicinity of 60 ~ until a single circular pattern is produced. The audio generator-frequency should be so carefully adjusted that the figure changes slowly from a perfect circle to an ellipse to a straight line and repeats the process along an axis displaced 90° from that of the previous figure. This simple movement can be interpreted in over a dozen different ways.

(10) The phi phenomenon. All connections and settings are identical to that just described and the same figure should be obtained. As the ellipse flattens into a line, a bright spot is momentarily seen at the end of this line in the upper right hand portion of the screen and a moment later a similar spot is seen at the end of another line in the upper left hand portion of the screen. With small changes in the setting

of the frequency-generator, the frequency may be smoothly increased to a value beyond that needed to demonstrate the phi phenomenon before a point is reached where the position of the bright spots changes. A cardboard disk is fitted over the scope screen with holes ⅛ in. in diameter punched in it to coincide with these two bright spots. It is convenient to prepare a series of cardboard disks, each with a pair of holes punched at different distances of separation. By using a different disk for each experimental series *E* can control the distance of separation and, to a limited extent, the size of the stimulus-circles. The scope can then provide a demonstration of the relevance of time, intensity, distance, and stimulus-area as variables in the phi phenomenon.

(11) After imagery as a function of stimulus-intensity. With the sweep generator on, the frequency-selector and frequency-vernier are so adjusted that a thin horizontal line is swept out at a frequency of approximately 250 ~ per second. A bit of white adhesive is placed on the edge of the screen at 12 o'clock and another at 6 o'clock so that the eye will sweep vertically through the center of the scope line when they are used as successive fixation-points. At a distance of 2 ft., *O* alternately fixates the top and bottom fixation-points without blinking as the fixation-point is changed. A series of positive after-images consisting of lines of the same apparent width as the stimulus-line will be generated. The intensity of these after-images can be varied by changing the setting of the intensity-control. At low settings a point will be found where the stimulus-line is still plainly visible but no positive after-image can be obtained.

(12) A stroboscope for 'stopping' repetitive movement. With the horizontal gain set to produce a line approximately 3 in. in horizontal width and the intensity-control at its highest setting, the frequency-selector and frequency-vernier are set at their lowest values. A disk of completely opaque cardboard with an aperture ¼ in. in diameter is taped directly on the glass surface of the screen over this line so that a flickering light source is produced. A rapidly rotating object, such as an electric fan, is brought within a foot of the scope in a dark room and viewed from the position of the scope. By turning the frequency-vernier slowly the fan blades can be brought to apparent rest.

(13) Critical fusion-frequency as a function of intensity. Use scope settings and an aperture as just described. At a given setting of the intensity-knob *O* fixates the aperture as the frequency-vernier is slowly advanced from its lowest position to a position at which he reports no flicker. The reading of the frequency-vernier is noted and compared with readings taken under different levels of intensity. Using the method of limits, comparisons can also be made between the critical fusion-frequencies of several *O*s. To demonstrate that the fusion is truly retinal and not due to the persistence of the scope-screen, use the scope for stroboscopic purposes as just described.

It is evident from this list that the oscilloscope is a valuable apparatus, particularly for laboratories with small budgets. It replaces many specialized pieces of apparatus which in the aggregate cost many times more than it. Most of the usages listed are clearly appropriate for demonstration purposes. It should also be apparent that when it is carefully calibrated, the instrument may be used in doing a considerable amount of research.

APPARATUS NOTES

A PHOTOELECTRIC METHOD FOR INTEGRATING MUSCLE-ACTION POTENTIALS

A method of integrating muscle-action potentials recorded with an *EEG* pen-writer has been developed. The basic principle consists of passing light through a given segment of the ink-tracing and measuring with a sensitive photometer the amount of blockage caused by the tracing.

The photoelectric unit used in this laboratory consists of a sensitive microammeter to which is attached a silicon photoelectric cell 2 cm. in width and 5 cm. in length. A trapezoidal prism (cut from a 2-in. thick slab of lucite) is mounted to the face of the photoelectric cell to increase the amount of lighted area to which the cell responds. The top of the prism has the same dimension as the cell; the base is 11 cm. in length and 6 cm. wide; and the depth is 5.5 cm.

The photoelectric unit is hinged to the top rear edge of a 1-ft. cube wooden box which has a 5 × 10-cm. aperture in the center of the top. The base of the lucite prism, which is bordered with a 5-mm. strip of wool felt, fits snugly over the aperture when the hinge is in the horizontal position. The sides of the prism are covered with electrical tape to prevent light entering other than through the base.

A 150-w. flood lamp, powered by commercial 110-v., 60 cycle AC, is mounted in the bottom center of the box. The current is stabilized with a constant voltage transformer, and light intensity is controlled by a continuously variable rheostat. A variable density filter, made of differing layers of onion skin paper, is mounted across the aperture in top of the box to make the intensity of the light per unit area passing through the aperture approximately homogeneous.

The integrative procedure consists of centering a segment of the *EMG* tracing over the aperture in top of the box, lowering the photoelectric unit into position, and reading in arbitrary units from the dial of the microammeter the amount of light blockage produced by the ink tracing. The instrument is 'zeroed' by passing light through a piece of *EEG* paper which contains no ink tracing and adjusting the intensity of the light source until the microammeter needle rests at the arbitrary zero position.

Since the length of the light aperture is a constant (10 cm.) the time-interval over which an integration occurs is determined by the speed with which the *EEG* paper moves during the recording of unintegrated muscle-action potentials. Thus, paper speeds of ½, 1, and 2.5 cm. per sec. will result in integration-periods of 20, 10, and 4 sec., respectively. A variety of integration-periods may be obtained by making a number of lucite prisms with bases of different length, and by making an adjustable light-aperture.

The instrument has been subjected to a number of evaluative tests. A comparison of the deflections of a microammeter to known amounts of aperture-blockage revealed that the results are linear. Product-moment correlations based on 15 different samples of original and repeated integrations (each sample containing from 10 to 25 measures) of muscle-action potentials indicated that the instrument is highly reliable. The *r*s ranged from 0.945 to 0.997. Photoelectric integrations of potentials recorded at three different paper speeds (½, 1, and 2.5 cm. per sec.) were correlated with electronic integrations of the same potentials, and found to be 0.95, 0.95, and 0.91, respectively. These high correlations indicate that the photoelectric method of integration is as valid as electronic pulse-frequency integration.

The advantages of the photoelectric integrator are to be found in its simplicity of design and relatively low cost of construction and operation. Furthermore, the principle could be applied as readily to film recordings of *EMG* activity as to paper tape recordings.

A word of precaution is in order for those who might wish to integrate ink recordings of *EMG* activity photoelectrically. For the procedure to be valid the pen writer must give forth a constant flow of ink at all times.

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NOTES AND DISCUSSIONS

CRITIQUE ON THE INTERPRETATION OF CULTURAL DIFFERENCES IN THE PERCEPTION OF MOTION IN AMES'S TRAPEZOIDAL WINDOW

In a recent paper,[1] Allport and Pettigrew, using Zulus as observers (*O*s), present the results of cultural differences in the perception of the Ames rotating trapezoidal window,[2] which bear on the nativism-empiricism controversy in general and the transactional theory in particular.

The trapezoid, a cut-out figure of a perspective drawing of a window, is rotated on an axis parallel to and between the parallel ends. The difference in height between the parallel ends of the 'window' is so great that it is never compensated by the actual perspective of the figure when it is viewed from a distance. Thus the short end is always shorter on the retina, and the long end is always longer, no matter which is in reality the nearer. The perceiver does not see rotation; he perceives the movement as oscillatory. The apparent oscillation does not, however, pertain to objects attached to the window; cues, such as interposition, seem to determine the perceived motion of appended cubes or rods which continue to rotate as the window oscillates, thus providing eerie effects for half of the cycle.

Ames's trapezoid is an excellent apparatus to use since the illusion is all-or-none; one either sees it or one does not. There are, furthermore, a number of reliable behavior-indices which correlate with the perception of the illusion—laughter and expressions of surprise, etc.—which point to a perceptual experience rather than merely to a response.

The differences in perception predicted from culture were such that the perceptions of 'civilized' people would be less veridical than those of primitive people. The results, then, cannot be accounted for on the basis of the superiority of Western man at his own task. Since those are two of the most frequent criticisms of studies of this kind, and since the Allport-Pettigrew experiment is carefully controlled for these and other artifacts, it demands our most serious consideration.

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[1] G. W. Allport, and T. F. Pettigrew. Cultural influence on the perception of movement: The trapezoidal illusion among Zulus, *J. abnorm. soc. Psychol.*, 55, 1957, 104-113.

[2] Adelbert Ames, Jr., Visual perception and the rotating trapezoidal window, *Psychol. Monogr.*, 65, 1951, (Whole No. 324), 1-32.

Before criticizing the Allport-Pettigrew article, let me clearly state: (1) I have no quarrel with the design, execution, or presentation of their results. The flaws in the experiment are minor and few. My quarrel is with the discussion and conclusion rather than with the experiment. (2) It is as much the general impression given by the article with which I disagree as it is any specific mistake of conclusion (although I do not wish to underestimate the importance of the latter). Allport and Pettigrew leave the reader feeling that their study—which has direct bearing on the nativism-empiricism controversy—does not support one side or the other, when actually their findings are inconsistent with the nativistic position. (3) I wish my efforts to be viewed not as a destructive critique of a poor experiment, but as *quite the opposite;* namely, a constructive reëvaluation of an exceedingly well-done experiment.

The errors, it appears, stem from a misunderstanding of the nature of the illusion with which they are dealing. Allport and Pettigrew believe that they are studying the perception of movement, whereas actually movement is not the critical effect of the illusion. Shape and size are the important variables here, since, in some sense, the illusion can be considered as achieved when *O* perceives the trapezoid as more rectangular than it *is, i.e.* more rectangular than he would perceive it if he were right up close to it and, therefore, at a different angle with respect to his line of sight. Although the *O*s respond by telling how the window moves, the illusion is essentially the same if they respond by telling us how it *lies*. The movement-effects are more dramatic but they are not theoretically critical. The effects of shape and size, which cause the apparent movement, *are* theoretically critical. From a strong empiricistic point of view we would be able to predict whether an *O* will get the illusion by knowing the *relative weight of his experience with rectangular objects as over and against his experience with trapezoidal objects.* Actually the experience is a matter of correlating trapezoidal retinal images with manipulative experience, leading to rectangular conclusions. Experience with other shapes, such as circles, would be important only as it transferred to or precluded experience with rectangles and trapezoids.

For the sake of argument, let me describe three mythical *O*s (*A, B,* and *C*), and predict the behavior of each in the Allport-Pettigrew experiment. *A* is a typical Western man who has had more weight of experience with rectangles than with circles, and has had very little experience at all with trapezoids. *B* (a 'circular' man) is an hypothetical Zulu who has had more weight of experience with circular shapes than with rectangles but who, like *A*, has had even less experience with trapezoids. The most important thing about *B* is that whatever his experience with circles may be, he has had less experience with rectangles than *A*. *C* (a 'trapezoidal' man) comes from Mars, where he lives in a world of obtuse and acute angles exactly equal to those on the Ames window. Nothing is normal, in the sense of lines perpendicular to planes; everything is 'cockeyed.' Whether he, like Piet Mondrian, abhors circles or not is irrelevant. The important thing is, unlike Mondrian, he definitely does not prefer rectangles. Let us now predict the 'perceptions,' and hence many aspects of the behavior, of these three *O*s under optimal and marginal conditions of the Allport-Pettigrew experiment. Under optimal conditions (monocular viewing at 20 ft.), *A* and *B* will perceive the illusion, *C* will not. Since *C* prefers trapezoids, he will even see a rectangle as a trapezoid. *A* and *B* will, however, be indistinguishable at this point, since the variable on which they differ is circularity vs. rectangularity, but the critical illusion is one of rectangularity vs. trapezoidy.

It should be noted that the meaning of optimal conditions is that set of conditions, which in this case happens to be monocular vision at 20 ft., where the uncontrolled cues which would tend the perceiver toward seeing a trapezoid are at a minimum. In other words, the number of 'give-away' cues at this distance, with one eye, is so small that it does not present a source of conflict disturbing to the rectangular perception of either *A* or *B*, even though *B* has had less experience with rectangles than *A*.

Under marginal conditions (binocular vision and monocular vision at 10 ft.) we would expect quite a different state of affairs. Marginal conditions are those in which the above-mentioned 'cues to trapezoidy' are so strong that even *A* wavers a bit in his perception of rectangularity. It is at this point that differences between *A* and *B* obtain, since even very slight differences in the weight of their respective experiences will play a part in deciding which way the whole perception will go. Since *B* has presumably less of a differential between trapezoidy and rectangularity experience than *A*, he should perceive less illusion at this point than *A*. *C*, of course, will continue to see a trapezoid and hence no illusion at all, although if *C* were presented with a rotating rectangle under these conditions his perceptions should waver a bit between rectangularity and trapezoidy.

The use of marginal conditions as an experimental device for demonstrating the influence of factors such as familiarity and emotional loading has actually been prescribed as an experimental technique by Ittelson and Slack, who, representing the strong empiricistic point of view, nevertheless do not expect to find individual differences at every combination of parameters in any perceptual phenomenon.[3]

If we could invent an illusion, the perception of which did not rest on

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If we could invent an illusion, the perception of which did not rest on the assumption of rectangularity vs. the assumption of trapezoidy, but rather on the assumption of circularity vs. the assumption of rectangularity, then for such an apparatus we would predict the results which Allport and Pettigrew expected from the strong empiricistic theory; namely, that the Zulus would differ from Western man under marginal *and* optimal conditions. Notice that in none of the above do we make a distinction in the way in which the assumptions, be they of circularity, rectangularity, or whatever, are learned by our *O*s, nor are we able to distinguish between the influence of our experimental stimulus-object 'as a whole' and the specific influence of the parts of that object; thus we cannot tell whether our *O*s prefer windows to non-windows, trapezoids to rectangles, or right angles to other types of angles. All of these factors are confounded. The experiment can, therefore, only illuminate the part played by experience, whether it be experience in terms of meaning or experience in terms of cumulative habit, as opposed to the part played by nativistic or non-experiential determinants. Regarding this point, Allport and Pettigrew conclude:

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[3] W. H. Ittelson, and C. W. Slack, The perception of persons as visual objects, in Renato Tagiuri and Luigi Petroullo's (eds.) *Person Perception*, 1958, 310-328.

> Our major result is clearly not compatible with a narrowly conceived object-connotation theory. It is not necessary for the *S* consciously to assume that the object is a window in order to experience the illusion. True, as Table 6 suggests, the specific object connotation seems somewhat to favor the illusion, but it is clearly not the decisive determinant. Thus, for example, 88 percentage of those who did not consciously recognize the frame as a 'window' nevertheless experienced the illusion at 20′ monocularly.[4]

In other words, since no differences were obtained between groups under optimal conditions, and since presumably the Zulus have had no experience with windows, it is assumed that 'windowness' *per se* is not a contributing factor at this distance.

Now even the most narrowly conceived objective theory must leave room for *O*'s connotations. To test the narrowly conceived objective theory we must know what *O* thought the object was. If not a window, then what? No data were obtained on this point. Again, as long as the connoted object is more rectangular than trapezoidal we predict the illusory percept. Thus, Allport and Pettigrew's results cannot rule out the objective, connotative theory under *any* circumstances, marginal or optimal. They conclude their paper, however, as follows:

> The perception of motion as represented in the rotating trapezoidal window is governed, under *optimal* conditions, by nativistic determinants or by the unconscious utilization of residual (but not immediately relevant) experience, or by both. (Our experiment does not enable us to decide this issue.) At the same time, object connotation (meaning) based on closely relevant cultural experience helps to determine

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[4] Allport and Pettigrew, *op. cit.*, 112.

the nature of the perceived movement under *marginal* conditions. An adequate theory of perceived movement must therefore allow a place for the subject's specific assumptions of meaning even though it cannot be based solely on this foundation.[5]

These paragraphs sum up the major errors in Allport and Pettigrew's interpretation of their results. In the first place, their results cannot be generalized to other conditions of perceived movement, since their experimental situation is not basically motional. Their results can be more easily generalized to other perceived shapes and sizes (and to movement based on change in shape and size).

Their experiment does prove that specific assumptions of meaning, or unconscious habit preference, or some other experiential influences, must have a place in the theory of perception. The main point of this critique is, however, that contrary to what Allport and Pettigrew state, their experiment sheds no light on whether or not the eclectic position is correct. *The results of their experiment are perfectly consistent with the strong empiricistic position which gives little or no weight to nativistic factors.* Empiricism would predict no difference between the special groups studied under optimal conditions, but would predict a large difference under marginal conditions. This conclusion is derived from an understanding of the relations between the basic differential of the illusion (rectangle vs. trapezoid) and the basic culture-differential (less rectangularity vs. more rectangularity) as they interact with the presence of 'give-away' cues. The most important conclusion from the study, however, is that the strong nativistic position is no longer tenable. Experience must be considered a major variable in the perception of shape.

Harvard University CHARLES W. SLACK

JAN SWAMMERDAM AND THE HISTORY OF REFLEX ACTION

The Dutch naturalist Jan Swammerdam (1637-1680) might well be considered the most important figure in the seventeenth century history of the study of reflex action; yet his major works did not appear in print until nearly 60 years after his death, and his findings seem to have had little influence upon subsequent thought. The man responsible for the publication of his works, Hermann Boerhaave, refused to accept some of his findings. Even today, the major histories of psychology make no mention of him and he receives but a single brief paragraph in Boring's history of sensation and perception.[1] Full credit for bringing Swammerdam to the attention of modern students of reflex action belongs to Fearing.[2]

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[5] *Ibid.*, 113.

The purpose of this note is to outline Swammerdam's experimental and conceptual contributions in the history of reflex action and to fill in the gaps in the available literature.

Swammerdam's researches are described in his *Biblia Naturae,* published in 1737 in both Dutch and Latin by Boerhaave. The pertinent section, in the English translation of 1758,[3] is entitled "Experiments on the particular motion of the muscles in frogs, which may be also in general applied to all the motions of the muscles in men and brutes." In this essay is found a discussion of experiments utilizing the nerve-muscle preparation and disproving Descartes' doctrine of animal spirits.[4] In addition, a description of *tonus,* the differentiation of sensory and motor nerves, and views on the nature of voluntary action are found. It is indeed unfortunate that someone else is given credit for nearly every one of these contributions. They will be discussed in turn.

First, a brief outline of the Cartesian background, against which Swammerdam worked, is in order. It was Descartes (1596-1650) who put forth the hypothesis of animal spirits to explain reflex action. According to this doctrine, involuntary behavior was due to the operation of animal spirits, which were "nothing but material bodies and their one peculiarity is that they are bodies of extreme minuteness and that they move very quickly like the particles of the flame which issues from a torch."[5] All nerves were conceived as hollow tubes containing threads, both tube and thread extending from brain cavities (probably the ventricles) to the skin and flesh. These threads terminated on valves along the walls of the brain cavities. The animal spirits were produced in the brain and 'stored' in the cavities. When external stimuli impinge upon the sensory receptors, they cause the threads to pull, opening the valves. With the valves thus opened, the animal spirits flow outward toward the muscles, inflating them. In summary, muscular contraction consisted in the inflation of the muscle by corporeal bodies having as their immediate source, the brain. Two major implications of this doctrine are that muscular contraction represents an increase in bulk of the muscle in question, and that for contraction to occur the muscle must be functionally connected to the brain.

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[1] E. G. Boring, *Sensation and Perception in the History of Experimental Psychology,* 1942, 55.

[2] Franklin Fearing, Jan Swammerdam: A study in the history of comparative and physiological psychology of the seventeenth century, this JOURNAL, 41, 1929, 442-455; *Reflex Action: A Study in the History of Physiological Psychology,* 1930, 41-53.

[3] Jan Swammerdam, *The Book of Nature, or the History of Insects: Reduced to Distinct Classes, Confirmed by Particular Instances, Displayed in the Anatomical Analysis of Many Species,* With life of the author by Hermann Boerhaave, translated from the Dutch and Latin original editions by Th. Flloyd, revised and improved by notes from Reaumur and others, by John Hill, 1758, 122-132.

[4] René Descartes, The passions of the soul (1649), in *The Philosophical Works of Descartes rendered into English,* by E. S. Haldane and G. R. T. Ross, 1, 1931, 331-427; *Traite de l'homme* (1662), excerpt in *The Philosophy of Descartes in Extracts from his Writings,* selected and translated by H. A. P. Torrey, 1892, 275-280.

[5] Descartes, *op. cit.,* 1649, 335.

It was against this background that Swammerdam performed his experiments. For his investigations of motor action, Swammerdam chose the frog due probably to the conspicuousness and accessibility of its nerves. His experiments with both the heart and muscles of the thigh (probably *M. semitendinosis*) yielded excellent descriptions of muscular action following stimulation of the attached nerve. and gave conclusive evidence against the concept of animal spirits. Swammerdam went through a series of experiments, each more refined and giving more information than the preceding. He described the first stage as follows:

> Another very delicate and useful experiment may be made, if one of the largest muscles be separated from the thigh of a Frog, and, together with its adherent nerve, prepared in such a manner as to remain unhurt. For if, after this, you take hold of each tendon with your hand, and then irritate the propending nerve with scissors, or any other instrument, the muscle will recover its former motion, which it had lost. You will see that it is immediately contracted and draws together, as it were, both the hands, which hold the tendons.[6]

This experiment was a demonstration that such a technique would produce contraction. His next experiment showed that decreases in length of the muscle accompanied increases in girth.[7] Then come experiments showing that the result of contraction is *not* an increase in bulk or volume of the muscle. This was demonstrated with both heart and thigh.[8]

> The experiment utilizing the thigh muscle was as follows. A glass cylinder, open at the bottom and closed at the top except for a narrow tube extending above, was used. The muscle was placed in the cylinder and its nerve attached to a wire, the wire leading out of the bottom of the cylinder and passing through a piston beneath the muscle. The piston sealed off the inside of the cylinder, and thus the muscle. Next, a drop of water was placed in the tube, completely sealing off the muscle from the outside. A 'gentle pull' on the string produced contraction of the muscle. Concurrent with the contraction, the level of the drop of water fell in the tube, indicating that the bulk of the muscle had, if anything, decreased, but certainly not increased. There could be no animal spirits entering the muscle.

Several pertinent quotations will summarize Swammerdam's conclusions drawn from these experiments. Regarding the concept of animal spirits, he wrote:

> But all these opinions are certainly destroyed, if it be considered, how often the motion of each muscle is restored by only stimulating, provoking, or irritating the nerve in my experiment before mentioned: and this, when the nerve hath for a long time been cut off, and the requisite animal spirits dissipated, or grown weak, after many times discharging their duty; and when there is no further communication between the nerve, brain and marrow. Therefore I would have it seriously con-

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[6] Swammerdam, *op. cit.*, 123. [7] *Idem.* [8] *Ibid.*, 126 f.

> sidered, that it cannot be demonstrated by any experiments, that any manner of sensible or comprehensible bulk flows through the nerves into the muscles. Nor does anything else pass through the nerves to the muscles; all is a very quick kind of motion, which is indeed so rapid, that it may be properly called instantaneous.[9]
>
> Add to these another argument of yet greater weight, which is, that the muscles themselves, when contracting, are not in the least inflated or swollen, but rather they lose their thickness; though the moving fibres in the meantime acquire a different situation; or, to express the matter more exactly, they are pressed closer to each other.
>
> From these experiments therefore, it may, I think, be fairly concluded, that a simple and natural motion or irritation of the nerve alone is necessary to produce muscular motion, whether it has its origin in the brain, or in the marrow, or elsewhere.[10]

Francis Glisson (1597-1677) is usually given credit for demonstrating that the bulk of a muscle does not increase during contraction, but his research actually came at a later date than Swammerdam's, and was done with an inferior method.[11] Glisson had a "strong, muscular man" place his arm in a tub of water and then alternately relax and contract it. Upon contraction, the water level did not rise as demanded by the animal spirits hypothesis, rather, it dropped.[12]

Swammerdam's lesser contributions, lesser only by comparison with his treatment of animal spirits, may be briefly summarized. On the subject of *tonus,* Swammerdam has the following to say:

> That all the muscles are naturally contracted; that is, they are in a state of contraction before they have performed any motion. . . . We must add to the causes already assigned, the perpetual and natural irritations which the nerves are constantly exciting in the moving fibres of the muscles themselves, and which are continually engaged in urging them to contraction.[13]

Not only did Swammerdam describe *tonus,* he offered a possible explanation, thus allying his description with what today is known as *neurogenic tonus.*

Descartes, Thomas Willis,[14] and Albrecht von Haller[15] held the view that the same nerves possessed both sensory and motor functions. Galen, Kinelm Digby, Robert Whytt, and possibly Descartes, had postulated the separate existence of sensory and motor nerves.[16] It remained, however, for Swammerdam to demonstrate experimentally the necessity of this distinction:

> We very clearly also find by experiments, that the motion produced in the muscle by irritating the nerve, is always propagated out of the larger into the smaller

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[9] *Ibid.,* 124. [10] *Idem.*

[11] Swammerdam is given full credit, however, both as to priority and superiority of technique, by J. F. Fulton, *Muscular Contraction and the Reflex Control of Movement,* 1926.

[12] Fearing, *op. cit.,* 1930, 37-38. [13] Swammerdam, *op. cit.,* 130.

[14] G. S. Brett, *A History of Psychology: Medieval and Early Modern,* 2, 1921, 191.

[15] Fearing, *op. cit.,* 1930, 74.

[16] Fulton, *Physiology of the Nervous System,* 3rd. ed., 1951, 18.

branches, and goes outwards continually descending. The nerves designed for the senses are circumstanced in a quite different manner; for in these, the sensitive motions, doubtless, tend upwards.[17]

Thus, although Swammerdam did not point to the dorsal roots as sensory and the ventral roots as motor, he preceded Magendie in demonstrating that the nerves involved are distinct in their function.

Finally, Swammerdam made a very explicit statement regarding the relation of voluntary to involuntary movement:

> I would have it particularly observed, that I admit no essential difference between the natural and spontaneous contraction of the muscles, and that performed by the will. I consider this difference as merely accidental; but, because we move all these muscles which we move voluntarily, in a contrary direction; that which is said to be essential in the contraction of all muscles, is a natural contraction.[18]

Descartes, of course, believed in two general types of behavior, voluntary and involuntary (or reflexive). Swammerdam rejected this dichotomy, asserting that all movement is involuntary, or "natural." Thus he was more mechanistic in his approach than was Descartes. His position was completely deterministic, and thus precedes LaMettrie in the eighteenth and Claude Bernard in the nineteenth century.

Swammerdam was far ahead of his contemporaries, both in conceptualization and in experimental method. With the following statement by Fearing, the present writer wholeheartedly agrees: "Against a background of uncertain speculation and vague generalization concerning the nature of nervous control, the cautious statement and quantitative experiments of Swammerdam reveal a brilliance which is equalled by but few of his contemporaries."[19] The course of the history of physiological psychology would undoubtedly have been different, and its development more rapid, if only Swammerdam's experimental findings and conceptual interpretations had been known and heeded by his contemporaries and successors.

University of Miami, Florida BENJAMIN H. PUBOLS, JR.

SPATIAL GENERALIZATION AND PROBABILITY-LEARNING IN A FIVE-CHOICE SITUATION*

Recently a number of investigators have been concerned with certain phenomena which appear under conditions where *S*s are required to predict on each of *t* trials which event of *n* possible events will occur.[1] Most of the data have been obtained from two-choice situations ($n = 2$). An example of such a situation would be where *S* is confronted by a panel on which are mounted two lights and two corresponding response-keys. One light comes on 70% of the time, while the other is presented the remaining 30% of the time. The lights occur in a random sequence and *S* must, on each trial, press the key which corresponds to the light which he predicts is about to be presented. A common finding in such two-choice probability-learning tasks has been that *S*s reach response-levels which correspond to the relative frequency of occurrence of each light. In the 70-30 case, the *S*s would be responding at asymptote 70% of the time to the 70% light and 30% of the time to the 30% light. These findings are deducible from a theoretical model which has been described by Estes.[2] When the number of choices was varied from two to eight, however, Gardner found that the most frequently presented stimulus was responded to more often and that the least frequently presented stimuli were responded to less often than predicted by the model, the amount of discrepancy increasing as a function of the number of response-alternatives.[3]

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[17] Swammerdam, *op. cit.*, 125. [18] *Idem.*

[19] Fearing, *op. cit.*, 1930, 66.

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This discrepancy between the Estes model and the Gardner findings in multiple-choice situations requires confirmation. The experimental design described here was chosen because it also made possible a test for the effects of spatial generalization, a phenomenon which has been demonstrated in situations somewhat similar to that of the task of probability-learning.[4]

Twenty men and 20 women, members of the laboratory staff, served as

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[1] M. H. Detambel, A test of a model for multiple-choice behavior, *J. exp. Psychol.*, 49, 1955, 97-104; W. K. Estes, Theory of learning with constant, variable, or contingent probabilities of reinforcement, *Psychometrika*, 22, 1957, 113-132; W. K. Estes and J. H. Straughan, Analysis of a verbal conditioning situation in terms of a statistical learning theory, *J. exp. Psychol.*, 47, 1954, 225-234; R. A. Gardner, Perception of relative frequency as a function of the number of stimulus and response categories, *U.S. Army Medical Research Laboratory, Report No. 328*, 1958; D. A. Grant, H. W. Hake, and J. P. Hornseth, Acquisition and extinction of a verbal conditioned response with differing percentages of reinforcement, *J. exp. Psychol.*, 42, 1951, 1-5; Edith Neimark, Effects of type of nonreinforcement and number of alternative responses in two verbal conditioning situations, *ibid.*, 52, 1956, 209-220.

[2] Estes, *op. cit.*, 116. [3] Gardner, *op. cit.*, 14.

[4] J. S. Brown, E. A. Bilodeau, and M. R. Baron, Bidirectional gradients in the strength of a voluntary response to stimuli on a visual-spatial dimension, *J. exp. Psychol.*, 41, 1951, 52-61; J. S. Brown, F. R. Clarke, and L. Stein, A new technique for studying spatial generalization with voluntary responses, *ibid.*, 55, 1958, 359-362; S. A. Mednick, Distortion in the gradient of stimulus-generalization related to cortical brain damage and schizophrenia, *J. abn. soc. Psychol.*, 51, 1955, 536-542; S. A. Mednick and Laura E. Lehtinen, Stimulus generalization as a function of age in children, *J. exp. Psychol.*, 53, 1957, 180-183.

Ss. None had had previous experience with tasks in probability-learning.

S sat directly in front of a panel on which were mounted five small red lights arranged in a horizontal row at about eye-level. Above the middle light was a larger light which served as a ready-signal and beneath each small red light was a response-key. Each time the ready-light appeared, *S* had to press the key which corresponded to the light which he predicted would be presented next. He was informed that the lights would appear in a random order but that some might occur more often than others. Following his response, the correct light was exposed for a period of 1 sec. This light then went off and ½ sec. later the ready-light reappeared to indicate the beginning of a new trial. Two hundred such trials were given. In each block of 20 trials, one of the lights was presented 60% of the time, while each of the four remaining lights was presented 10% of the time. The light-sequence was random in each trial-block, with the 60% light being presented on the extreme left for one group of *Ss* and on the extreme right for another. The light-sequence was programmed by the use of punch-paper tape while all time-intervals were controlled by means of two Hunter Decade Interval-Timers. At the termination of each trial-block, the number of responses to each light was read from one of five counters.

It follows from the Estes model that the proportion of responses to the 10 and 60% lights will reach asymptotic levels of 10 and 60% respectively. If the Gardner findings are confirmed, response-proportions of less than 10% and greater than 60% will be obtained. Spatial generalization-effects, if they should exist, will be revealed by a tendency for *Ss* to respond to the 10% lights in proportion to their spatial proximity to the 60% light.

As trials progressed, the *Ss* responded more and more frequently to the 60% lights and less and less frequently to the 10% lights, both trends being statistically significant at the 1/10% level (Fig. 1). Chi-square tests, based on frequency of responses, over the last four trial-blocks, revealed significant departures from the predicted asymptotes at each of the four points, the 60% function going beyond the predicted asymptote of 60% and the 10% function reaching an asymptote of less than the predicted value of 10%. The overall χ^2-value was statistically significant at the 1/10% level. These findings confirm those of Gardner and present strong corroborative evidence for the need to modify the Estes model in the case of multiple-choice situations.

The analysis of the data revealed no statistically significant tendency for the *Ss* to respond to the 10% lights in proportion to their spatial proximity to the 60% light. Hence the phenomenon of spatial generalization failed to appear. A study done subsequent to the main investigation obviated the possibility that this phenomenon was obliterated by the presence of position-preferences. Had there been any evidence for either spatial generalization-effects or position-preferences, the assignments of reinforcement-probabilities to each of the five lights would have had to be counterbalanced before any confirmation could have been made of the discrepancy between Estes' model and Gardner's findings.

The failure to support the hypothesis of generalization in the present setting suggests that 'reinforcing' one stimulus more often than any of the others, where the stimuli fall along a visual-spatial dimension, is not a sufficient condition for demonstrating the phenomenon of spatial generalization. It would appear that this phenomenon may be revealed only where the experimental procedures employed are analogous to those used to demonstrate stimulus-generalization in a classical conditioning setting.[5] Where such an analogy may be applied, the effects of spatial generalization are deducible from an extension of the Hullian concept of generalized habit-strength.[6]

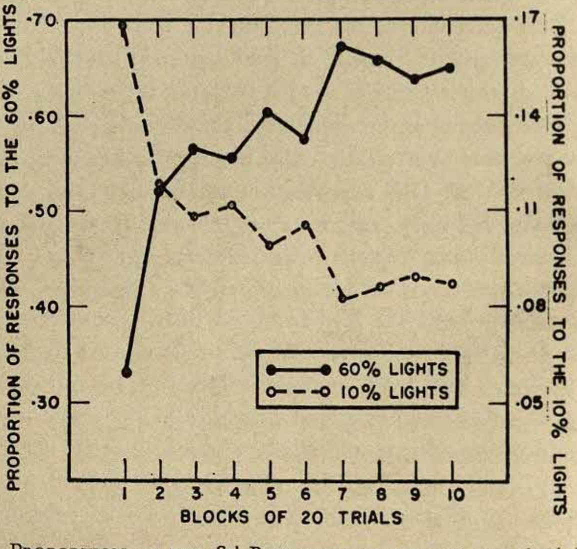

FIG. 1. PROPORTION OF THE *Ss*' RESPONSES TO THE 10 AND 60% LIGHTS

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P. D. McCORMACK

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[5] Brown, Bilodeau, and Baron, *op. cit.*, 61; Brown, Clarke, and Stein, *op. cit.*, 362; Mednick, *op. cit.*, 541; Mednick and Lehtinen, *op. cit.*, 183.

[6] C. L. Hull, *Principles of Behavior*, 1943.

SOME OBSERVATIONS ON THE SPIRAL AFTER-EFFECT

There have been few attempts to explain the after-effects which follow upon inspection of a rotating Archimedes spiral. Recently, two British psychologists, Deutsch and George, have outlined theories to account for this effect.[1] They have apparently considered only the situation in which the after-effects are projected upon the stationary spiral since, in each case, their theories require that the processes set in motion during the inspection-period affect the consequent perception of contours. It is, therefore, of some interests to know what properties, if any, the projection-surface must have to yield the perception of the after-effects.[2]

Our observers reported that the after-effects were seen against textured surfaces, such as paper and plastered walls, thus ruling out contours as a necessary condition for perceiving them. On a surface of this type, the after-effects are seen as a clearly delimited area which is uniformly expanding or contracting. They were not, however, observed projected against a clear blue sky, the only textureless surface readily available, but they could be observed when projected on nearby clouds. This indicates that texture is a necessary condition for perceiving the after-effects, while visual stimulation alone is not.

During inspection of the rotating spiral *O*s report various changes such as lines becoming blurred and angles appearing. These changes occur so regularly as to suggest that they are necessary for the subsequent perception of the after-effects. Actually, this is not the case. The appearance of blurring and angles can be readily eliminated by switching monocular vision, while the after-effects remain unchanged.

There are at least two parallels between the spiral after-effects and the stationary visual after-effects. The first is that both seem greater when they lie in peripheral vision. In addition, the inspection-time required to produce the spiral after-effects is of roughly the same order, between 1–5 sec., as that which other investigators found to be necessary to produce figural after-effects.[3] The region affected by the inspection of rotating spiral does not, however, act exactly like a satiated area. In this regard, the authors made several attempts to discover whether figures would be displaced or changed in size if they fell just outside the area altered by viewing the spiral. There were, however, no cases of discernible size- or displacement-effects under these conditions.

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[1] J. A. Deutsch, The statistical theory of figural after-effects and acuity, *Brit. J. Psychol.*, 49, 1956, 208-215; F. H. George, On the theory of the figural after-effect, *Canad. J. Psychol.*, 7, 1953, 167-171.

[2] A. G. Goldstein, On the after-effects of the 'waterfall' and 'spiral' illusions, this JOURNAL, 71, 1958, 608 f.

[3] Wolfgang Köhler and Hans Wallach, Figural after-effects: An investigation of visual processes, *Proc. Amer. phil. Soc.*, 88, 1944, 269-357; E. R. Hammer, Temporal factors in figural after-effects, this JOURNAL, 62, 1949, 337-354.

Edward R. Johnstone Training
and Research Center

B. C. Griffith
H. H. Spitz

FURTHER RESULTS CONCERNING THE EFFECT OF MOTIVATION BY ELECTRICAL SHOCKS ON REACTION-TIME IN RELATION TO AGE

In a recent number of this Journal, we reported the results of a study which showed that shock-motivation decreased reaction-time for both an old and a young group of *S*s (median ages 71.0 and 23.5 yr., respectively), and that the extent of decrease was similar statistically for both groups.[1]

We have repeated the original study in which the preparatory intervals were varied in an irregular series and we obtained similar results. We then used a different series of irregular intervals and again obtained similar results. When, however, we used intervals in regular series, the *S*s of the older group improved their reaction-times more than did the younger group, and significantly so, as ($p = <0.01$). Thus, the effect of shock-motivation on reaction-time in relation to age appears to be contingent upon the type of series of preparatory intervals used.

National Institute of Mental Health

Jack Botwinick
Joseph F. Brinley
Joseph S. Robbin

A NEW AUTOKINETIC ILLUSION

During the course of a study on autokinesis, the visual phenomenon reported here was accidentally discovered. A wooden rod ½ in. in diameter and 35 in. long was covered longitudinally on one-half of its surface by luminous green paint. After the rod had been placed vertically at eye-level in a dark room at a distance of 10 ft. from *O*, the paint was briefly 'charged' by the light from a 75-w. lamp. Then the rod was rotated on its base until *O* saw only a narrow strip of luminous light. In repeated observations, our *O*s (9 of 10) reported, usually at a latency of less than 30 sec., that the strip of light became broken, *i.e.* resolved itself into dots and dashes. Six of our *S*s further observed an irregular movement; drift or flow among the dots and dashes in both 'up' and 'down' directions. These *O*s stated that movement continued whether they fixated a specific spot or dot of light or scanned the line, up and down as a whole. In addition, they reported that once the movement started, it was difficult to stop it.

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[1] Jack Botwinick, J. F. Brinley, and J. S. Robbin, The effect of motivation by electrical shocks on reaction-time in relation to age, this Journal, 71, 1958, 408-411.

An unusual aspect of this phenomenon is its inconsistency with typical autokinetic effects. The narrow strip of light did not fluctuate as a whole, *i.e.* it did not become larger or smaller, flicker, bend, or move as a whole one way or another.

Colgate University ROBERT D. MYERS

THE INTERAMERICAN SOCIETY OF PSYCHOLOGY

The annual Congress of the Interamerican Society of Psychology, scheduled for December, 1958, was omitted. The next congress, the sixth, will be held in Rio de Janeiro, Brazil, on August 16-21, 1959. The central theme chosen for this meeting is: "Personality evaluation and human relations." Under this general theme the following sessions and symposia are being organized: "Experimental foundations of personality-theory"; "Social factors in mental health"; "Personnel selection in industry"; "Human relations in industry"; "Group relations and attitudes"; "Recent developments in gerontology"; and "Testing personality and aptitude in schools."

Members of the Society, as well as applicants for membership, who are interested in participating in any of these sessions or symposia are requested to submit abstracts of not more than 200 words to Samuel Pearlman, Executive Secretary, Brooklyn College, Brooklyn 10, N.Y., from whom more detailed information regarding the 1959 Congress may also be obtained.

Brooklyn College SAMUEL PEARLMAN

ERRATUM

Dr. R. Allen Gardner invites attention to a regrettable error that was made in Table II, p. 714, of his article on "Multiple-choice decision-behavior" in the December, 1958 issue (Vol. 71, No. 4). The number of choices under Choices 2—Period 2 should read 70.5 instead of 79.5. Subscribers are requested to make this correction in this table.

K. M. D.

Karl Spencer Lashley: 1890–1958

K. S. Lashley died in Poitiers, France, on August 7, 1958, bringing to an end a brilliant career, perhaps the most brilliant in the psychology of this century. In particular, the sustained vigor of his studies on the mechanisms of learning from 1920 to 1929 seems to me to be without equal in recent experimental psychology. His career was also a controversial one, for the general tenor of his work was critical: first, in the series of experiments that destroyed the simple stimulus-response theories of the day; and, secondly, in his later theoretical papers, which addressed themselves with vigor to the shortcomings of current explanatory notions. The net effect, however, was more than a critique of theory, to be forgotten when the theory itself is outmoded. In support of his criticism he sought new phenomena and new analyses of behavior with which to confront the ideas he was opposing, and these additions to knowledge, together with his mastery of a widely scattered literature, make his work of continuing importance for psychology as well as for the neurological sciences.

Because his work impinged on neurology and neurophysiology, psychologists have been known to assert that Lashley was "only a physiologist," with the implication that for 'pure' psychology his papers are not required reading. Undoubtedly such an idea, especially since the recent development of positivistic ideas in psychology, has led to some neglect of his papers. No idea could be more wrong.

Though his formal training was chiefly in biology, he was in his own eyes a psychologist. His interest in the problem of mind, cognate with the problem of brain mechanisms, was excited as an undergraduate, and as a graduate students he was publishing in psychology and collaborating with Watson before the completion of his doctoral work with Jennings. He became an accomplished anatomist and histologist and made great use of surgical methods, but for him these skills were avenues to the all-important problem of behavior. Lashley regarded behavior as a biological phenomenon and saw no reason why he should not use every means at hand for analyzing it, including surgical operations; but he also contributed brilliant studies of the intact organism. Whatever else he may have been, he was in addition, by any definition and in the strict sense, an experimental psychologist of the first rank.

Karl S. Lashley was born in Davis, West Virginia, on June 7, 1890. His father was sociable, fun-loving, and easy-going, but capable; at various times he was storekeeper, postmaster, and banker, and he served several terms as mayor. His mother was energetic and 'intellectual,' with literary and artistic interests. Early in Karl's life the family did some moving about, going to California, probably with the hope of helping his father's arthritis, and then in 1898 joining the gold rush to the Klondike—this latter move, which must surely have had less therapeutic value, apparently due to his mother's spirit of adventure. The family then returned to Davis in 1899. The trip to Alaska and the Klondike remained a vivid memory for Lashley, and he returned twice, in 1949 and, following his second marriage, in 1957.[1]

His early education, in the formal sense, was thus somewhat interrupted; but for a bright child interruptions of schooling may only mean that he escapes from the lockstep of year-by-year promotion through the grades. Lashley, at any rate, graduated from the high school in Davis at the age of 14 yr., having become an avid reader (his mother had assembled at home a library of 2000 volumes), a collector of wildlife (from snails and butterflies to raccoons and—it is reported—beavers, though this last item is hard to understand), and a lover of mechanical gadgets. He was fascinated at first by the sewing machine and learned to use it (very handy later for sail-making), and then by the jig saw which he was given to wean him away from the sewing machine. One of his main relaxations in adult life was shopwork. He also learned to cook, and as an adult he would on occasion give evidence of considerable artistry as a chef.

At the University of West Virginia, Lashley was required to take a preparatory year, since the high school at Davis was not accredited. Next year, as a freshman, he proposed taking a major in Latin, but the registrar insisted on English. (Lashley, who loved a good story and was not averse to helping it out a bit, sometimes told students that his early training was in Latin.) In a vacant period he signed up for a class in zoölogy under John Black Johnston, the neurologist, and within a few weeks "I knew that I had found my life work." He changed his registration and studied general zoölogy and comparative anatomy, which attracted him because of his interest in animals and his liking for the free-hand drawing that was part of the work, and also because "it seemed to support the materialism which, even at 16 yr., I was beginning to formulate vaguely."

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[1] Lashley never prepared a formal autobiographical account, and some difficulty was experienced in obtaining the details of his life. Assistance has been received from E. G. Boring, F. A. Beach, Leonard Carmichael, K. M. Dallenbach, R. M. Elliott, E. R. Hilgard, Heinrich Klüver, Carney Landis, W. H. Taliaferro, E. C. Tolman, and L. E. Wiley. A special debt is owed to Claire S. Lashley for details of the early life, from conversations with Lashley and from some scattered notes written by Lashley himself.

Next year he was made an assistant under Albert M. Reese, Johnston's successor. He found a set of Golgi sections of the frog brain and proposed that he should draw all the connections between cells: "then we would know how the frog worked. It was a shock to learn that the Golgi method does not stain all cells, but I think almost ever since I have been trying to trace those connections." That year he took embryology, histology, and animal behavior from Reese. Next year Reese had too much to do to put on courses for Lashley, the only third-year major, so he sent him off to work up, on his own, courses in comparative osteology and neural anatomy.

Upon graduating with an A.B. in 1910, Lashley obtained a teaching fellowship in bacteriology at the University of Pittsburgh, and received his M.S. in 1911. K. M. Dallenbach was also a teaching fellow, in psychology, and the two spent much time together. Lashley took Dallenbach's course in experimental psychology, and, when he went next year to Hopkins to enroll for his Ph.D. in zoölogy under H. S. Jennings, he elected a minor in psychology, under John B. Watson and Adolf Meyer. His first publication, in 1912, was on form- and size-discrimination in the rat, in the second volume of the new *Journal of Animal Behavior*. In 1913 he published two papers, with Jennings, one on the inheritance of size and the other on the question of sexuality in paramecium. He obtained his Ph.D. in zoölogy in 1914.

At this time he seems already to have been working more with Watson than with Jennings, but it was only in 1915 that he decided finally to make his career psychology. In 1914-15 he was Bruce Fellow, in 1915-16 Johnston Scholar, at Hopkins. Those who remember him at this stage speak of his brilliance and a contagion of excitement about his discussion of varied psychological problems. Certainly he covered a wide range. He published on homing with Watson (1915; he also wrote some of the discussion of learning-theory in Watson's book in 1914); on instinctive behavior in birds (1915); on human learning ("the acquisition of skill in archery," 1915); on color-vision in the hen (1916); on behavior of paramecium (with S. O. Mast, 1916); on the effect of drugs—strychnine and caffeine—on rat-learning (1917); and on handedness in the monkey (1917).

At some time in this period he came in contact with Shepherd Ivory Franz, who was then at St. Elizabeths Hospital in Washington. Now the pattern of his major work appears, in two papers jointly with Franz in 1917, on the effects of brain-damage on learning in the rat. He also did some clinical research, on motor control in tabetic patients who lacked sensation in the limb being moved, and reported it in a paper that is still of considerable interest.[2]

In 1917 he was appointed instructor in psychology at Minnesota, but the situation was, to say the least, confused. R. M. Yerkes was Professor and Director of the Psychological Laboratory; as such he recruited staff, but never went to Minnesota to take up his own post. Lashley found the facilities for work deficient. He returned to Washington in 1918 to take a post as Investigator for the U.S. Interdepartmental Hygiene Board, working with Watson on popular attitudes toward sexual matters, and knowledge about them. As sometimes happens, the fund-granting body had rather different interests from those of the experimenters; the hope of the Board was to lay a foundation for campaigns against venereal disease, while that of Watson and Lashley was to study the basis of sexual motivation in man. The method was mostly by questionary and survey, but it had experimental aspects too. Later Lashley had some tall tales to tell about advertising a free movie (a real attraction in 1919) in an unsophisticated community, without warning as to what kind of picture would be shown, and having later to escape an outraged citizenry. This activity was not, however, a mere interlude in his career. Though Lashley's first interest lay in experimental work with animals, there is no doubt that he saw human sex behavior as a fundamental problem for psychological study, and he was later a long-time member of the National Research Council's Committee for Research on Problems of Sex.

In 1920, however, R. M. Elliott, the new chairman at Minnesota, got Lashley an appointment as assistant professor and relieved him of routine teaching. Now he returned to the neurological analysis of the rat's behavior. A letter from him to Calvin Stone, discussing possible Ph.D. thesis-problems that Stone might undertake, lists Lashley's own interests early in 1920: (1) sensory processes (comparative); (2) effect of drugs and hormones upon learning; (3) experimental control of instincts; (4) neurological basis of learning; and (5) functional anatomy of the brain.

The foundations were laid, and Lashley went on to eminence. In 1924 he was made full professor at Minnesota. In 1926 he left to go to the Behavior Research Fund of the Institute for Juvenile Research, Chicago, apparently in the hope that the change of locus might help his wife's asthma. In 1929 he was President of the American Psychological Association, published his monograph *Brain Mechanisms and Intelligence*, accepted a professorship at the University of Chicago, and delivered his presidential address to a combined meeting of the American Psychological Association and the Ninth International Congress at New Haven. The address was a devastating review of reflexological theory, based primarily on the brain-injury experiments he had done in the preceding nine years, and an attempt to reformulate the problems of the cerebral control of behavior. In 1935 Lashley moved again, to Harvard, at first as Professor of Psychology (1935-37) and then as Research Professor of Neuropsychology with nominal teaching duties (1937-55), an arrangement that permitted him in 1942 to accept the Directorship of the Yerkes Laboratories of Primate Biology (1942-55) in Orange Park, Florida. He retired as Professor Emeritus at Harvard, and Emeritus Director of the Yerkes Laboratories (1955-58).

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[2] K. S. Lashley, The accuracy of movement in the absence of excitation from the moving organ, *Amer. J. Physiol.*, 43, 1917, 169-194.

He received many honors. In addition to the presidency of the American Psychological Association in 1929, he was President of the Eastern Psychological Association in 1938. He received an honorary A.M. from Harvard (1942); the Sc.D. from Pittsburgh (1936), Chicago (1941), Western Reserve (1951), and Pennsylvania (1955); and the LL.D. from Hopkins (1953). He was a member of the National Academy of Sciences, of the Royal Society, London, and of the American Philosophical Society, to which, not long before his death, he gave funds to endow the Karl Spencer Lashley Award for distinguished work in neurobiology. He was a member of the Society of Experimental Psychologists, the American Society of Zoologists, the American Physiological Society, the Society of American Naturalists, and an Honorary Fellow of the British Psychological Society. He received the Warren Medal awarded by the Society of Experimental Psychologists, the Elliott Medal of the National Academy, and the Baly Medal of the Royal College of Physicians.

With his students Lashley was unpretentious and informal. He gave little guidance except when the student asked for it; his attitude was that he and the student were investigators together. He disliked teaching in the sense of giving lectures and was not particularly good at it, but he was extremely effective in the laboratory and in small discussion groups. Here he was relaxed and friendly though he kept the relation impersonal, with staff as well as with students except for a few close friends. To a colleague of the early days at Hopkins he seemed all intellect, and another recalls his talking only shop at Minnesota; but this was in the period of his first engrossing successes. It was not all psychology in the luncheon discussions held outdoors in Florida at the Yerkes Laboratories of Primate Biology. Away from the laboratory he could drop the role of scientist entirely. He loved good plays and good music (except opera), but he also enjoyed burlesque and vaudeville, and he had a mischievous humor that he might use either to entertain or to slyly puncture another's argument.

Music and sailing were his two great hobbies, but for relaxation he depended also on shopwork, at which he was skilled, as well as on cabinet making and construction work at home. His interest in music dated from the ages of 18, when he taught himself to play the violin, and 20, when he first heard classical music and was fascinated by it. It was through this interest that he met his first wife, Edith Baker, a professionally trained musician who graduated in piano from Peabody College of Music in Baltimore and could have gone on the concert stage had her health permitted. They maintained their musical activity as an important feature of their marriage, Karl on the violin or cello, Edith on the piano. Through her, Lashley said, he met many professional musicians, and "since 1920 [I] have devoted at least one evening each week to the playing of chamber music of which I have collected an extensive library." In sailing also he was self-taught, mastering the art by trial and error and sheer force of intellect. He had conducted some early experiments with a sailing canoe, but later he got a 30-ft. yawl, *Banshee,* with which he and Lester E. Wiley (both novices in a vessel of that size) could go venturing forth on Lake Michigan to return, in the early days, only by the grace of God. Lashley could not swim (he learned how at the age of 67).

Lashley's marriage to Edith Ann Baker was in 1918. They had one child, a son, who died shortly after birth. Mrs. Lashley suffered from severe asthma, which must have been a threat constantly hanging over her head and Lashley's, conditioning their whole lives. At Minnesota Lashley would receive a telephone call, hurry home to give his wife an injection of adrenalin, and return to go on with his work. They were, however, a happy pair. Mrs. Lashley was always cheerful, and the two were skillful hosts in setting awkward students at their ease. She died in 1948, and nine years later Lashley married another charming woman, Claire Imrédy Schiller, whose husband Paul Schiller had died in a skiing accident in 1949. Lashley had had a serious illness (acquired hemolytic anemia) which began in 1954 and was treated at first with cortisone. This led to softening of the vertebrae, and finally the spleen was removed in 1955, after which he made a slow recovery. His health seemed good again in 1957, when he and Mrs. Lashley took a wedding trip, back to Alaska. In 1958 they set out on a trip abroad, when he had heart failure and died in France on August 7.

It is not an easy task to evaluate Lashley's contributions to science, and especially to psychology. He made important contributions to neurophysiology and neuroanatomy, in two early studies of the motor area[3] and in studies of the anatomy of the visual system and of thalamo-cortical projection in the rat.[4] An outstanding study was the critique of cortical architectonics with George Clark.[5] In the first years of the present century, Brodmann, Campbell and others had described differences in the finer structure of the cortex, all of which, it came to be assumed, represented differences of function. Brodmann defined some 47 distinct areas in the human cortex. Others made a further subdivision into more than a hundred areas all told, each of which might be thought of as a separate 'organ,' with its distinctive role in psychological processes. Lashley and Clark put an abrupt end to such extravagances, showing that all but a half-dozen or so of the areas existed only in the eye of the beholder and provided no basis for localization of function.

It is in the field of behavior, however, that Lashley's main contribution was made. Here I shall not attempt to document the statement that he was as brilliant in his psychological observations as in his application of surgical and histological methods to the problem of learning; the best supporting evidence will be found in the volume of selected papers to appear shortly.[6] As examples, the reader need only be reminded of his extensive analysis of visual pattern-perception in the normal rat[7] and of language and thought in man.[8]

He kept constantly before him his question, from undergraduate days, of 'how the frog worked'—the *whole* frog (or whole man). He was a dedicated materialist, to whom it was clear that what man does is determined by the brain and not by any animistic principle. He attacked the problem head-on with his brain-operation studies at Minnesota and Chicago and found complexities for which he was not prepared to offer a neurological explanation in detail. To have recourse to animism because of such difficulties was, for him, to give up any attempt to solve the problem; and to approach it by way of intervening variables, without physiological definition, was simply to avoid or postpone it. His principles of mass action and equipotentiality were not the explanation he sought, but rather summary statements of observations which themselves needed explanation. He tentatively formulated an hypothesis of interference-patterns[9] as a mechanism to account for equipotentiality in perception as well as for perceptual learning, but he was not satisfied with it. (He remarked that he had destroyed all theories of behavior, including his own.) Thus Lashley was left in the uncomfortable position of being against existing theory, a persistent critic of new as well as old ideas, without having others of his own which he was willing to support as an adequate substitute.

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[3] Lashley, Studies of cerebral function in learning: III. The motor areas, *Brain*, 44, 1921, 255-286; Temporal variation in the function of the gyrus precentralis in primates, *Amer. J. Physiol.*, 65, 1923, 585-602.

[4] *E.g.* Lashley, The mechanism of vision: VIII. The projection of the retina upon the cerebral cortex of the rat, *J. comp. Neurol.*, 60, 1934, 57-79; Thalamo-cortical connections of the rat's brain, *ibid.*, 75, 1941, 67-121.

[5] K. S. Lashley and George Clark, The cytoarchitecture of the cerebral cortex of Ateles: a critical examination of architectonic studies, *J. comp. Neurol.*, 85, 1946, 223-306.

[6] A selection of Lashley's papers to be published in 1959 by McGraw-Hill, with introductions by E. G. Boring and Stanley Cobb (edited by F. A. Beach, D. O. Hebb, C. T. Morgan, and H. W. Nissen).

[7] Lashley, The mechanism of vision: XV. Preliminary studies of the rat's capacity for detail vision, *J. gen. Psychol.*, 18, 1938, 123-193.

[8] Lashley, The problem of serial order in behavior, in *Cerebral Mechanisms in Behavior* (L. A. Jeffress, ed.), New York, Wiley, 1951.

On the other hand, it does not do his theoretical position justice to regard his criticism as destructive only. He clarified many problems, often by setting them in a new context that his own research or scholarship made possible, with corresponding implications for new research. He certainly had biases and fixed ideas, resulting from earlier controversies, biases which affected these formulations. On such points he was undogmatic and open to conviction in the course of a discussion, but he did not remain convinced: it seems, essentially, that he failed to realize how great the impact of his own work had been, even for those who opposed his views. In 1945, he still saw the issues of the innate vs. the acquired, of S-R connections, and of the law of effect, very much as he had seen them in the twenties. On the question of the innate, for example, he tended to regard his opponents as Watsonian empiricists still, who would deny all hereditary organization. But a remarkable feature of his earlier analysis is the extent to which it clarified the great continuing problems of psychology. Thus his thinking remains relevant today, even though it was tied to problems formulated earlier. One of the most fertile papers in the current literature is his contribution to the Hixon Symposium,[10] and the paper read at the 1956 meetings of the Association for Research in Nervous and Mental Disease is unexcelled as a statement of what is involved in the physiological analysis of mental functions.[11]

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[9] Lashley, The problem of cerebral organization in vision, *Biol. Symp.*, 7, 1942, 301-322, esp. 312-316. The problem is to account for variability in the afferent neurons which give rise to a perception, and at the same time for the memory-trace, which must involve specific cells. Lashley proposed that a given pattern of activity might excite the same foci of activity throughout the cortex, by summation analogous to an interference of standing waves in a fluid medium, even though the locus of the afferent activity differed from one occasion to the next.

[10] See Footnote 8.

[11] Lashley, Cerebral organization and behavior, *Res. Publ. Ass. Res. nerv. ment. Dis.*, 36, 1958, 1-14.

His influence was widely felt outside psychology. Cobb has remarked the attention paid by neurologists and psychiatrists to his work;[12] Walshe considers that the Nobel prize has been won by lesser men.[13]

Within psychology today his work is not given the attention it had earlier, but this in a way was his own doing. In 1929 he demolished physiological conceptions of learning so completely that physiologizing became slightly ridiculous. Having burnt its fingers, psychology turned away in the thirties to E. C. Tolman's intervening variables or B. F. Skinner's positivism. Neither Tolman nor Skinner adopted his own position because of Lashley; yet it seems clear that the appeal of such positions must have been greater in the negativistic climate created by Lashley's work. One extreme, the naïve physiologizing of the twenties, was corrected by another. an antiphysiological one, in which Lashley's work inevitably came to be neglected. Today the pendulum is swinging again (though one may hope that its excursions are becoming shorter, so that we can keep in mind both the values and the limitations of physiologizing), and it seems likely that history will see him in something like the same light as he was seen in 1930, when he stood head and shoulders above the field. His development of Franz's methods was profoundly original; in addition to his important anatomical contributions, his studies of learning, perception, and thought are outstanding, and his later papers are still the best corrective for any over-confident theorizing or for the narrowness to which a theorist is prone.

McGill University

D. O. HEBB

AN ACKNOWLEDGMENT

The photograph and signature under it of Karl S. Lashley, which are reproduced in the frontispiece of this number, were both obtained through the kindness and courtesy of his wife, Mrs. Claire S. Lashley. The photograph was taken during the spring of 1948. The signature, a rare sample, as he usually abbreviated his given names or omitted his middle initial when signing letters, was taken from a bank check which he signed shortly before death.

K. M. D.

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[12] See Footnote 6.

[13] Francis Walshe, the distinguished former editor of *Brain*, in an appreciation in *Neurology*, 8, 1958, 870.

BOOK REVIEWS

Edited by George L. Kreezer, Washington University

Minnesota Studies in the Philosophy of Science: Volume I. The Foundations of Science and the Concepts of Psychology and Psychoanalysis. By Feigl, Herbert and Scriven, Michael, Eds. Minneapolis: University of Minnesota Press, 1956. $5.00.

To one inexpert in philosophy of science it is not an easy task to evaluate this book, but it is a stimulating experience. One must presume that the reactions of an ordinary, mortal psychologist were being sought when the present review was invited.

Apparently all of the papers, twelve altogether, have appeared separately elsewhere. Nevertheless, several central themes emerge, and it does appear useful to put the papers together into a single volume. A very helpful synopsis of each of the papers is included at the beginning of the book.

One general impression is that the philosophers of science seem to be having their difficulties. Some of the changes in their positions should make experimentalists who are not experts in the area wary of following blindly the perscriptions of any one set of them. For example, strictly reductionist operationism and positivism has lost favor among the authors presented in this book who are primarily philosophers: Feigl, Carnap, Scriven, Sellars, Flew, and Buck. There seems to be a generally increased tolerance for "looser" theorizing, as was adumbrated for psychologists by Cronbach and Meehl in their paper on construct validity, which is reproduced in this volume.

A secondary impression is that there appears to be a strong tendency for philosophers of science to become amateur or armchair psychologists. Indeed, many of the questions the philosophers discuss appear to this reviewer to be reducible to empirical questions in psychology. Yet, there is a tendency for the philosophers to talk about psychological questions as if they were conversant with the relevant experimental data in the erea, for which they present little evidence in this volume. Perhaps this results from the general impression which appears to be abroad that there is a large disparity in the number and quality of discoveries made by the physical and behavioral sciences. Indeed, the final chapter in this book, by Scriven, is an introduction to an argument that there is a distinction in principle between the behavioral sciences and the older natural sciences, and that while one may expect constant improvement in the former, one can never expect the simple formulations of laws to be found in the latter.

With respect to psychological problems, major attention is devoted to the concepts of psychoanalysis; next in amount of space is that devoted to some problems of psychometrics; then there is a critique of Skinner, and finally, a critique of general behavior systems theory.

Skinner's paper, which has previously appeared in the Scientific Monthly, is a critique of psychoanalytic theory. The content and approach of psychoanalytic theory to psychological problems are contrasted with his own behavioristic approach, and this is combined with a more general attack on the theory as such. Essentially, Skinner believes that Freud dealt in fruitless concepts while attending to important problems. Freud's generalizations obscured significant variables and ignored the specific acts engendered by childhood episodes, it is argued. Finally, Skinner believes that too much has been made of the distinction between conscious and unconscious. He says: "The important point was not that the individual was often unable to describe important aspect of his own behavior or identify important causal relationships, but that his ability to describe them was irrelevant to the occurrence of the behavior or to the effectiveness of the causes. . . . But the act of self description, as of self observation, plays no part in the determination of action. It is superimposed upon behavior. Freud's argument that we need not be aware of important causes of conduct leads naturally to the broader conclusion that awareness of cause has nothing to do with causal effectiveness." (P. 83)

This is, in fact, an inductive leap. Surely Skinner does not mean to exclude the possibly reinforcing effects of self description or self observation, or indeed, the possibly interfering effect of such activity upon events meant to be reinforcing by the experimenter. The degree to which the importance of awareness or knowledge is universally assumed is testified to by the great care which experimenters usually take to hide the purpose of an experiment from a human subject. There is an implicit assumption in this procedure that such awareness or knowledge will affect behavior. This may not detract from the effectiveness of Skinner's critique of psychoanalysis, but it does raise questions as to the limitations on the alternatives available.

Skinner's paper is followed by a critique of Skinner by Scriven. The main argument here seems to be that we cannot do without intervening mental states as explanatory principles. There are in addition some clever arguments showing that Skinner, in his own explanations of experimental results, uses some concepts which are inadmissible in science, according to Skinner.

At the same time, Scriven appears to believe that there are methods other than experiments for gaining knowledge, for he attributes to Freud "discoveries," and seems to argue that such knowledge is as "real" as that gained by experiment. Scriven wants psychologists to meet psychoanalysis on its own grounds and either produce evidence of the untenability of the concepts or produce a better theory. Here we see the strange consequences of a philosopher of science attempting to prescribe appropriate paths of work for psychologists. Perhaps, a more appropriate function for the philosophers in this area is to tidy things up after the experimentalist has hewn a zig-zaggy and sloppy trail, full of stumps, through which only other experimentalists on foot can travel. It would indeed seem a proper function for philosophers to uproot the stumps, straighten out the sides of the road and pave it so that all can follow on appropriately modern conveyances.

Scriven's chapter is followed by a valient effort by Ellis to give an operational formulation of psychoanalytic concepts. It is perhaps a courageous effort, but it is bound to be acceptable neither to psychologists nor psychoanalysts. Definitions are confounded with hypotheses and vice versa. Psychoanalytic concepts are shorn of their most attractive aspect, their richness, and in their stead is produced a set of elementary definitions of psychological functions, such as perception and thinking, which would be unacceptable for a first textbook in general psychology. Surely this is not what the Minnesota center for the philosophy of science is aiming for.

Following this is a paper entitled "Motives and the Unconscious" by Antony Flew. It is an exploration of the implications of the concept of the unconscious, difficult to follow, and apparently one part of a polemical discussion going on in philosophical-psychoanalytic circles. A major shortcoming is its failure to explore the evidence offered to support the notion of the unconscious, or its potential fruitfulness relative to other hypotheses. Although Flew states that he had no intention to do this, the lack of such wider investigations leaves many such discussions relatively meaningless to the general psychologist.

Psychologists are much more familiar with the papers by Cronbach and Meehl reprinted in this volume, and they would seem to need no further comments here.

Finally, a brief comment on Scriven's final chapter, which attempts to draw a distinction in principle between the physical sciences and behavioral sciences. Scriven believes that the number of critical variables to be controlled even in a laboratory experiment in the behavioral sciences is so great and creates such complexity that we cannot expect ever to formulate laws of the simplicity of Newton's or Einstein's. "This is a claim," he believes, "based on the empirical evidence, not on any *a priori* necessity." If this is so, of course, the only test for such a notion can be made only by repeated trials. From a most mundane point of view, we psychologists would appear to have no choice but to act as if such general laws were discoverable; for to act otherwise, would guarantee our failure to test Scriven's hypothesis. Moreover, it is a serious question whether there do not exist already general psychological laws of sufficient number and quality to warrant some cataloging. Perhaps psychologists feel unnecessarily inferior.

It is probably not an accident that psychoanalysis receives the lion's share of the attention in this book, for psychoanalysis is really the only system which has attempted an explanation of *peculiarly* human behavior, and of all of it. Apparently, the philosophers of science take it as a sign of the need for careful analysis that psychoanalysts rarely find behavior puzzling and generally have ready explanations for almost anything. Or is it, perhaps, that here is another field in which exact data need not be considered; indeed, the theory requires a method which cannot produce exact data. Almost all of the authors include at least one sentence in their paprs stating their awareness of this fact, and they continually reassure us that they are not passing on the correctness or incorrectness of psychoanalytic theory. Few say anything about the tenability of the theory's requirement that a method which cannot produce exact data be employed for its investigation. None seems to recognize that the methods he would require for the testing of this theory are precisely the objective methods universally unacceptable to the psychoanalysts. If this is an *ad hominem* argument, it hardly seems avoidable, since were it not for the presence of institutes of psychoanalysis many of the current discussions might not be necessary at all. Psychoanalytic theory would be evaluated alongside other psychological theories. We do not have institutes for Newtonian physics or Einsteinian physics. One strongly suspects that the psychoanalysts have enticed some philosophers, and one can only hope that they will emerge unconfused and ready again to lend their aid in the scientific study of behavior.

University of Pennsylvania JULIUS WISHNER

A Theory of Cognitive Dissonance, By LEON FESTINGER, Evanston, Ill., Row-Peterson, 1957. Pp. v, 291.

There is some dissatisfaction with learning-theory as an explanation for behavior. Theoretically, the trouble seems to be that in placing the explanatory burden on past associations and reinforcements, the organism's present cognitions and motivations do not receive the attention they deserve as determinants of behavior. The difficulty is reflected empirically in the fact that past experience is found to be neither directly nor simply related to present responses. Witness the increasingly bewildering data on partial reinforcement. As a reaction to this difficulty, there has been (1) a noticeable emphasis upon discrimination, decision-making, and other contemporaneous processes; (2) a vigorous anarchistic approach in which no theoretical assumptions whatever are made concerning the effects of experience or 'reinforcement schedules' upon response-production; and (3) a resurgence of interest in cognition and cognitively related motivation. Festinger's theory of cognitive dissonance falls within this last category.

In brief, it is proposed that much behavior reflects the organism's attempt to reduce 'dissonance'—roughly an inconsistency between cognitions. There is no presumption on Festinger's part that this formulation is fundamentally new; indeed, he likens the reduction of dissonance to classical, homeostatic processes set off by drives like hunger, and to modern conceptions of the cognitive processes such as Heider's theory of balance. Considering the range of phenomena, however, that are comprehended and predicted in terms of dissonance-reduction, Festinger has developed an idea of considerable power and originality which is well supported by experimental evidence.

Since the idea that people 'strain toward consistency' is not new, why is a wholly new term introduced? Will not the concept of conflict serve just as well? Festinger would presumably argue that, not only is 'conflict' a term with multiple meanings, but it is a notion that is basically different from 'dissonance.' On the latter point, dissonance arises after a decision, *i.e.* after a conflict of action-tendencies is resolved. While plausible, this argument is based on a specific assumption concerning the locus of conflicts. Moreover, there is no necessary reason for assuming that decision resolves conflict, just as there is no reason to suppose that eating necessarily satifies hunger. The manifest confusion surrounding conflict may be a better justification for a new concept.

The conceptual definition of dissonance is, however, vague. Festinger says that "two cognitive elements are in a dissonant relation if, considering these two alone, the obverse of one element would follow from another." The catch is, of course, that *obverse* and *follow from* are not clearly defined, hence it is not clear conceptually when a dissonance exists.

The magnitude of dissonance is defined as the weighted (according to their importance) proportion of dissonant elements. Apart from the rather formidable problem of measurement (*e.g.* how is a cognitive element defined?), there are other difficulties in this formulation. For example: (1) The intensity of dissonance, *i.e.* the degree of 'obverseness' does not seem to be handled by the proportion of dissonant elements in the formulation. (2) The role of uncertainty in producing cognitive dissonance is not clear. The cognitive elements implicit in dissonant relations are assumed to be perfectly reliable. Yet most cognitions are uncertain, at least to some degree. (3) It seems plausible to assume—as Festinger does—that punishment affects the degree of dissonance via changing the *importance* of the dissonant elements. Punishment also affects the state of the organism directly as pain, discomfort, etc. Is punishment as a motivational variable different from punishment defined in terms of cognitive importance?

Attempts at the reduction of dissonance take three forms: (1) changing one or more elements involved in the dissonant relationship; (2) adding more cognitive elements that are consonant with existing cognitions; and (3) decreasing the importance of the elements involved in the dissonant relationship. In general, Festinger handles this problem in terms of the quantities involved and the circumstances surrounding the dissonant situations. As one surveys the experiments as a whole, however, it is not always clear why each dissonant relation had the unique solution that he adopted. Obviously, predictions of the mode of dissonance-reduction, based on the quantities of the cognitive elements involved, require more effective methods of measurement than are now available.

In connection with the central role of cognition in the theory of dissonance, it is not clear how the theory can apply to the case of unconscious processes, *e.g.* being forced to associate with someone toward whom there is *unconscious* hostility. One possible advantage of conflict as an explanatory device is that it is not restricted to levels of consciousness.

A final question concerns the degree of generality held by the theory of dissonance. By implication, the painful, or at least negative character of dissonance is universal. At the same time, there presumably are wide individual variations in the 'tolerance for dissonance.' One wonders whether the pressure toward consistency is, at least to some extent, a reflection of certain values in Western society. For example, most Americans have an abhorrence of being cheated. Any purchase consequently may be accompanied by gnawing doubts of a mildly paranoid variety. How do they handle such uncertainty? Many brag about their material advantages, and, where possible, derogate the articles they decided against.

The criticisms in this review do not detract from the fact that Festinger's theory of dissonance generates subtle, non-obvious predictions which cover a wide variety of phenomena that can be confirmed experimentally. This alone is a sizable achievement.

University of Pennsylvania ALBERT PEPITONE

The Concept of Development. An Issue in the Study of Human Behavior. Edited by DALE B. HARRIS. University of Minnesota Press, 1957. Pp. x, 287, $4.75.

The present-day passion for symposia sometimes makes strange bed-fellows. But surely there have been few stranger assortments than the conglomeration of men, ideas, and viewpoints represented in *The Concept of Development.* The reader must at the outset solve two problems: first, in what sense is development a "concept"; and second, how does such a concept become an "issue" in the study of human behavior.

The most probable answer to the first question is that by "development" is meant what the biologists call "epigenesis," the coming into being, through development, of new qualities not represented in previous stages. In the study of human behavior, this concept is apparently intended to integrate researches concentrated on different levels of organization. Or at least, this is a likely interpretation; to identify a unifying theme in this collection of miscellaneous essays is rather more difficult than seizing a droplet of quicksilver between the finger tips.

The symposium is divided into five sections, of which the first comprises a general introduction. In the most interesting paper in this section, "Determinism and Development," Ernest Nagel makes a well-reasoned defense of moral responsibility in the face of determinism; it is however unfortunate that the author devotes a considerable part of the space allotted to him to biological viewpoints that are rather out-of-date. The other major essay in the section, by John E. Anderson, is a terminologically elaborate review of some commonplaces of development.

The second section is biological. Here Viktor Hamburger and Howard V. Meredith deal with development in the straightforward terms of embryology and anatomy. Extending this kind of approach into the problems of behavior *per se*, J. P. Scott provides some examples of genetically determined behavior patterns, including his own interesting work on puppies of different strains. T. C. Schneirla, in a long and valuable essay on the comparative psychological aspects, deals at length with the evidence, in a wide variety of animals, that the developmental pattern underlying behavior includes "a range of circular stimulative and reactive processes based on the gains of earlier stages."

In the third section, on the development of human behavior, Heinz Werner makes an interesting case for the study of the development of individuality; one could wish, though, that this author had chosen to enlarge on his proper subject instead of devoting much attention to some ill-digested biological speculations. In the same section, W. A. Russell explores the possibility that the development of behavior may provide a useful subject for experimental research; his conclusion, though hedged with restrictions, is in the affirmative. Robert R. Sears contributes a stimulating study, that refreshingly adheres to the matter in hand, of the development of identification.

The concept of development in the humanities and social sciences is the theme of the fourth section. To the credit of Normal DeWitt, who discusses the humanities, and Herbert Heaton, whose subject is history, it may be said that these authors clearly face up to the difficulty and even futility of applying biologically-derived concepts to their own quite different fields. In cultural anthropology, on the other hand, the situation may be somewhat more amenable to a limited developmental approach, particularly, as Robert F. Spencer maintains, in dealing with single cultural entities.

The last section is devoted to social applications of the concept of development. From the viewpoint of a practicing physician, John A. Anderson explores the lack of parallelism exhibited by different aspects of the development of the individual child. From a similar point of view, Willard C. Olson takes up developmental problems in education, emerging with the not unexpected conclusion that children do not all mature at the same rate. John C. Kidneigh provides a readable essay on social work, without however making it clear that developmental theory has a role to play in this important field. A Freud-centered essay on infancy, by Hyman S. Lippman, completes the section.

Although several of the individual papers are of interest in themselves, *The Concept of Development* as a whole is lacking in impact. The bringing together of specialists in different disciplines may have a cross-fertilizing effect on all concerned. But you can't cross an oak tree with a cabbage; and "development" in this book remains an inert word instead of growing into a living concept. Whatever profit the participants may have found in the symposium sessions, the publication itself suffers from the failure of the sponsors to define their terms critically and to limit their field to matters linked by something more vital than a merely verbal concept. The book is pretentious, but not so important as a less ambitious but better conceived plan might have made it.

Washington University FLORENCE MOOG

Social Class and Mental Illness. By AUGUST B. HOLLINGSHEAD and FREDERICK C. REDLICH. New York, John Wiley and Sons, 1958. Pp. ix, 442. $7.50.

The earlier findings of Hollingshead and Redlich of significant class-linked factors in the prevalence and treatment of mental illness in New Haven, Connecticut have already gained widespread currency. In the book reviewed here, they draw their data together in comprehensive form and present detailed discussion of research design, sociological and psychiatric features of the New Haven community, and interpretation of results. What emerges is a lucid, sophisticated presentation of data bearing upon two basic questions, each of which is affirmatively answered in their study: (1) "Is mental illness related to social class?"; and (2) "Does a mentally ill patient's position in the status-system affect how he is treated for his illness?"

It is not possible in a brief review to summarize adequately either the study itself or its extensive findings. Suffice it to say that the design, execution, and analysis of their data are elaborate, meticulous, and well-presented. As a study, this work will undoubtedly become an epidemiological classic. It is a work to be read and pondered by clinicians, mental hygienists, social psychologists, and anyone with more than passing interest in mental illness and its treatment.

As is well-known, epidemiological studies of mental illness are exceedingly difficult to make, mainly because we have no clear idea or criterion of what mental illness is. As a result, one encounters an enormous problem in defining the basic unit of analysis, the 'case.' However this problem is solved, it will represent a compromise of sorts. For this reviewer, however, Hollingshead and Redlich press their compromise too far in defining a 'case' as, "Any person in treatment with a psychiatrist or under the care of a psychiatric agency." By restricting their definition to psychiatric care, so many potential cases are excluded that it becomes difficult to decide how accurately the data reflect even treated prevalence, much less true prevalence (which the authors do not pretend to determine).

This would not be a serious criticism were it not for the fact the authors' own data suggest such 'cases' are not randomly distributed among the various social classes. It is very possible that a large percentage of 'cases' in 'treatment' with non-psychiatrists are lower-class members.

In another respect, too, their data are unclear. There is reason to believe that the psychiatric *act* of diagnosis is not unrelated to the class position of the patient. To the extent diagnoses (as distinct from actual illnesses) vary as a function of class, it is hard to know exactly to what these data refer. To be sure, their study is epidemiological—but the epidemiology of what?

In any event Hollingshead and Redlich are especially troubled by their findings of 'discrimination' in the treatment of lower class patients. They attribute this primarily to economic considerations and to value-differences between patient and therapist, and then go on to suggest ways of making psychotherapy more available to the masses. In doing so they tacitly assume: (1) that psychotherapy is an effective form of treatment; and (2) that it is effective for everyone, regardless of class. Before large sums of public or private funds are expended in promoting psychotherapy for the masses, there should be a good deal more empirical support for these assumptions than is available at present. For one thing, the authors' own data can be readily used to argue that not only is the practice of psychotherapy class-linked, but so also are the 'principles.'

Similarly, their suggestions that we are in need of 'less expensive' therapists and perhaps even a new profession of 'lower class therapists' are not very convincing. There is no assurance that psychologists or social workers (two of the possibilities mentioned) would be able to 'break the value-barrier.' This is apart from the question of whether psychologists or social workers would be willing to assume such a function. Anyway, before such professional developments make any sense at all, we should know that psychotherapy 'works,' and not just with adherents to the Protestant ethic.

It should be made clear that none of these comments is intended to derogate the substance of Hollingshead and Redlich's work. This is a provocative and important book. It would be notable for no other reason than the fact it makes abundantly clear that the therapist works in a socio-cultural setting and must not confine his attention solely to 'intra-psychic' elements. The book does leave many questions still unanswered and certainly should not form the sole basis for extensive reform-movements.

Washington University RAYMOND G. HUNT

Perception in Present Day Psychology: A Symposium. WILLLIAM C. BIER, SALVATORE G. DIMICHAEL, and RAYMOND J. MCCALL, Editors. New York, American Catholic Psychological Association, 1956. Pp. viii, 109. $1.00.

The papers comprising this volume were presented at the tenth annual meeting of the American Catholic Psychological Association that was held in conjunction with the annual meeting of the American Psychological Association in 1956. The papers are divided into three parts: (1) basic research findings; (2) philosophical implications; and (3) psychological implications.

Part 1 on basic research findings, is a 21-page review of the literature of perception by Sister Marian Dolores. Though the reviewer is undoubtedly competent in her knowledge of the area, one often wishes she had cited fewer articles and dealt with them more comprehensively, since a student unfamiliar with the material would find the explanations often incomplete and sketchy. Too much has been attempted in the space available. This is fine for anyone familiar with the field who wished a review of concepts, but it is of little value to the beginner.

Part 2, on philosophical implications of perceptual theory, is presented by John Stafford as a treatise on the problem of veridicality in perception as related to the problem of truth in epistemology. Here, as elsewhere, the scholastic view directs the treatment of the problem toward a philosophic discussion of this position. One conclusion reached is that philosophers might profit from perception-theorists through being induced to expand their analyses to include purpose and value—the *purpose* of perception being adaptive and thus of functional *value.*

Part 3, on psychological implications of perceptual theory, is subdivided into implications for (1) experimental psychology, (2) clinical psychology, and (3) social psychology. The first paper, by Mary Reuder, was the most interesting to the reviewer. Dr. Reuder is keen in her criticism of many current perceptual theories. Generally, she advocates elaboration and extension of theories to the point that crucial experiments may be designed in the areas in which the theories overlap with one another. Current theories are too specific and piecemeal to permit this possibility. After describing the designs of a few researches and indicating where further work is needed, she concludes by proposing that perception may profitably be viewed in "terms of 'levels' much as many persons, *e.g.* Razran, today view learning." The second paper, on implications for clinical psychology by Charles Curran, presents some interesting case histories which show a relation between increases of the patient's 'insight' in therapeutic counseling and increases of objectivity or breadth of the patient's perception of his problems. The final paper, on implications for social psychology by Rudolf Allers, is an historic review of the problems of social area as related to perception. An interesting question is raised: Is adjustment, in its broad sense, a danger to individuality in that adjustment leads to stereotypy of people? The handling of the question left something to be desired, and one wonders if perception has any direct relation to the issue.

University of Texas MILTON A. WHITCOMB

Education of Exceptional Children and Youth. WILLIAM M. CRUICKSHANK and G. ORVILLE JOHNSON, Editors. Englewood Cliffs, New Jersey, Prentice-Hall, 1958. Pp. xiv, 723. $6.95.

This book, a collective effort by various experts in its field, was aimed by its editors to bring current research in the education of exceptional children and youths to the attention of a broad field of readers—to special and regular teachers, to administrators, to parents, and to graduate and undergraduate students of psychology and education. Its coverage of subject matter is as broad as its aim. It treats of the intellectually different, both high and low; the physically different, including deviations in sight, hearing, and speech; the crippled and chronically ill; and the emotionally and socially disturbed.

It is a long book, as it had to be to accomplish its assigned task. Its various chapters are richly documented by footnotes and bibliographies and each chapter is closed by a summary of the wealth of material included within it. It is not, however, a handbook but a practical guide for readers interested in its field.

As is perhaps inevitable in such a large book, some startling statements, or misstatements, appear and some strange omissions have been made. For example; on p. 46 the text reads: "All are in agreement that the state must assume responsibility for the training, care, and treatment of children with intelligence levels below 50 or 55." There are many people in many states, communities, and school systems who do not today accept that socialistic point of view. The discussion (pp. 166 ff.) of the advantages of grouping children with high mental ability is one-sided; the disadvantages are not mentioned, as they should be in an unbiased treatment. The chapter on the socially maladjusted and the emotionally disturbed is incomplete in that it considers only the causes related to the lower socio-economic and so-called 'minority' groups. The work of some of the leaders in this field—Doll, Brandwein, Worcester, Laycock, Gates, Meister, to mention but a few—is omitted. So also are the projects conducted jointly by the National Education Association and the North Central Association on the academically talented.

The organization and general format of the book are excellent. The editing on the whole was well done as the various chapters, though written by many hands, are tightly and smoothly coordinated. All in all, this book is a real contribution to the rapidly growing field of professional education.

Arizona State College WILLARD ABRAHAM

Mental Health in College and University. By DANA L. FARNSWORTH. Cambridge, Massachusetts, Harvard University Press, 1957. Pp. ix, 244.

According to the author, who is a professor of Mental Health and Director of University Health Services, Harvard University, this book represents an attempt to convey a point of view about the interaction of psychiatry and education which will be most valuable for the educators, parents, and the interested psychiatrist. The contents embrace a skillfully presented advocacy of college mental health programs, a discussion of factors related to the emotional maturation and emotional stress problems of undergraduate and graduate students, a relatively orthodox definition of the psychiatrist's role vis-a-vis the administration and the student, and the contributions which can be made by academic faculty and deans to insure better mental health in colleges and universities.

The psychologists and professional counselors are likely to deplore the absence of any serious discussion of their functions, or of the role of the counseling service, on the college campus. Their importance as a possible source of specialized services and as possible promoters and organizers of a mental health program, is vaguely referred to, here and there, throughout the book, but their function in the overall mental health program is not spelled out with the same thoroughness as, for example, the role of the psychiatrist or the faculty. In general, the system of counseling described is one in which the faculty member assumes the primary role with the advice and aid of the college psychiatrist.

institutions of higher learning must take emotional factors into account in planning

The book is very readable, with a liberal sprinkling of case histories and brief summaries of published research marshalled to support the author's view-point that their programs.

Washington University — DAVID J. MARKENSON

The Fabric of Society: An Introduction to the Social Sciences. By RALPH ROSS and ERNEST VAN DEN HAAG. New York, Harcourt, Brace, 1957. Pp. xv, 777.

This wide-ranging volume is an introduction to personality psychology, sociology, philosophy of science, economics, and political science. It is best suited for mature students, introducing them at an advanced level to a substantial segment of social science. In a way it is hard to fit into the American college curriculum as a text; this may be a fault of our colleges more than of the book. It demands a breadth and depth of reading that is unusual in freshmen, and yet is so inclusive in its coverage as almost to overflow the boundaries of a single course for seniors. But for independent reading and discussion, outside the restrictions of the customary course program, it is stimulating and provocative.

The aspects of social science that one meets here are largely the speculative, rather than the more rigorously scientific. Few examples are chosen from experimental and statistical studies; much more space is devoted to intelligent speculation, and to illustration of hypotheses by literary criticism ranging from the Greeks to the present. The approach to personality is largely Freudian. One begins to recognize the style, background and approach of the two authors in their respective sections: van den Haag, for example, exemplifying a more European concern with the decline of high culture, continually tracing terms to the Greek or Latin roots from which they arise; Ross conducting a more precise and less literary analysis of science, logic, morals, and free will. Both authors show throughout the book a central concern for the problems confronting modern society: cultural homogeneity, the performance of capitalism, international politics, the functioning of democracy. They make value judgements, take controversial positions and support them with skill. For a mature student with time to read, reflect and argue, this book can be very rewarding.

University of Chicago DUNCAN MACRAE, JR.

Vorlesungen über Psychologie. By C. G. CARUS. Edited by Friedrich Arnold. Darmstadt, Germany, Hermann Gentner, 1958. Pp. 460.

As various commentators have observed, a wave of romanticism spread over Europe after World War II. This is especially true in Germany where several books by leading 'philosophers of nature' of the 1830s have been republished. Among these is Carus' *Vorlesungen über Psychologie,* edited by Dr. Friedrich Arnold, that were originally given at Dresden in 1829-1830.

Following the method of Goethe and Schelling, Carus discusses evolution from the point of view of mind-body theory. He, as do all the romanticists of his generation, uses a loose type of philosophical speculation for which there was no solid empirical foundation. Mental life, as Carus states, cannot be observed through the 'outward senses,' *i.e.* vision, hearing, etc. We must enter regions of the 'inner senses' if we are to understand the mind in a soulful way. The world we perceive through our outward senses is called *nature.* We get knowledge of our mental development by an analogical investigation of events and occasions in nature. Carus approved of the genetic method of investigation and used it to demonstrate the insufficiency of the descriptive and analytical methods.

The evolution of all living forms in nature as well as the inner maturation of various mental creations (*e.g.* a musical composition) shows, as Carus believes, the preëxistence of a higher idea—a divine primary phenomenon. This divine idea manifests itself in the evolutionary history of all souls and displays itself in three stages: (1) unconscious souls; (2) world-conscious souls; and (3) self-conscious souls. Some animals are conscious of the world but only man is conscious of himself. It was upon this plan that Carus built his system of psychology: imagination, memory, intellect and volution, respect for others, individual liberty, love, and wisdom. His speculations are supported by examples, analogies, and symbolic suggestions, not by empirical observations.

As far as present-day psychology is concerned, Carus' *Vorlesungen* are anachronisms. They are, however, of historical interest as they reveal the similarity between the tenets of the romantic philosophers and those held today by practitioners of psychsomatic medicine and psychoanalysis.

University of Helsinki KAI VON FIEANDT

The Young Handicapped Child: By AGATHA H. BOWLEY. With a section on "The Young Deaf Child" by L. GARDNER. Edinburgh & London, E. & S. Livingstone, 1957. Pp. v, 128.

This little book grew, in the author's own words, "rather like Topsy" out of her extensive educational and clinical experience with blind, cerebral palsied, and deaf children. The author's chief goal is to make intelligible to parents and teachers some of the literature having implications for daily practical situations and formal classroom programs. She does not propose to produce a scientific treatise, and the product should be judged accordingly. Throughout the book the author buttresses her premise that any successful training program will relate ultimately to attitudes toward pain and adversity held by responsible adults. While some readers may object to the philosophy of life expressed in the book and given a prominent role in the exposition, no one could object to the warmth and good feeling for all handicapped persons which the author so obviously possesses. The book should be an excellent resource for parents' groups concerned with the different types of handicap, but the student and professional worker would probably be disappointed.

School of Medicine, Washington University BETTYE M. CALDWELL

The Medical Interview: A Study of Clinically Significant Interpersonal Reactions. By AINSLIE MEARES. Springfield, Illinois. Charles C Thomas, 1957. Pp. IX. 118. $3.50.

It is not clear for whom this book is written. It is a description of how to conduct an interview with a patient. The author, using a good deal of psychological jargon, slants this very basic medical procedure to the psycho-therapeutic side. It is probably not possible to teach a doctor how to interview patients successfully by means of a short book but the author is clearly sincere in his effort to help his patients indirectly in this way.

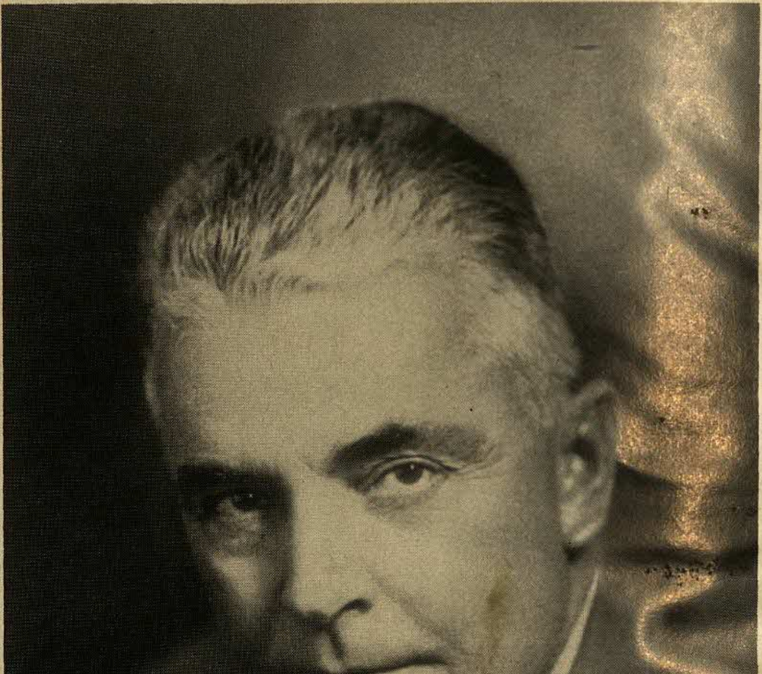
Headings to sections are written often in journalese ("Avoid game of chess approach"), and this is wearing. If the general practitioner takes much of the book to heart, he will be so conscious of harm he may do, and of pitfalls, that the resultant interview must be a little sterile and cold—but there is a whole chapter on Silences.

University of Louisville Medical School FRANK FALKNER, M.D.

Basic Psychology: A Study of the Modern Healthy Mind. By LEONARD CARMICHAEL. New York, Random House, 1957, Pp. 340. $3.95.

This book, for the non-technical reader, is intended to be an introduction to physiological psychology, and covers the core topics of mental life considered in relation to bodily processes. In addition, chapters are devoted to values, economics, marketing, and adaptive social life. In these chapters "crude oversimplified mechanistic philosophies" are rejected, and the values of American society are defended. In general *Basic Psychology* relies heavily on definition, example, and brief description of the views of historically important individuals.

Air Force Personnel and Training Research Center CHARLES W. BRAY



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(*See page 301*)

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DEPTH PERCEPTION OF STEREOSCOPIC IMAGES RESULTING FROM FUSION OF CROSSED AND UNCROSSED DOUBLE IMAGES

By J. L. ZAJAC, University of Edinburgh, Scotland

In the study of binocular vision, the existence of double images has long been known. An object lying farther from *O* than the fixation-point gives an uncrossed double image; an object lying nearer to *O* than the fixation-point gives a crossed double image. When the fixation-point, the near object, and the far object, all lie in the median plane, it is possible, by adjusting the distance of the fixation-point, to obtain a condition in which the crossed images of the near object occupy the same lateral position as the uncrossed images of the far object. The result is that the number of images is reduced from four to two; one image lying to the right of the fixation-point and one to the left. This overlapping was suggested as a possibility by Bourdon.[1]

In observing those fused double images, the writer has found that they appear at a definite distance in comparison with the fixation-point and that they give an impression of relief which is different from that of the objects used. If, for example, the objects observed are vertical steel rods of like diameters, then the fused double images of these rods are seen as two flat, upright surfaces extending away from *O* and converging toward each other at their more distant ends (Fig. 1). It is as though there were two short 'walls,' 'ribbons,' or 'strips,' one on each side of the fixated rod and inclined toward the median plane. These images are stereoscopic in character because they are produced by the fusion or overlapping of two images of two different objects formed on the two retinas. To the best of the writer's knowl-

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* Received for publication November 23, 1957. My thanks are due Professor James Drever for permission to conduct this investigation in his laboratory, to the Senatus Academicus of the University of Edinburgh, the Committee of the Earl of Moray Endowment Fund, and the Executive Committee of the Carnegie Trust of the Universities of Scotland for grants-in-aid which made this study possible, to Captain W. Bogucki, who constructed the apparatus used in the experiments, and to the many students who served as *Os*.

[1] Benjamin Bourdon, *La perception visuelle de l'espace*, 1902, 294.

edge, this stereoscopic phenomenon has not been previously reported. It was not reported by Bourdon, who, as has been mentioned above, noted only a possibility of overlapping of crossed and uncrossed double images. Neither was it observed by Aall, who experimented on depth-perception of double images with a similar disposition of objects in the median plane.[2]

The position of these stereoscopic images is steady in regard to distance. They are, however, subject to the phenomena of rivalry but that does not change their perceived distance, though it frequently alters their general appearance and relief. The practiced *O* is able to see the stereoscopic images without a fixation-point. He may also move his eyes about the 'walls,' from one 'wall' to the other, or along the width of the 'walls,' without changing the general appearance of the images. This may be explained by the tendency of the double images to blend into one image by the process of fixation or of stereoscopic fusion.

One very striking observation was made in a study of the width of the 'walls.' When the conditions of brightness are such that a thin dark image of the farther rod appears on the brighter surface of the wider image of the nearer rod and the fixated rod is placed about the middle of the 'walls,' and when *O* moves his eyes along the 'walls,' the following phenomena occur: when *O* diverts his eyes to the back of the 'wall,' the thin dark line of the far rod is seen at the inner part of the 'wall' and behind the fixation-point; when he moves his eyes along the 'wall' toward the nearer edge, the thin distinct image of the far rod is displaced to the outer edge of the 'wall,' and, at the same time, is seen as if moved to a position nearer than the fixation-point. Similar phenomena are observed when rods of different colors are used. In the first case, the convergence and accommodation of the eyes are beyond the fixation-point; in the latter case, they are in front of it. The same changes of the relative positions of the two images fused stereoscopically in the conditions described above occur also when the fixated rod is moved from the distance at which it is seen as equidistant with the farther edges of the 'walls' to a position where it is seen as equidistant with their nearer edges. These phenomena may be regarded as *crucial* for stereoscopic vision because they are in conformity with the principle of retinal disparity.

It should be noted that the images are steady only in the positions mentioned, *i.e.* when two edges of the two images coincide. In other positions, *e.g.* when one image covers the interior parts of the other, they are very unsteady.

The phenomena of stereoscopic vision resulting from placing three objects in the median plane are very interesting because this symmetrical arrangement: (1) enables one to gain a better insight into the relations and differences between monocular and binocular vision; (2) permits one to define the role of the different factors involved in the perception of depth—namely, interocular distance, convergence, accommodation, and size and shape of the pupil; and (3) leads one to hope that, with more precise apparatus, a simple method of diagnosing the differences between the two eyes may be devised.

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[2] Anathon Aall, Ueber den Massstab beim Tiefensehen in Doppelbildern, *Z. Psychol.*, 49, 1908, 108-127; 161-205.

METHODS USED IN EARLIER EXPERIMENTS

In earlier experiments, three rods, placed in the median plane, were viewed by *O* through a slit, 3.25 in. wide and 0.5 in. high. For stability, *O* rested his chin on a support. The experiments were carried out in a darkened room illuminated by a 100-w. electric bulb placed 4 ft. above the slit. The middle rod was so placed that, when it was fixated, the centers of the stereoscopic images of the other two rods were seen as equidistant from it.

By changing the distances of the rods, their size and the colors of the filters through which the rods were viewed, an attempt was made to establish the following facts regarding the stereoscopic images: (1) distance from the eyes and differences in distance, if any, of the left and right images; (2) the lateral distance of the images from the fixated rod; (3) relief, shape, width, and angle of inclination to the median plane; (4) brightness; and (5) distinctness of the images. Some of these features were measured, others were estimated, by the *O*s. The results obtained were compared with those seen monocularly by closing one and then the other eye.

(1) Depth and lateral distance from the fixation-point. The place of the steroscopic images is geometrically defined by the intersection of the visual lines connecting the centers of the eyes with the corresponding stereoscopically fused objects. This distance, *Y*, can be ascertained by so placing the fixated rod that it is seen as equidistant with the middle portions of the stereoscopic images. It is given by the formula

$$Y = \frac{2Y_A Y_B}{Y_A + Y_B} \tag{1}$$

obtained from the geometrical construction, where Y_A is the geometrical distance of the near rod, and Y_B is the distance of the far rod (Fig. 1).

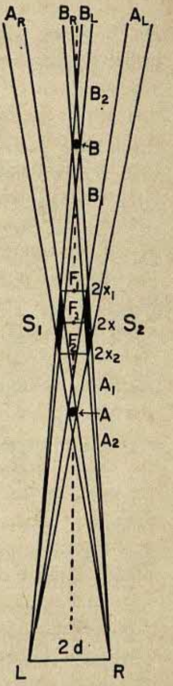

FIG. 1. FORMATION OF STEREOSCOPIC IMAGES IN FORM OF 'WALLS'
(Three rods in median plane)
L = left eye; R = right eye; AB = the two rods; S_1, S_2 = stereoscopic images; F, F_1, F_2 = fixation-points; A_1, A_2, B_1, B_2 = points of intersection of visual lines with the median; 2d = interocular distance; X, X_1, X_2 = lateral distances of different parts of S_1 and S_2 from the median plane.

It has been found that this formula gives the correct distance of stereoscopic images in all cases under investigation, and not only when the 'walls' appear. This proves that the stereoscopic fusion, even of not very similar images with respect to blurredness, brightness, or color, is a sufficient factor for seeing the distance of the image as compared with the distance of the fixation-point. With different degrees of blurredness, only the general appearance and the width of the stereoscopic images are changed.

Thus it may be concluded that blurredness, as a result of a state of accommodation, is not the deciding factor affecting the perception of distance in stereoscopic vision in the near field view. The results obtained by measurement during the experiments very nearly approach the distances calculated from the formula.

The lateral distance, X, from the fixation-point may be calculated from any of the following formulas:

$$X = \frac{d(Y_B - Y_A)}{Y_A + Y_B} = \frac{d(Y_B - Y)}{Y_B} = \frac{d(Y - Y_A)}{Y_A} \qquad [2],$$

where Y, Y_A, and Y_B have the same meaning as before, and $2d$ denotes the interocular distance. The values for X given by the formulas are verified by experiments.

(2) Distance between the stereoscopic images. If the rods are placed precisely in the median plane and if there is no difference in accommodation between the two eyes, the two images are of the same shape, the same width, and the same distance from the eyes. When these conditions are not fulfilled, differences occur in those different features, especially differences in distance of the stereoscopic images. The necessary condition for seeing the images at the same distance is the equality of the visual angles subtended at both eyes by the two objects that are fused in stereoscopic images—this is another form of the law of intersection of visual lines. If these angles are unequal, then the distance of the stereoscopic images from the eyes is different; when the angle subtended at the left eye is greater than at the right eye, then the left image is seen farther away than the right one (Fig. 2).

Differences in distance may be due to various causes; but in every case when we are able to ascertain, by closing one eye, that the visual angles are different, then the stereoscopic images appear at different distances. First of all, the whole image might be seen nearer and this might be attributed to changed conditions of accommodation, *e.g.* when a glass filter (colored or clear) is put in front of one eye. This also occurs, when the angle of inclination of the glass filter is changed. If the glass filter is placed in front of the right eye and turned with its inner edge toward the nose, the stereoscopic images and the fixated rod are shifted to the right and recede from O; but the right image moves least, hence it is seen as nearest. When the filter is turned away from the nose with its inner edge, all three images shift to the left and approach O, but the left image is now seen as nearest. Some observations were also made in binocular vision when different color filters were placed in front of each eye. In these investigations it was not intended to investigate the effect of color on depth-perception; the colors were used only to obtain different conditions of accommodation for each eye and to examine their influence on the features of stereoscopic images. Different color-filters give differences of refraction in the media of the eye because of differences in wave-length of light transmitted through them and thus change the conditions of accommodation for corresponding eyes.

Difference in depth could also be observed when, on one side of the fixation-point a 'wall' was seen, and on the other a rod, a thin line, or a 'pillar,' appeared. In this case the 'wall' was wider than the stereoscopic image on the other side. It happened several times that, if the nearer edge of the 'wall' was seen nearer than the nearer edge of the image on the other side, the centers of both stereoscopic images were seen as equidistant with the fixated rod and with one another.[3]

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[3] These various investigations were reported in (a) author's unpublished doctoral dissertation: Depth perception in indirect vision, University of Edinburgh, 1951;

PRESENT EXPERIMENTS

The experiments described in this paper were carried out with the author's "natural stereoscope."[4] They are as follows.

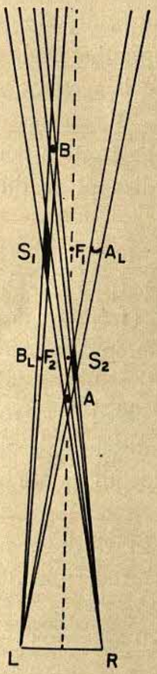

FIG. 2

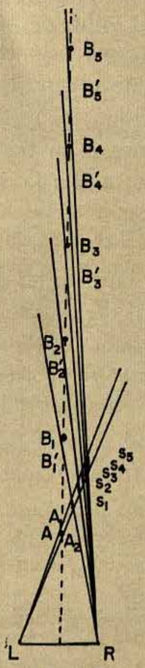

FIG. 3

FIG. 2. EFFECT WHEN ONE ROD IS DISPLACED FROM THE MEDIAN PLANE
(Notation the same as in Fig. 1)

FIG. 3. EFFECT OF VARIATION OF THE DISTANCE OF ROD B
(Notation same as in Fig. 1)

Experiment I. A stationary rod, 6 mm. in diameter, was placed at 41.4 cm. in front of *O* and in his median plane. The position of the farther rod of the same diameter was varied, by steps of 10 cm., from 70 to 150 cm. from him. When a fixated rod is introduced between them at an appropriate distance, one can see two stereoscopic images, frequently seen as 'walls,' with their farther parts inclined towards the median plane. By moving the fixa-

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(b) Annual Reports of the Polish Society of Arts and Sciences Abroad, 1951-1952 and 1953-1954; and (c) Demonstrations concerning the visual depth perception, Preliminary draft, Edinburgh, December, 1954.

[4] J. L. Zając, Depth perception of double images in the vicinity of other images, *Acta Psychol.*, 12, 1956, 111-129.

tion-point, one can easily measure the geometrical distances at which the nearer and the farther edges of these stereoscopic images are seen as equidistant with the fixated rod from *O*. Differences between those two measurements represent the geometrical length of the stereoscopic images (width of the 'walls,' or rather its projection on the median plane, Fig. 3).

With increased geometrical distance of the farther rod, four various phenomena are observed: (1) The stereoscopic images move away from *O*, *i.e.* Y_F grows; (2) their length (width) Y_{F1}-Y_{F2} increases; (3) their lateral distance from the median plane and the fixation-point increases, *i.e.* X grows; and (4) inclination to the median plane diminishes, *i.e.* X_2-X_1 lessens.

The results of experiments with one *O* (*C*) are given here. The nearer rod, 6 mm. in diameter, was placed at 39.3 cm. from *O*. The distance of the middle of the 'walls' increased by 11.15 cm. (from 51.35 to 62.5 cm.), when the distance of the farther rod (also 6 mm. in diameter) was moved from 70 to 150 cm. from *O*. The width of the stereoscopic images increased from 2.9 to 7.1 cm., *i.e.* more than doubled. By geometrical construction and computation, the difference in distance should have been 12.67 cm. (from 50 cm. to 62.67 cm.) but the length should have increased only by 50%.

Results of experiments with another *O* (*D*), in which the nearer rod was placed at 48.1 cm. and the farther rod was moved from 80 to 160 cm., were as follows: the change in distance was 20.78 cm. (from 59.22 to 80.0 cm.) and the increase in length of the 'wall' was 7.95 cm. (from 2.25 to 10.2 cm.).

Results of two other *O*s, (*A*, with and without glasses, and *B*), are given in Fig. 4.

Experiment II. Similar investigations were carried out in Experiment II with a nearer object (a rod 6 mm. in diameter) placed at 41.4 cm. and the farther object (a cardboard strip 1.75 cm. wide), moved from 80 to 157 cm. from *O*. This case differs from the one just described since in it the farther object was always seen as thinner because of its greater distance, while in this case a strip 1.75 cm. wide was seen as larger than the rod up to a distance of 110 cm.; as equal at 120 cm.; and as narrower at farther distances. The result was that, at distances of the strip from 80 to 110 cm., the stereoscopic images in form of 'walls' were seen inclined to the median plane with their nearer edges; at 120 cm. no 'walls,' but rods were seen; and at greater distances of the strip, the 'walls' were inclined to the median plane with their farther ends. When the farther strip was moved continuously from 80 to 157 cm.; one *O* reported that (a) the stereoscopic images moved away from him; and that (b) the stereoscopic images rotated from a position at which they were inclined to the median plane with their nearer edges to a position where they increased their inclinations with their farther edges. (To obtain the effect of rotation without changed distances, the rods or strips of appropriate size and shape should be moved along the same visual axis of one eye, *i.e.* along the same visual direction and not in the median plane.) The results of these series of investigations are given in Table I and in Fig. 5.

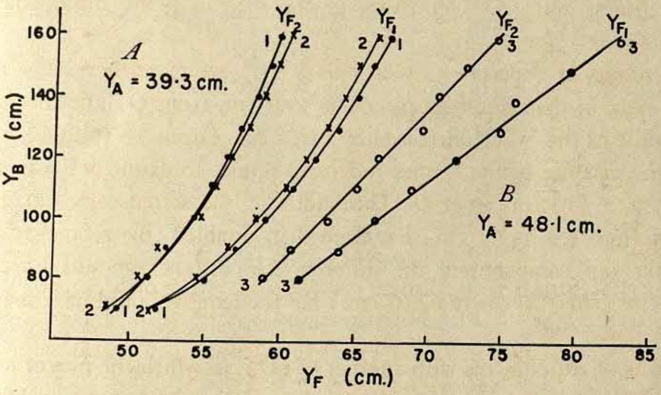

FIG. 4. DISTANCES OF NEAR AND FAR EDGES OF THE STEREOSCOPIC IMAGES AS A FUNCTION OF POSITION OF THE FARTHER ROD

Curves 1 = Observer *A* without glasses; Curves 2 = Observer *A* with glasses; Curves 3 = Observer *B;* Y_{F1} = position of far edges of 'walls'; Y_{F2} = positions of near edges; Y_F = distance of the fixation-point; Y_A = distance of the nearer object; Y_B = distance of the farther object.

(3) Still other experimental variations were made which consisted in changing the diameter of the further rod while the size of the nearer remained unchanged, and vice versa, but without changing the geometrical distances of the two objects used (see Figs. 6 and 7). Table II gives the results for Observer *C* when the nearer rod was placed at a distance of 39.3 cm. and the farther rod at a distance of 123.5 cm. Conclusions from

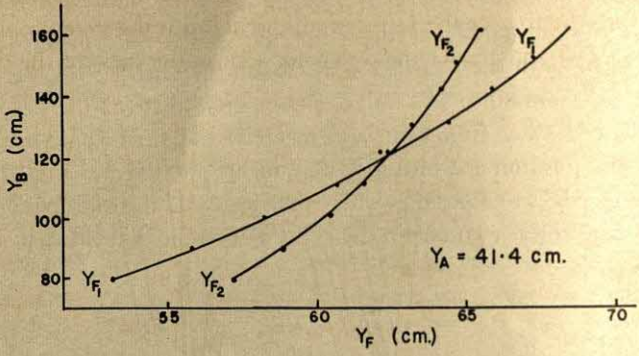

FIG. 5. DISTANCES OF NEAR AND FAR EDGES IMAGES AS A FUNCTION OF POSITION OF THE FARTHER OBJECT WHEN IT WAS A STRIP
(Notation same as for Fig. 4)

TABLE I

RESULTS OF EXPERIMENTS IN WHICH THE NEAR OBJECT IS A ROD AND THE FARTHER OBJECT A STRIP

(Width of strip, 1.75 cm.; diameter of rod, 6 mm.; distance of rod, 41.4 cm.)

Distance of strip	Distance stereoscopic image*	Width of image*
80 cm.	55.25 cm.	4.05 cm.
90 cm.	57.50 cm.	2.90 cm.
100 cm.	59.47 cm.	2.15 cm.
110 cm.	61.20 cm.	1.00 cm.
120 cm.	62.31 cm.	0.25 cm.
130 cm.	64.45 cm.	0.90 cm.
140 cm.	65.21 cm.	1.62 cm.
150 cm.	66.10 cm.	2.50 cm.
157 cm.	67.25 cm.	2.90 cm.

* Means of 3–5 measurements.

TABLE II

RESULTS OF EXPERIMENTS IN WHICH THE DIAMETERS OF THE RODS WERE VARIED

(Distance of nearer rod, 39.3 cm.; of farther rod, 123.5 cm.)

Diameter nearer rod	Diameter farther rod	Distance of middle of stereoscopic image*	Width stereoscopic image*
3 mm.	3 mm.	60.50 cm.	3.30 cm.
3 mm.	6 mm.	59.47 cm.	1.95 cm.
3 mm.	8 mm.	59.22 cm.	1.05 cm.
6 mm.	3 mm.	61.33 cm.	5.70 cm.
6 mm.	6 mm.	60.60 cm.	4.00 cm.
6 mm.	8 mm.	60.32 cm.	2.15 cm.
8 mm.	3 mm.	61.60 cm.	8.50 cm.
8 mm.	6 mm.	60.90 cm.	5.80 cm.
8 mm.	8 mm.	60.32 cm.	2.15 cm.

* Means of 3–5 measurements.

this series of experiments are as follows: (a) with fixed diameter of the nearer rod, the thicker the farther rod, the nearer is the stereoscopic image seen; and (b) the greater the difference seen in thickness of the two rods or strips, the greater the length of the stereoscopic images—*i.e.* width of the 'walls.' These conclusions are similar to those drawn from the previous investigations.

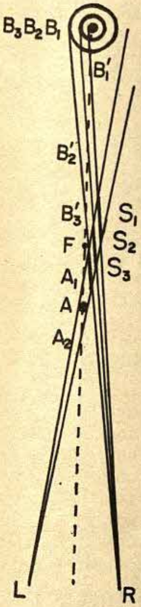

FIG. 6

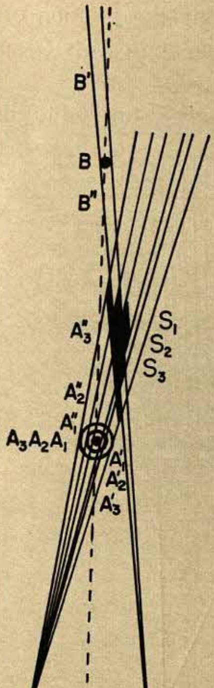

FIG. 7

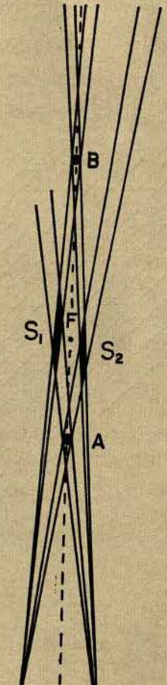

FIG. 8

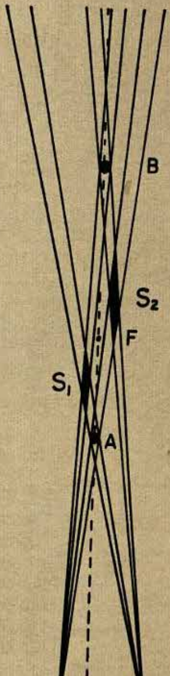

FIG. 9

FIG. 6. IMAGES FORMED WITH DIFFERENT DIAMETERS OF THE FARTHER ROD
(Notation same as for Fig. 1)

FIG. 7. IMAGES FORMED WITH DIFFERENT DIAMETERS OF THE NEARER ROD

FIG. 8. IMAGES WHEN *O*'S HEAD IS ROTATED

FIG. 9. IMAGES WHEN *O*'S HEAD IS MOVED TO THE RIGHT

The experiments described above were carried out when objects were placed in the median plane and *O*'s head was held in a symmetrical position. When this position was a asymmetrical or when the objects were placed outside the median plane, *O* perceived changes in the distances of the stereoscopic images and in their relation to one another. These changes were as follows.

(1) When objects are placed in the median plane and *O*'s head is turned to the right without changing the position of the center of the rotation, the right stereoscopic image approaches *O*, and the left one recedes (see Fig. 8).

(2) When *O*'s head is moved, without turning to the right of the principal position, the right image recedes and the left one comes nearer. (Fig. 9.)

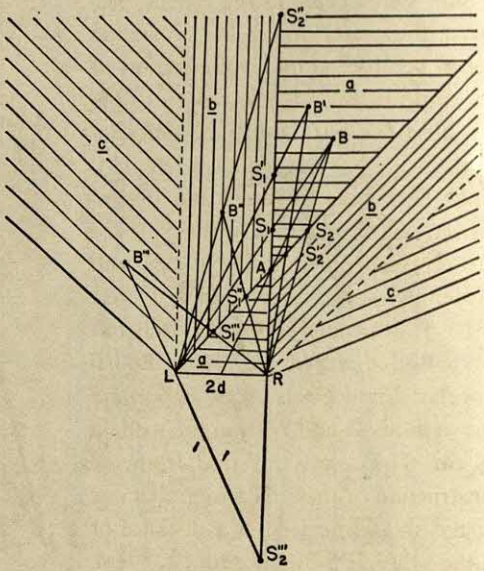

FIG. 10. DIAGRAM ILLUSTRATING POSITIONS OF THE STEREOSCOPIC IMAGES FOR DIFFERENT ASYMMETRICAL POSITIONS OF THE RODS

(3) When the objects are placed outside the median plane, we can predict from the law of intersection the following results (Fig. 10): (a) When two objects are placed in the vertical plane passing through the middle of the interocular distance, the two stereoscopic images are seen at the same distance from *O*. The sizes and shapes of the images might be different, especially when objects are placed at farther lateral positions (distances) from the median plane, because then the sizes of images in both eyes must differ from one another. (b) When the second object is placed within Area *a*, comprised by visual lines connecting both eyes with the first object, two stereoscopic images will be seen at different distances from *O* but

between the perceived distances of the two objects when they are fixated. (c) When the second object is placed outside Area *a* but within Area *b*, comprised between the visual lines for the first Object *A* and the lines through *L* and *R* and parallel to those visual lines, the place of the two stereoscopic images can be geometrically defined for one as nearer than the nearer object and the other as farther than the farther object. (d) When the second object is placed in Area *c* outside Areas *a* and *b*, only one stereoscopic image can be produced nearer than the nearer object, but no second one could be seen because no other intersection of the visual lines could be obtained in front of *O*—they intersect behind *O*'s head and thus could not be perceived.

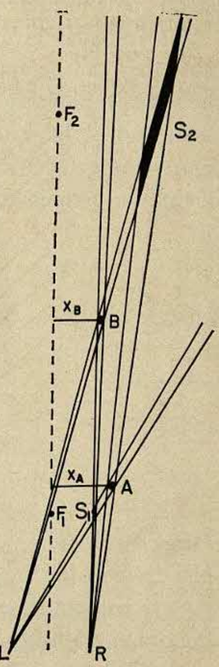

Fig. 11. Experiment 3c in Which One Stereoscopic Image is Formed Nearer than the Near Rod (A) and the Other, Farther than the Far Rod (B)

These findings have been analyzed and the investigations carried out on the basis of their proved validity. Fig. 10 represents these different cases and different areas defined above in relation to Object *A*. One result, (3c), illustrated in Fig. 11, is as follows: Distance of Rod *A* was 60 cm. and of Rod *B* 110 cm. Interocular distance was taken as 60 mm. Lateral distance of Rods *A* and *B* from the median plane was 3.5 cm. The results inferred from the geometrical construction of these data were that one stereoscopic image should be seen at a distance of 57.5 cm., and the other at a distance of 122.5 cm. By computation, the corresponding values obtained were 58 cm. and 120.3 cm. The values read on the apparatus, as a result of an experiment, were 56 and 123.7 cm., respectively. Thus the error did not exceed 3–4%; not great, particularly when one takes into account, the fact that *O*'s real interocular distance was 64 instead of 60 mm.—which was chosen to simplify the geometrical construction. From the construction one could also predict the direction of the inclination of the 'walls,' as well as the fact, that the nearer rod was much shorter than the farther one—both of which were verified by observation.

A Theory of Stereoscopic Vision

In seeking an explanation and generalization of these results the author established various geometrical constructions illustrating the phenomena observed. In these constructs, no parameters are involved indicating the passage of the light rays into the eye nor their refraction due to the lateral position of the objects; the resulting error, however, was small: estimated, at a lateral angle of 10° from the fixation-point, to be about 15′ but the lateral angle in these investigations never exceeded 3°.

The formulas given at the beginning of this article are theoretical because they can be applied only to one-dimensional objects (the centers of the stereoscopic images), while the objects used here are three-dimensional —the 'walls' have their length and thickness, as described above. The figures and geometrical constructions illustrate this. The geometrical length of the stereoscopic images (width of the 'walls') is given by the magnitude of

$$Y_{F_1} - Y_{F_2} \qquad [3],$$

where

$$Y_{F_1} = \frac{2Y_{A_1}Y_{B_1}}{Y_{A_1} + Y_B} \qquad 4],$$

and

$$Y_{F_2} = \frac{2Y_{A_1}Y_{B_1}}{Y_{A_2} + Y_B} \qquad [5]$$

(see Fig. 1). The values of Y_{A1}, Y_{A2}, and Y_{B1} are distances on the median plane, where the visual line connecting the optical center of the corresponding eye with an edge of the object intersects the median plane. If we know the distance Y of the object from O, the radius x of a rod or half width of a strip and the interocular distance of O ($2d$), we can compute this distance from the formula:

$$Y_{A_1} = \frac{Y_A d}{d \pm X_A} \qquad [6],$$

or

$$Y_{B_1} = \frac{Y_B d}{d \pm X_B} \qquad [7],$$

where Y_A and Y_B are the geometrical distances of the objects A and B and X_A, X_B are the lateral distances of the edges of the objects from the median plane. This substitution in the case of stereoscopic images is possible because of the hypothesis verified by the facts that they are formed at the distance of intersection of the two visual lines and this distance is independent of the position and geometrical distance of the objects along these lines. The signs '+' or '−' in the denominator are used, depending on whether d and X are placed on the same or on the opposite sides of the median plane, in the first case '+' is used and in the second '—.' When the lateral distance X is greater than d and the '—' sign is used, then the distance of the point of intersection of the visual line with the median plane assumes a negative value. This point is then placed behind O, a fact which has an influence on the final result. The same can be applied to the value of Y_{A2} or formula for Y_{F2}.

The difference of the lateral distances of the edges of the stereoscopic images is

$$X_2 - X_1 \qquad [8]$$

where

$$X_1 = \frac{d(Y_{B_1} - Y_{A_1})}{Y_{A_1} + Y_{B.}} \qquad [9],$$

and

$$X_2 = \frac{d(Y_{B_1} - Y_{A_2})}{Y_{A_2} + Y_{B_1}} \qquad 10.$$

These two values: $\dot{Y}_{F1} - Y_{F2}$ [3], and $X_2 - X_1$ [8] illustrate the length of the images, their distance from the median plane and the direction and the amount of the inclination of the stereoscopic images to the median plane. These features can be named the 'relief' of the image, which in many cases is quite different from that of the objects used. This is then the geometrical measure of 'distortions' occurring in stereoscopic vision.[5]

This geometry of stereoscopic perception of depth is applicable only to the points of stereoscopic images in the horizontal plane passing through the centers of the eyes. For other planes, further more complicated computations are necessary.

The computations based on the formulas presented above and the geometrical constructions on which they are based, have not only been verified by further investigations, but also serve as means of forecasting distances and shapes of stereoscopic images as well as distortions brought about with stereoscopic vision.

A result of a stereoscopic fusion of images of different sizes is a distortion which we experience, for example, in stereoscopic films. We observe distortions also when we place different glasses in front of one or both eyes (Ames' Leaf Rooms) by using aniseikonic glasses or by changing the angles of turn of the glasses.[6] In the present investigations, they result from using two similar objects whose image-sizes are different because of differences in their perceived size (brought about by differences of geometrical sizes and by differences in geometrical distances *i.e.* by differences in visual angles). In the simple arrangement of the author's 'natural stereoscope' used for these experiments, they could be easily predicted and measured; this is much more difficult in the case of using artificial devices, such as stereoscopes, glasses of different kinds, etc.

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[5] All these formulas represent *geometrical* and not perceived features of the stereoscopic images.

LAWS OF STEREOSCOPIC VISION

The theory of stereoscopic vision formulated above is based mainly on three laws.

(1) The first law concerns the size and distance, from the median plane and the fixation-point, of double and monocular images of objects placed in the lateral parts of the visual field. According to this law, the size and lateral distance of these objects are directly proportional to the distance of the fixation-point from *O* (Fig. 12). It may be called the law of 'visual angle' and it may, like Emmert's Law, be applied also to after-images. Where more exact measurements are required, appropriate corrections must be made in connection with changes of refraction resulting from changes of eye-accommodation and fixation. These corrections are needed for size as well as acuity of the images. Blurred images are in general larger than sharp ones, but blurredness also brings changes in relief of the stereoscopic images. Differences in blurredness change the general aspect and shape of stereoscopic images in that they cease to appear as 'walls' or 'ribbons' and are seen as rods, 'pillars,' or thin lines. Three kinds of blurredness were observed: (a) the middle part was seen to be quite distinct and parallel lines were seen along the edges; (2) the middle part was seen to be quite distinct, only the edges were blurred; and (3) the whole image was more or less blurred. In the first two cases, the size of the distinct part of the image was related to the width of the walls.

(2) The second law, called the law of intersection, is the basis of depth-perception in stereoscopic vision. The stereoscopic images are formed at the intersection of two visual lines—one connecting the center of one eye with one object and the other connecting the other eye with the other object. (This law, which Panum described as an empirical law, is of great support to the projectionists' theories of depth-perception).[7]

For fixated objects, this law is easily understood, because at the point of fixation the visual axes intersect. When the images of two objects subtend the same angle at both eyes, their retinal images are formed at identical or corresponding points on both retinas. This corresponds geometrically to the horopter and in perception to seeing stereoscopic images at the same distance from *O* as the fixation-point. The notion of the identical or corresponding points (the anatomical-physiological factor) is related to geometrical conception of the horopter and to (the phenomenological or psychological factor) single vision of objects at the same distance from *O*. This relation must be considered as an hypothesis as no one has ever seen points on retinas which correspond to each other. Some investigators have questioned whether objects are always seen singly and at the same distance when corresponding retinal points are affected. The investigations of Lewin and Sakuma lead them to conclude that one can see objects at different distances whose images are formed on corresponding points of the two retinas.[8] This conclusion cannot, however, be proved directly as they overlooked one of the principal factors, *i.e.* accommodation.

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[6] W. H. Ittelson, *Ames's Demonstrations in Perception*, 1952, 53-62.

[7] P. L. Panum, *Physiologische Untersuchungen über das Sehen mit zwei Augen*, 1859, 80-81.

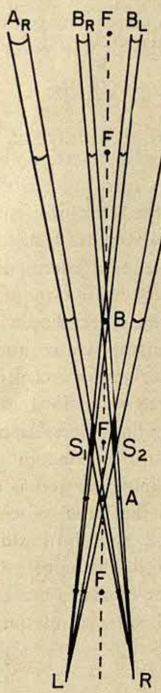

FIG. 12

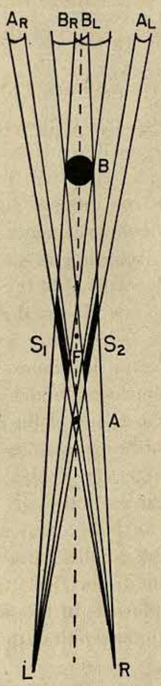

FIG. 13

FIG. 12. DIAGRAM ILLUSTRATING THE LAW OF VISUAL ANGLE
(Semi-circles denote the sizes and lateral distances of monocular and double images.)

FIG. 13. CASE WHEN THE FARTHER ROD IS SEEN THICKER THAN THE NEARER ROD

If we have two similar objects in natural setting (not using the stereoscope) and we wish their double images to be fused stereoscopically into one image, we must introduce a fixation-point so that the intersection will be situated in the neighborhood of horopter; only then will they fuse stereoscopically and the resulting stereoscopic image be seen at the same distance as the fixation-point, or nearer or farther according to the direction (temporal or nasal) of retinal disparity. Thus Hering's criticism of the projectionists' theories that stereoscopic images are seen at the intersection of visual lines has no foundation in fact because Hering did not take into consideration the fixation-point.[9] Stereoscopic fusion may occur in natural settings, when one becomes accustomed to fuse stereoscopically images of two objects without a fixation-point. In this case, one fixates directly the point of intersection and stereoscopic fusion follows.

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[8] Kurt Lewin and Kanae Sakuma, Die Sehrichtung monokularer und binokularer Objekte bei Bewegung und das Zustandekommen des Tiefeneffektes, *Pychol. Forsch.*, 6, 1925, 298-357.

(3) To the law of intersection two corollaries must be added. First, the distance of stereoscopic images depends on the place of intersection, but not on the geometrical distance of objects from *O*'s eyes. This corollary enables one to generalize the formula for distance from *O* and for the lateral distance from the median plane. This can be done by replacing the value for geometrical distance of objects of a certain thickness or breadth by computed distance of corresponding points in the median plane.

A second corollary concerns the 'relief' of the stereoscopic images with all the distortions. The 'relief,' which consists of length, width, and inclination to the median plane, and also distortions in stereoscopic vision, may be predicted and computed from these formulas. It looks as if the image seen as smaller (thinner) in one eye influenced the direction of the inclination and the elongation of the larger image in the other eye. When 'walls' appear, they are inclined to the median plane at their near or far ends. The first occurs when the nearer rod is seen under a greater visual angle (as thicker) than the farther rod; the latter, when the opposite occurs (see Figs. 1 and 13). This could be considered as a third principal law of stereoscopic vision, although it could be derived from both the other laws. The geometrical construction illustrates also the increase in length of stereoscopic images with the increase of differences of size between the images of one and the other eye. This increase in length is not related to any absolute value of the size of one or the other image alone.

DISCUSSION

From the results reported here, the following conclusions may be drawn concerning the role of interocular distance, convergence, and accommodation in stereoscopic vision.

The interocular distance in man varies between 56–70 mm. The greater this distance the greater is the lateral distance of the stereoscopic images from the median plane, the smaller is the length of these images, and the greater is their inclination to the median plane (see Figs. 14 and 15). Thus, the interocular distance influences the relief of the stereoscopic images, but it does not determine their median distances from *O*'s point of fixation.

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[9] Ewald Hering, *Beiträge zur Physiologie,* II, 1862, 132 ff.

Convergence is one of the principal factors in seeing and in locating stereoscopic images in the third dimension. This is due to the fact that, when we fixate an object, the visual axes of the two eyes intersect at the object, and the images of other objects are seen indirectly. Thus binocular disparity is determined and conditions favorable for stereoscopic fusion and depth-perception are obtained, or double images appear.

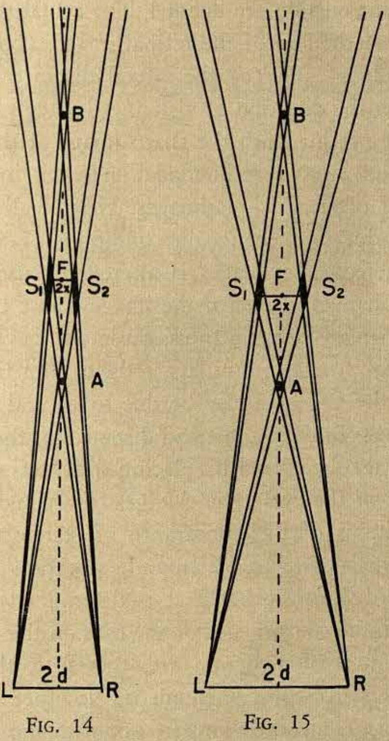

FIG. 14 FIG. 15

FIGS. 14 AND 15. EFFECT OF DIFFERENCES IN INTRAOCULAR DISTANCE

When we place different glasses (colored or clear) before our eyes, we change convergence. Under these conditions, fixation-points may change their appearance, *i.e.* become more or less blurred, be shifted sideways or change their relative distances from other objects, etc. Interocular distance also influences convergence. With greater interocular distance, convergence becomes greater and with it the possibility of binocular perception.

Another factor in the perception of depth is accommodation. It is strange that Helmholtz, who formulated the prevailing theory of accommodation, did not analyze its role in depth-perception of stereoscopic images.[10] Instead, he turned to his famous theory of unconscious inference.

The role of accommodation can be determined easily, if the main effects of changes in accommodation are defined. The actual state of accommodation determines: (a) the size of the retinal image, (b) the degree of distinctness or blurredness, and (c) the lateral distance from the image of the fixated object or the direction of vision. All three vary according to the position within the eye in which the sharp image of the object is formed. These three consequences of accommodation result in changes in shape, distance, and relief of stereoscopic images. First of all, if a change in the lateral distance is due to a change in conditions of accommodation, the distance of the stereoscopic images is altered because of the change of retinal disparity. Changes in size of the images cause changes in width of the 'walls;' and changes in distinctness, cause changes of the shape of the images: 'walls' may change into pillars, rods, or thin lines. Any difference in the images of the two eyes may be due to natural differences (aniseikonia), or to changes in the geometrical distances of the objects in the field of vision. It may also be caused by placing different glass-plates (colored or clear) in front of the eyes. The changes of accommodation may result in many modifications of the appearance of stereoscopic images. These modifications cannot, however, destroy the location of the stereoscopic images in the third dimension as that is determined by retinal disparity which results from the convergence of the eyes on the fixation-point.

Convergence and accommodation are, therefore, the principal factors responsible for the stereoscopic phenomena. No other factors, the so-called 'clues' or 'cues,' can compare in importance with those two in perception of distance and relief of the stereoscopic images in the near field of vision. It must be understood, however, that neither accommodation nor convergence is to be regarded in the sense of muscular effort or kinesthetic sensation. These sensations do not explain anything about depth-perception nor does the relating of the seen distance with the angle of convergence.

Because of the role of accommodation and convergence in depth-perception, one can divide the whole field of vision into three areas: (1) an area within the range of accommodation and convergence, in which discrimination of distance and relief is good; (2) an area within the range of practical convergence and outside the range of accommodation, in which the differences in distance are still well perceived but discrimination of reliefs is weaker; and (3) an area outside the range of accommodation and convergence. In the first two areas, there are differences concerning depth-perception between what we see with both eyes and what we see with only one eye; in the third area there is no difference between binocular and monocular depth-perception. Limits between these three areas are different for different eyes and different glasses used; also limited between Areas (1) and (2) might differ for the two eyes of the same *O*.

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[10] Herman von Helmholtz, *Handbuch der Physiologischen Optik,* English trans., J. P. C. Southall, ed., 3, 1925, 1-37; 281-369.

We now turn to some other general problem of depth-perception. The problem of depth-perception has been complicated by the use of concepts not adequately defined and by the lack of discriminative categories of spatial experiences. It is proposed to distinguish between the following three categories:

(1) Perception of depth and relief. The perception of depth and relief is concerned with the relative distance of objects in comparison with other objects and of their relief. These perceptions tell us whether some object, or some part of the object, in the field of view is seen as equidistant, nearer, or farther than other objects or other parts of the same object. In binocular vision near the fixation-point, this discrimination is more precise than in monocular vision and in more distant parts from *O*. These perceptions are 'immediate data' and 'certainty' is their main characteristic.

(2) Estimates. Estimates concern mainly judgments about 'absolute' distances of objects from *O* as well as quantitative evaluations of differences of perceived distances. These conscious or unconscious judgments are mainly brought about by experience; 'uncertainty' is their characteristic, but they can be learned and improved.

(3) Impressions of depth. Impressions of depth are experienced when we look at drawings, pictures, films, photographs, etc. Here factors intervene, such as perspective (both linear and aerial), lights and shadows, height, superposition, etc., which make them represent depth in two-dimensional media. Uncertainty and influence of different factors, such as attitude and attention of *O*, are features of this kind of visual experiences.

Another complication is the lack of precise discrimination among the different factors in depth-perception, *e.g.* accommodation, convergence, size and shape of pupil, and others, which some authors call empirical factors, viz. clues or cues.

A further generalization regarding depth-perception is that stereoscopic as well as monocular vision does not give what we would call the perception of an absolute distance or of relief. Stereoscopic vision produces a better and more precise (qualitatively different from monocular) perception of relative depth near the horopter, *i.e.* near the fixation-point. What we call absolute distance may be a subject for judgment and estimates, but not for perception.

Many misunderstandings exist about what we call stereoscopy. Three points must be distinguished: (1) stereoscopic fusion—two images belonging to two eyes (double images) fuse into one; (2) perception concerning the localization in third dimension of stereoscopic images (Ogle reserves for this category the name 'stereopsis.'[11]—these two phenomena, (1) and (2), are frequently confused with one another; and (3) perception of relief. Although the source of relief is geometrically the same as perception of distance, it is well to discriminate between them for the purpose of investigation because, in the case of relief, different factors must be taken into account, *e.g.* accommodation, pupil-size, etc., which do not importantly influence perceived distance.

To avoid these different misunderstandings and misinterpretations, one must introduce a proper and precise terminology which until now is unfortunately lacking.

SUMMARY

Depth-perception and relief, resulting from fusion of crossed and uncrossed double stereoscopic images—of rods and strips of different diameters and widths which were placed in and near the median plane—are studied. The images observed appear at definite distances from *O* in relation to his fixation-point and they are seen in relief different from that of the objects used. They frequently appear on each side of the fixation-point as two short 'walls,' 'ribbons,' or 'strips.' The distance of these stereoscopic images, their lateral distances from the median plane as well as their relief, their length, their inclination to the median plane, and their general shape can be defined by formulas derived from the empirical law of intersection of visual lines. This law states that the stereoscopic image is formed at the intersection of two visual lines connecting the centers of the eyes with the corresponding objects whose images fuse stereoscopically.

Formulas are given containing values for the geometrical distances of the objects from *O*, for interocular distances of *O*'s eyes, and for distances of points on the median plane where the visual lines connecting the centers of the corresponding eyes with the edges of the objects intersect the median plane. The formulas define not only the distance and relief of stereoscopic images of objects placed in the median plane but also of those placed outside it; they also allow definition and prediction of distortions in stereoscopic vision occurring when images of different sizes brought about in natural setting or by using glasses of different kinds are fused stereoscopically.

Experiments are described which confirm the conclusions drawn from formulas based on the geometrical constructions.

The theory of depth-perception in stereoscopic vision must be based on three main laws: (1) the law of visual angle, which states that the sizes

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[11] K. N. Ogle, Essential aspects of binocular vision, *A.M.A. Arch. Ophthal.*, 52, 1934, 3 f.

and the lateral distances of images of objects perceived laterally from the median plane or from the fixation-point increase with increasing distance of the fixation-point (Emmert's law for after-images); (2) the law of intersection of visual lines, defined above; and (3) the law of 'relief,' which defines the influence of the image in one eye on the perceived direction and inclination of the image of the other eye—which also determines the length of the resulting stereoscopic images.

Also defined are the roles of different factors in stereoscopic vision of interocular distance, of convergence, and accommodation. Convergence is responsible for the perceived distance, while accommodation, together with convergence, determines the relief and general shape of the stereoscopic images. The point was stressed, however, that neither accommodation nor convergence was to be understood in the sense of muscular efforts or kinesthetic sensations, which do not explain anything about depth-perception.

PARTIAL REINFORCEMENT IN THE FISH

By JEROME WODINSKY, American Museum of Natural History, and
M. E. BITTERMAN, Bryn Mawr College

We report here the latest in a series of exploratory investigations of learning in the fish *Tilapia macrocephala* (the African mouthbreeder) under conditions analogous to those which have been used for the study of learning in higher animals. Previous papers have been concerned with questions about habit-reversal and probability-learning in discriminative situations.[1] Here we are concerned with two further questions, related both to each other and to those considered earlier: one about the effect of partial as compared with consistent reinforcement on the acquisition and extinction of a simple instrumental response, and a second about changes in the pattern of acquisition and extinction of such a response in the course of conditioning-extinction series (*i.e.* successive blocks of reinforced and unreinforced trials).

The paradoxical increase in resistance to extinction often produced by partial reinforcement continues to challenge the learning theorist. Of the several resolutions proposed, the most widely accepted (in one form or another) is that which Mowrer and Jones have named the *discrimination-hypothesis*.[2] If rate of extinction increases with the difference between conditions of training and conditions of extinction, and if that difference is greater for consistently reinforced than for partially reinforced *S*s, then extinction should be more rapid after consistent reinforcement. Phrased in such general terms, the hypothesis does not, of course, specify the basis upon which the similarity between conditions of training and extinction is to be evaluated, and there has been some disagreement on that score. The Hullians have been inclined to emphasize the relation between the stimulating conditions which prevail on reinforced training trials and those which prevail in extinction.[3] If reinforcement

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[1] Jerome Wodinsky and M. E. Bitterman, Discrimination-reversal in the fish, this JOURNAL, 70, 1957, 569-575; M. E. Bitterman, Jerome Wodinsky, and D. K. Candland, Some comparative psychology, this JOURNAL, 71, 1958, 94-110.

[2] O. H. Mowrer and Helen Jones, Habit strength as a function of the pattern of reinforcement, *J. exp. Psychol.*, 35, 1945, 293-311.

[3] V. F. Sheffield, Extinction as a function of partial reinforcement and distribution of practice, *J. exp. Psychol.*, 39, 1949, 511-526; C. L. Hull, *A Behavior System*, 1952, 120-121.

and nonreinforcement have different sensory after-effects, and if the after-effects persist through the intertrial interval, then partially reinforced *Ss* sometimes are reinforced for responding to a stimulus-complex which, because it includes the after-effects of nonreinforcement, closely resembles the stimulus-complex encountered in extinction; consistently reinforced *Ss*, by contrast, never are reinforced for responding to a complex of stimuli which includes the after-effects of nonreinforcement. The insufficiency of this interpretation is indicated, however, by the results of a variety of experiments on partial delay of reinforcement,[4] on patterns of nonreinforcement,[5] and on distribution of practice,[6] which suggest that certain larger features of the training series may be discriminated.[7]

As a supplement to the discrimination-hypothesis, the neo-Guthrians have proposed an *habituation-hypothesis*.[8] If the probability of certain characteristic responses to nonreinforcement is reduced *(sic)* as a result of their elicitation on unreinforced training trials, and if rate of extinction is directly related to the availability of such responses (which are assumed to compete with the response undergoing extinction), then rate of extinction should be reduced by partial reinforcement. The compatibility with contiguity-theory of the notion that the elicitation of a response reduces the likelihood of its recurrence is, of course, far from obvious. Quite apart from the question of internal consistency, however, the habituation-hypothesis is contradicted by the results of experiments on the patterning of nonreinforcement. A particularly damaging bit of evidence (because it simultaneously rules out habituation and discrimination of after-effects), has been reported by Capaldi: when reinforced and unreinforced trials are regularly alternated in training, resistance to extinction actually decreases as the number of training trials increases.[9]

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[4] Janet Crum, W. L. Brown, and M. E. Bitterman, The effect of partial and delayed reinforcement on resistance to extinction, this JOURNAL, 64, 1951, 228-237; E. D. Scott and E. L. Wike, The effect of partially delayed reinforcement and trial-distribution on the extinction of an instrumental response, this JOURNAL, 69, 1956, 264-268; F. A. Logan, E. M. Beier, and W. D. Kincaid, Extinction following partial and varied reinforcement, *J. exp. Psychol.,* 52, 1956, 65-70; Elizabeth Fehrer, Effects of amount of reinforcement and of pre- and postreinforcement delays on learning and extinction, *ibid.,* 52, 1956, 167-176.

[5] D. W. Tyler, E. C. Wortz, and M. E. Bitterman, The effect of random and alternating partial reinforcement on resistance to extinction in the rat, this JOURNAL, 66, 1953, 57-65; E. J. Capaldi, The effect of different amounts of training on the resistance to extinction of different patterns of partially reinforced responses, *J. comp. physiol. Psychol.,* 51, 1958, 367-371.

[6] Solomon Weinstock, Resistance to extinction of a running response following partial reinforcement under widely spaced trials, *J. comp. physiol. Psychol.,* 47, 1954, 318-322; Wilma Wilson, E. J. Weiss, and Abram Amsel, Two tests of the Sheffield hypothesis concerning resistance to extinction, partial reinforcement, and distribution of practice, *J. exp. Psychol.,* 50, 1955, 51-60; Tyler, Extinction following partial reinforcement with control of stimulus-generalization and secondary reinforcement, this JOURNAL, 69, 1956, 359-368; D. J. Lewis, Acquisition, extinction, and spontaneous recovery as a function of percentage of reinforcement and intertrial intervals, *J. exp. Psychol.,* 51, 1956, 45-53.

[7] That the Hullian account of partial reinforcement is a variant of the discrimination-hypothesis often has not been recognized. Since the principle of stimulus-generalization plays a central role in the Hullian derivation of the paradox, it usually has been called the *stimulus-generalization hypothesis,* while the term *discrimination-hypothesis* has been reserved for the view that one must go beyond the notion of sensory carry-over from trial to trial if the similarity between training and extinction conditions is properly to be assessed.

[8] Weinstock, *op. cit.,* 318-322; D. W. Lauer and W. K. Estes, Successive acquisitions and extinctions of a jumping habit in relation to schedule of reinforcement, *J. comp. physiol. Psychol.,* 48, 1955, 8-13; D. W. Lauer and T. S. Carterette, Changes in response measures over repeated acquisitions and extinctions of a running habit, *ibid.,* 50, 1957, 334-338.

Related to the phenomenon of greater resistance to extinction following partial reinforcement is the phenomenon of adjustment to conditioning-extinction series. When *Ss* are conditioned, extinguished, conditioned again, extinguished again, and so forth, resistance to extinction often decreases progressively while rate of acquisition increases progressively.[10] Noting that just such changes were to be expected from the discrimination-hypothesis, Mowrer and Jones predicted less adequate adjustment to conditioning-extinction series in partially reinforced as compared with consistently reinforced *Ss*,[11] and their prediction subsequently was confirmed by Perkins and Cacioppo.[12] When practice is distributed to rule out the possibility of discrimination based on the after-effects of reinforcement and nonreinforcement, the habituation-hypothesis requires increasing resistance over a series of extinctions separated by reconditioning sessions, and some supporting evidence has been obtained by the contiguity theorists in experiments that were not carried very far.[13] Even with massed practice, Wickens and Miles found an initial increase in resistance to extinction which soon gave way to a progressive decline.[14] Level of original training probably is an important parameter in these studies.

All of the results thus far considered were obtained in experiments with the rat, and little is known about their phyletic generality. Parallel observations have been made at the human level,[15] but there has been hardly any work at all with submammalian forms.[16] In the present study, the problem was carried to the level of the fish. Our first aim was to determine whether the mouthbreeder would show the phenomenon of increased resistance to extinction following partial reinforcement. We were prepared for the possibility that it might not, since our earlier experiments with a discriminative situation had shown it to be somewhat differently affected than the rat by intermediate percentages of reinforcement. Our second aim was to study the adjustment of the mouthbreeder to conditioning-extinction series. In our previous experiments on reversal-learning, it had given very little evidence of the progressive improvement in performance over a series of reversals which is characteristic of higher animals and which, like adjustment to conditioning-extinction series, has been attributed to the discrimination of stimuli reflecting the events of preceding trials.[17]

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[9] Capaldi, *op. cit.*, 367-371.

[10] See, for example, D. H. Bullock and W. C. Smith, An effect of repeated conditioning-extinction upon operant strength, *J. exp. Psychol.*, 46, 1953, 349-352. The effect was noted but not clearly demonstrated in a variety of early studies.

[11] Mowrer and Jones, *op. cit.*, 293-311.

[12] C. C. Perkins, Jr., and A. J. Cacioppo, The effect of intermittent reinforcement on the change in extinction rate following successive reconditionings, *J. exp. Psychol.*, 40, 1950, 794-801.

[13] Lauer and Estes, *op. cit.*, 8-13; Lauer and Carterette, *op. cit.*, 334-338.

[14] D. D. Wickens and R. C. Miles, Extinction changes during a series of reinforcement-extinction sessions, *J. comp. physiol. Psychol.*, 47, 1954, 315-317.

[15] See, for example, L. G. Humphreys, The effect of random alternation of reinforcement on the acquisition and extinction of conditioned eyelid reactions, *J. exp. Psychol.*, 25, 1939, 141-158; D. A. Grant, J. P. Hornseth, and W. W. Hake, The influence of the inter-trial interval on the Humphreys 'random reinforcement' effect during the extinction of a verbal response, *ibid.*, 40, 1950, 609-612; E. D. Longenecker, John Krauskopf, and M. E. Bitterman, Extinction following alternating and random partial reinforcement, this JOURNAL, 65, 1952, 580-587; Capaldi, The effect of different amounts of alternating partial reinforcement on resistance to extinction, this Journal, 70, 1957, 451-452.

[16] A beginning has been made with the bird. See W. O. Jenkins, Howard McFann, and F. L. Clayton, A methodological study of extinction following a periodic and continuous reinforcement, *J. comp. physiol. Psychol.*, 43, 1950, 155-167; B. F. Skinner, Are theories of learning necessary?, *Psychol. Rev.*, 57, 1950, 193-216.

METHOD

Subjects. The *S*s were 16 sexually mature African mouthbreeders, about 2 in. long, which were drawn from the stock-tanks of the Department of Animal Behavior of the American Museum of Natural History.

Apparatus. The two-target apparatus illustrated in an earlier paper was employed once more.[18] One of the openings in the black housing was covered, and only a single target (gray, 2 in. in diameter) was presented. Displacement of the target by *S* completed an electrical circuit and sounded a buzzer, which signalled the response to *E*.

Preliminary training. The preliminary training was given at 24-hr. intervals. A target baited with liver was introduced into *S*'s individual living tank, and each time *S* displaced the target while nibbling at the liver *E* dropped a pellet of an attractive dry food (Aronson's mixture) into the water at the opposite end of the tank. There were five days of training with the baited target, and then six days of training with an unbaited target, during the last three of which the target was lifted out of the water after each of the 20 daily responses. On the basis of their adjustment in pre-training, the animals were divided into two roughly equated groups of 8 *S*s each—a Consistent Group and a Partial Group.

Experimental training. Throughout the experiment proper, the interval between experimental sessions for each *S* was 48 hr., half the *S*s in each group being used on alternate days.

Part I of the experiment began with 10 training sessions, each consisting of 20 discrete trials on which latency of response was measured to the nearest second. Each trial began with the introduction of the (unbaited) target. After response, the target was withdrawn, and reward was or was not administered as the conditions required. The Consistent Group was reinforced on each trial. The Partial Group was reinforced on only 10 of the 20 trials of each session, reinforced and unreinforced trials following selected Gellermann orders with the restriction that the first and last trials of each day were reinforced.[19] Each *S* in each group received 22 pellets of food every 48 hrs.; each *S* of the Consistent Group earned 20 pellets during the experimental session and was given 2 more 1 hr. after return to the living quarters, while each *S* of the Partial Group earned 10 pellets and was given 12 more afterward. The inter-trial interval was only a few seconds.

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[17] Hull, *op. cit.,* 114-119.

[18] Wodinsky and Bitterman, *op. cit.,* 570.

[19] L. W. Gellermann, Chance orders of alternating stimuli in visual discrimination experiments, *J. genet. Psychol.,* 42, 1933, 206-208.

In the eleventh session, all *S*s were extinguished to the criterion of five successive failures to respond in 30 sec. When, on any trial, an animal failed to respond in 30 sec., the lever was withdrawn for a few seconds and then reinserted to begin the next trial. Each animal later was given 22 pellets of food in the living quarters. After the first extinction, there were three reconditioning sessions of 20 trials each, a second extinction session, three more reconditioning sessions, and so on, until the animals had been extinguished 10 times.

Half the animals in each group were extinguished again in the session following the tenth extinction, then given three reconditioning sessions before being extinguished for the twelfth time. The remaining animals in each group had the three reconditioning sessions between the tenth and eleventh extinctions rather than between the eleventh and twelfth. After the twelfth extinction, there were three reconditioning sessions for all animals, and then four consecutive extinction sessions, which concluded Part I.

In Part II of the experiment, there were 10 sessions of 30 trials each for all *S*s. The Consistent Group was reinforced on each of the first 10 trials, while the Partial Group was reinforced only on Trials 1, 2, 4, 8, and 10. The last 20 trials of each session were unreinforced. If, on any trial, the latency of response exceeded 30 sec., the target was withdrawn. After five successive trials without response in 30 sec., the session was terminated, and *S* was assigned a score of 30 sec. for each omitted trial.

RESULTS

Mean log latency declined in negatively accelerated fashion in the initial training sessions of Part I. The mean latency of the Partial Group reached a quite stable asymptote of approximately 0.25 log sec. on the third day of training, while the latency of the Consistent Group reached a stable asymptotic value of approximately 0.1 log sec. on the sixth day of training. The two asymptotes differ significantly at the 1% level by Wilcoxon's nonparametric test for unpaired replicates. The first portion of Fig. 1 gives a trial-by-trial record of performance in the two groups on the tenth day of training. It shows the characteristic difference between the latencies of the two groups, as well as the tendency toward within-days improvement which persisted to some extent even into the tenth training session.

The second portion of Fig. 1 shows changes in latency for the two groups over the Trials 1-70 of the first extinction. The curves converge during the first 15 trials and from then on are virtually identical. Extinction is by no means complete, however, when the graphic presentation stops; only three of the 16 *S*s (one 'Consistent' and two 'Partial') reached the criterion of extinction in the first 70 trials. After the seventieth trial, the performance of the two groups diverged once more, the latency of the

Partial Group increasing more rapidly than that of the Consistent Group. On the average, the 'Consistent' *S*s extinguished in about 153 trials, while the 'Partial' *S*s required only about 95 trials. The difference is significant at about the 6% level of confidence by Wilcoxon's test for unpaired replicates.

Mean trials to criterion for the two groups in the first 10 extinctions may be read from Fig. 2. Both curves decline, the curve of the Consistent Group more sharply than that of the Partial Group. Nonparametric analysis of variance by Friedman's method shows a significant order-effect in each case (beyond the 1% level for the Consistent Group and at the 5% level for the Partial Group). In the early extinctions, resistance is greater in the Consistent Group, and only in the fourth extinction does the paradoxical effect commonly found in higher animals make its appearance. The resistance of the Partial Group is significantly greater at about the 5% level of confidence in Extinctions 4-9.

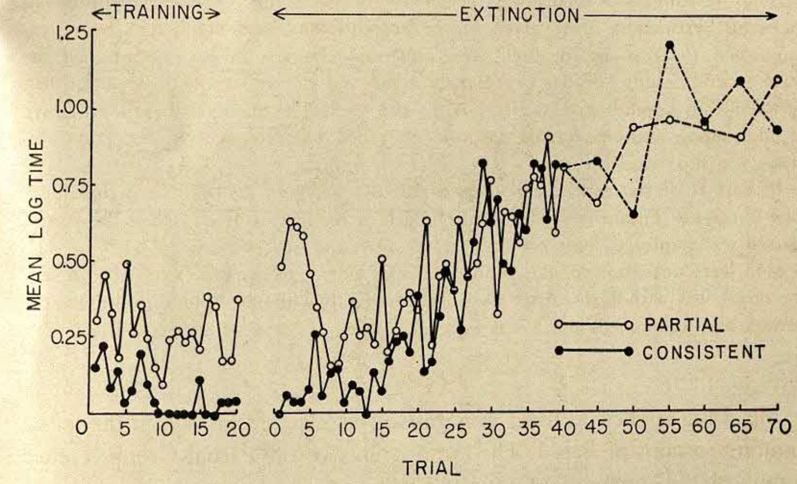

FIG. 1. PERFORMANCE ON THE TENTH DAY OF TRAINING AND ON TRIALS 1-70 OF THE FIRST EXTINCTION

(After the fortieth trial of extinction, the latency only of every fifth trial is plotted. Extinction continued well beyond the seventieth trial.)

Beyond the fifth extinction, neither group shows a significant decline in resistance, and the curves for that extinction, plotted in Fig. 3, are quite representative of asymptotic performance. The difference between the two curves lies mainly in the region of Trials 5-15, where the progress toward extinction of the Partial Group is reversed for a time. All extinction curves

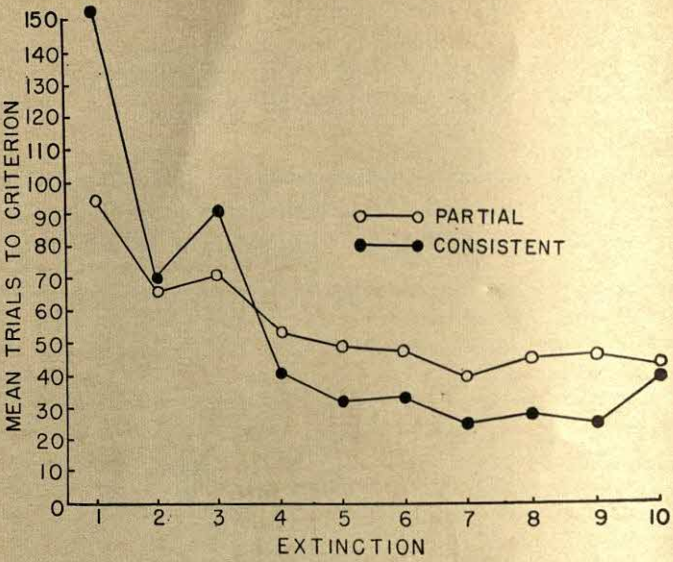

FIG. 2. MEAN TRIALS TO CRITERION IN THE FIRST TEN EXTINCTIONS

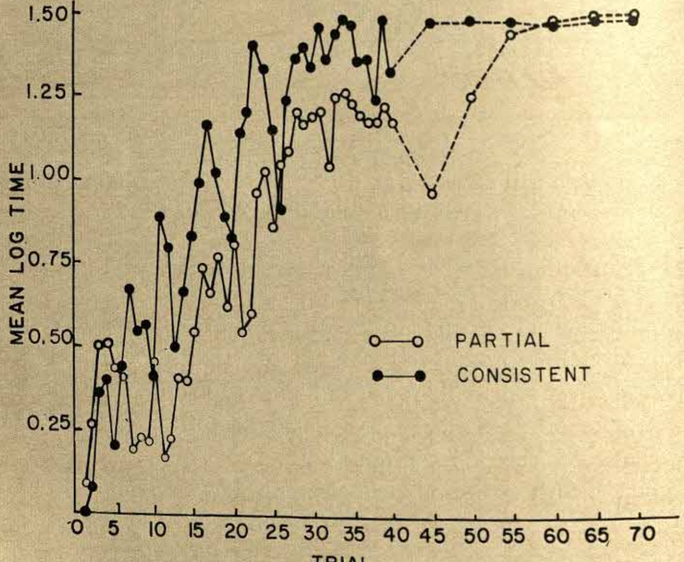

FIG. 3. PERFORMANCE DURING THE FIFTH EXTINCTION

for the Partial Group show the same pattern, while the curves for the Consistent Group show uninterrupted progress toward extinction.

Changes in rate over the first 10 extinctions are illustrated in Fig. 4 (Trials 1-70 of the first, second, and fifth extinctions for the Consistent Group) and Fig. 5 (corresponding curves for the Partial Group). With the exception of the change from the first to the second extinction in the Consistent Group, there was no significant improvement before Trial 15.

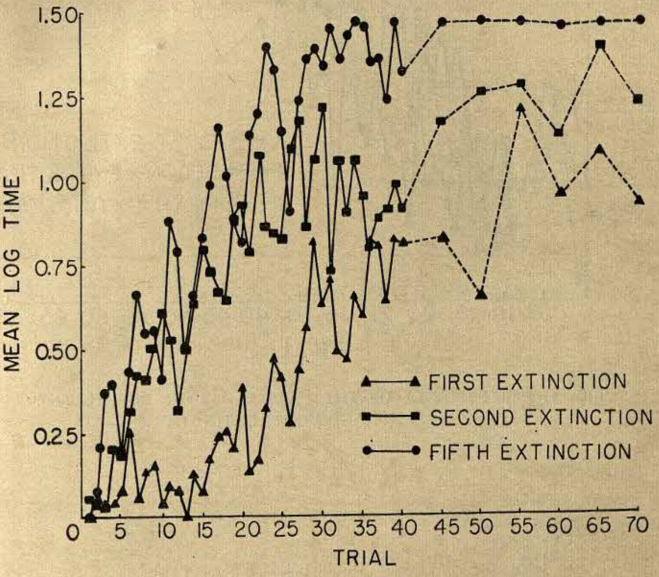

FIG. 4. PERFORMANCE OF THE CONSISTENT GROUP DURING THE FIRST, SECOND, AND FIFTH EXTINCTIONS

The Partial Group made no significant improvement at all in the first 15 trials. A comparison of Fig. 4 with Fig. 5 again reveals greater change in the 'Consistent' than in the 'Partial' *S*s, although the performance of the 'Consistent' *S*s at asymptote is by no means impressive. Fig. 5 shows the pattern of extinction characteristic of the 'Partial' *S*s—an initial increase in latency followed by a decline and then by a further increase.

Progressive improvement in performance also appeared in reconditioning. Mean log latency in the first training session following each of the first nine extinctions is given by Fig. 6. There was no significant improvement in the Consistent Group beyond the third reconditioning (Wilcoxon's test for paired replicates) and none in the Partial Group beyond the second

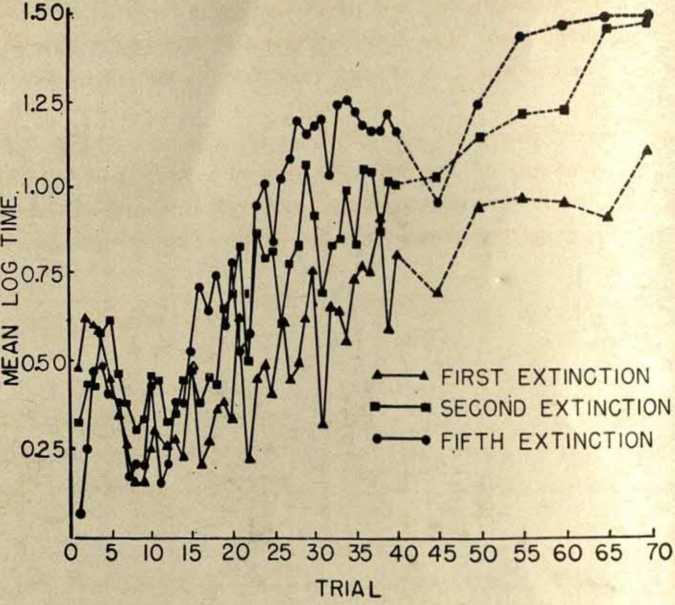

FIG. 5. PERFORMANCE OF THE PARTIAL GROUP DURING THE FIRST, SECOND, AND FIFTH EXTINCTIONS

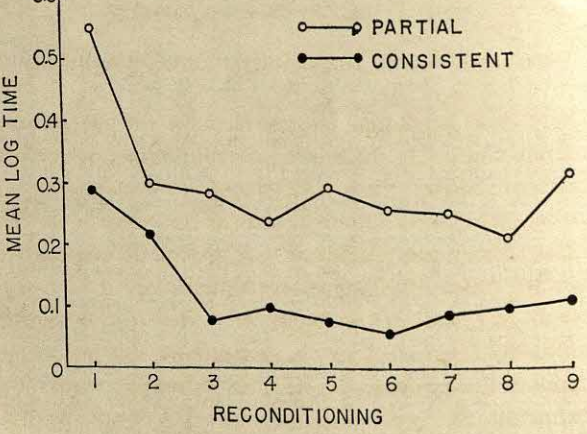

FIG. 6. PERFORMANCE DURING THE DAY OF RETRAINING AFTER EACH OF THE FIRST NINE EXTINCTIONS

reconditioning, although the two sets of data may be fitted by essentially parallel decay-functions. The difference in mean latency between the two groups is significant at a level of confidence well beyond 1% by Wilcoxon's method.

Changes in the pattern of reconditioning are illustrated by Fig. 7. Recovery from the effects of the first extinction was much less rapid in the Partial than in the Consistent Group, although in the third day of the first reconditioning the mean latencies of the two groups approached equality. There was substantial improvement in the performance of the 'Partial' *S*s from the first to the second reconditioning, but extinction continued thereafter to disrupt their performance in constant degree, and there were no further changes in rate or pattern of recovery. In the 'Consistent' *S*s, by contrast, spontaneous recovery was essentially complete following the third and all subsequent extinctions in the series.

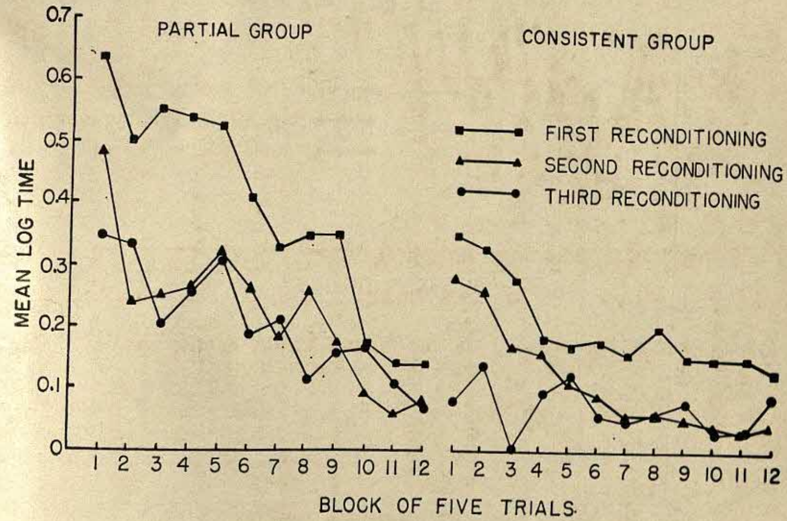

FIG. 7. PERFORMANCE DURING THREE DAYS (SIXTY TRIALS) OF RETRAINING

The eleventh and twelfth extinctions were used to provide further evidence on differential resistance in the two groups. For each *S*, one of the two extinctions followed three days of reconditioning, while the other did not. The extinction after reconditioning showed a significant difference of the same magnitude as before—a mean of 47 trials to criterion for the Partial Group and 34 trials for the Consistent Group. In the extinction which was not preceded by reconditioning, the resistance of both groups fell to the same low level—a mean of about 22 trials to criterion. Somewhat inconsistent results were obtained in Extinctions 13-16, which were not separated by reconditioning sessions. The thirteenth extinction did not differentiate the two groups, but the resistance of the Partial Group was significantly greater in the fourteenth and fifteenth. By the sixteenth, the resistance of both groups had fallen to the same low level (about 12 trials to criterion).

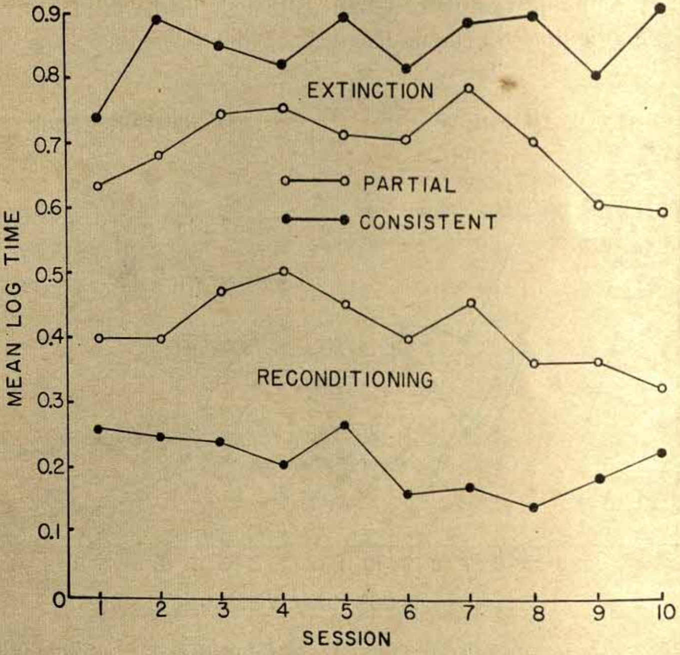

FIG. 8. PERFORMANCE OVER THE TEN CONDITIONING-EXTINCTION SESSIONS OF PART II

In Part II of the experiment, which consisted of 10 sessions, there were 10 reconditioning trials followed by 20 extinction-trials in each session. As Fig. 8 indicates, there was no significant change from session to session in the performance of either group during the reconditioning or during the extinction-phases (Friedman's analysis), and the over-all within-sessions performance of the two groups for the 10 sessions combined therefore could be plotted (Fig. 9). The curves of Fig. 9 show some familiar relationships. Spontaneous recovery was essentially complete in the Consistent

Group, but in the Partial Group extinction produced more lasting disruption in performance from which there was only gradual recovery in the reconditioning phase. On trials which followed reinforced trials—the second, third, fifth, and ninth training trials, and the first reconditioning trial—the mean latency of the Partial Group was significantly higher than on the first five trials following nonreinforcement (Wilcoxon's test for paired replicates, 1% level of confidence). The same pattern was present, and to the same extent, in the very first session. Resistance to extinction was significantly greater in the Partial than in the Consistent Group (Wilcoxon's unpaired replicates, 1% level). The extinction-curve for the Partial Group shows the familiar fall before the terminal rise, but the initial rise which appeared in the curves of Part I is not in evidence.

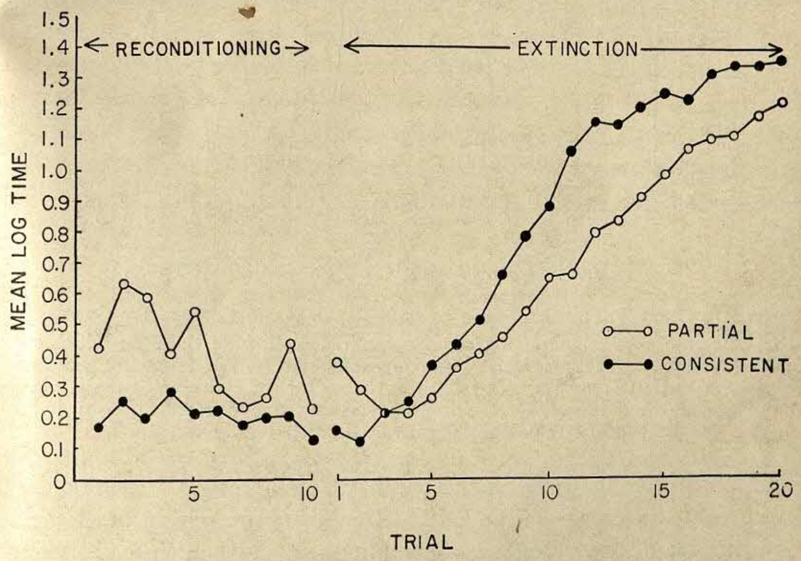

FIG. 9. WITHIN-SESSIONS PERFORMANCE FOR THE TEN CONDITIONING-EXTINCTION SESSIONS COMBINED

The origin of the tendency of the Partial Group in Part II to slow down after reinforcement and to speed up after nonreinforcement was sought in the data of Part I. For each group, for each day of original conditioning and for each day of each subsequent reconditioning, the mean log time on trials following those on which the Partial Group was reinforced was subtracted from the mean log time on trials following those on which the Partial Group was unreinforced. These difference-scores are plotted in

Fig. 10. The curve for the Partial Group shows a gradual, negatively accelerated development of the tendency to run faster after nonreinforcement than after reinforcement. By Wilcoxon's test for paired replicates, the difference-score falls significantly below zero (at the 5% level of confidence) for first time on the tenth day of the original conditioning series. That the change in the Partial Group is not merely an order-effect independent of the patterning of reinforcement and nonreinforcement is shown by the control curve for the Consistent Group, which continues throughout the experiment to hover about zero.

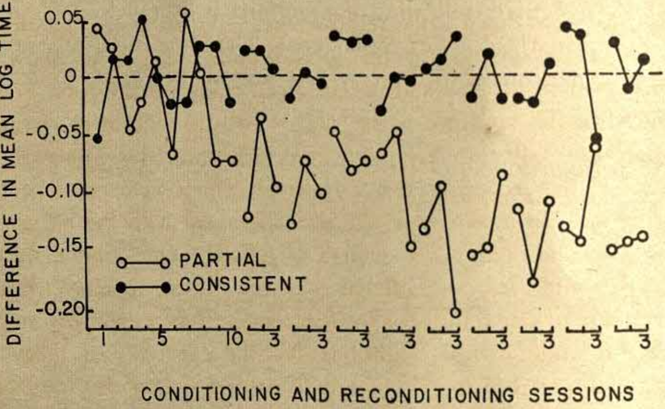

FIG. 10. DIFFERENTIAL RESPONSE OF THE PARTIAL AND CONSISTENT GROUPS
The difference between mean log seconds for trials following those on which the Partial Group was reinforced and mean log seconds for trials following those on which the Partial Group was not reinforced is plotted for each day of the original training and the subsequent reconditionings. A negative difference means faster response after nonreinforcement than after reinforcement.

DISCUSSION

These results for the mouthbreeder are in some respects similar and in some respects quite different from those obtained in analogous experiments with the rat. The major points of similarity lie in the greater resistance to extinction eventually developed by the partially reinforced animals and in the adjustment of both groups, better in the Consistent than in the Partial, to repeated conditioning and extinction. Several points of difference are worth noting. First, resistance to extinction tended to be greater at the outset in the Consistent Group, with the paradoxical effect characteristic of the rat appearing only gradually in the course of successive extinctions. Secondly, the shape of the extinction-curves generated by the Partial Group in Part I is unique. Partially reinforced rats often show an initial decline in latency of response during extinction, but the latency of our fish increased to begin with, then declined, and then increased once more. Thirdly, the development in our partially reinforced *S*s of a stable tendency to slow down after reinforcement and to speed up after nonreinforcement is revealed by the reconditioning data. Rats show a like tendency when reinforced and unreinforced trials are alternated regularly, but not when the sequence is irregular.[20] Fourth, long after resistance to extinction had fallen to asymptote, our fish showed significantly lower resistance in the second of two extinctions which were not separated by reconditioning. In a somewhat similar experiment with the rat, the omission of a reconditioning session had no effect.[21] One cannot, of course, give much weight to these differences at the present time. Not only does the generality of our results for the fish remain to be established in further experiments under varied conditions, but the generality of many of the results for the rat which serve as a basis of comparison also must be demonstrated. What is required is a series of parallel experiments with the two forms in which such parameters as drive, distribution of practice, pattern of reinforcement, and the like, are varied systematically over a broad range.

Irrespective of their representativeness, we may ask how well our results for the fish are handled by the concepts which have been developed to deal with the behavior of the rat in analogous experiments. The habituation-hypothesis does not fit the progressive decrease in resistance to extinction shown by both groups; if anything like a tendency toward increased resistance did develop in the course of training, it was masked by other factors. The concept of discriminated after-effects of reinforcement and nonreinforcement is contradicted at several points. The higher initial resistance to extinction in the Partial Group does not fit, nor does the pronounced tendency of the Partial Group to react more slowly after reinforcement than after nonreinforcement. (Since the Gellermann sequences which dictated the pattern of partial reinforcement were selected with the restriction that the first and last trials in each daily series be reinforced, the probability of reinforcement after nonreinforcement actually was slightly higher than the probability of reinforcement after reinforcement—25/50 or 50% as compared with 20/45 or 44.4%. It is rather unlikely, however, that our fish were discriminating so small a difference, since the rats of a previous study failed under corresponding conditions to discriminate a somewhat larger difference—50% as compared with 40%[22] The hypothesis may, of course, be entertained that the after-effects of reinforcement and nonreinforcement are more discriminable in the fish, but then the relatively slow development in the fish of superior resistance to extinction following partial reinforcement is difficult to explain.) The shape of the extinction-curves yielded by the Partial Group in Part I—specifically, the increase in latency produced by early nonreinforcements—is not compatible with the notion that the greater resistance to extinction eventually developed by the partially reinforced *S*s may be attributed to reinforcement for response to the after-effects of nonreinforcement, nor is it easily reconciled with the more rapid response after nonreinforcement which appeared in reconditioning. The over-all results of the experiment certainly do not rule out a broadly formulated discriminative hypothesis, but the Hullian version seems no more adequate at the level of the fish than it does at the level of the rat.

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[20] Tyler, Wortz, and Bitterman, *op. cit.*, 57-65; Capaldi, *op. cit.*, 367-371.

[21] Wickens and Miles, *op. cit.*, 293-311.

It remains for us here but to comment briefly on the relation between the present results and those of our previous experiments on habit-reversal and probability-learning in the mouthbreeder. Whether the processes underlying progressive improvement in performance over a set of habit-reversals are the same as those underlying adjustment to conditioning-extinction series, or whether they are different, we do not know, but a search for both effects at a variety of animal levels—a process of phylogenetic screening—might provide a useful clue. If one effect appeared without the other in some animal, a disparity of underlying function might be assumed, which is not, of course, to say that an identity of function would be assumed if the two effects were inseparable. Only in one of a series of previous experiments on habit-reversal did our fish give any sign of progressive improvement in performance, and then only to a limited degree and in a form quite different from that which seems to be characteristic of the rat. In the present experiment, the fish showed an adjustment to conditioning-extinction series like that found in the rat, but a much more detailed study of the phenomenon is necessary at both levels. Amount of interpolated reconditioning should be varied systematically, as should extent of extinction. Adequacy of adjustment in each form then might be estimated in terms of the degree to which resistance to extinction becomes independent of the amount of interpolated reconditioning, and recovery from extinction independent of the extent of interpolated extinction.

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[22] Tyler, Wortz, and Bitterman, *op. cit.*, 57-67.

The present results on partial reinforcement modify a conclusion drawn from our previous work on probability-learning. The previous study—which showed 'matching' by the fish and 'maximizing' by the rat in 70:30 problems with guidance (forced correction) after error, but 'maximizing' in both forms when guidance was eliminated—suggested that fish and rat were differentially affected by reinforcement of the minority-stimulus but not by nonreinforcement of the majority-stimulus. In the present experiment, with no minority-stimulus and with another measure, differences appeared in the behavior of the two forms which suggest that they may be differentially responsive to occasional nonreinforcement. The tendency of the Partial Group to respond more promptly on trials following nonreinforcement than on trials following reinforcement led us to look for sequential patterning of response in the probability-data for the fish. The results were negative: both in the visual and in the spatial problem, the majority-stimulus was positive on just about 70% of the trials preceding minority choices (as would be expected by chance).

SUMMARY

Two groups of African mouthbreeders were trained in discrete trials to strike at a target for food-reward. Latency of response was measured. After prolonged training, with one group consistently and the other partially reinforced, both groups were subjected to a series of extinction and reconditioning sessions. The results were compared with those obtained in analogous experiments on the rat.

THE IDENTIFICATION OF RECURRENT REGULARITY

By JEROME S. BRUNER and MICHAEL A. WALLACH, Harvard University,
and EUGENE H. GALANTER, University of Pennsylvania

Much of what we classify as learning, recognition, and problem-solving consists of being able to identify recurrent regularities in the environment.[1] What makes such a task a problem is that recurrent regularities—be they turns in a maze, elements in a temporal pattern, or a pattern in successive events—may either be masked by factors that are irrelevant to the regularity, or the regularity itself may be of such a complexity that it exceeds the memory span that an observer brings to the task.

One may think of the task involved in identifying recurrent regularity as requiring the observer to construct a model that is isomorphic with the redundancy of the environment. The case is easily illustrated by reference to the identification of language-sequences. If the individual knows a language, *i.e.* has a model of the recurrent regularity of letters or words in the language, his task of identification is rendered easy in proportion to the degree to which a linguistic display approximates the typical sequences of the language.[2] So, too, for the rat learning a single alternation-pattern; he must construct an adequate, predictive model of the environmental regularity to be identified over time.

Model-construction involves the elimination of irrelevancies where such exist or the evolving of methods of recoding stimulus-events in such a way as to bring them within the compass of attentional or immediate memory-span. An illustration of the first kind of operation is the strategy employed in attaining concepts; the second is exemplified by the recoding of stimulus-inputs to conform to the limits imposed on memory.[3]

The present study is concerned with the sources of interference that

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* Received for publication February 11, 1958. This research was supported in part by the Cognition Project of Harvard University and in part by the Air Force under Contract No. AF41(657)-118.

[1] J. S. Bruner, Going beyond the information given, in J. S. Bruner, Egon Brunswik, *et al.* (eds.), *Cognitive Approaches to Psychology,* 1957, 1-34. E. H. Galanter and M. Gerstenhaber, On thought: The extrinsic theory. *Psychol. Rev.,* 63, 1956, 218-227.

[2] G. A. Miller, J. S. Bruner, and Leo Postman, Familiarity of letter sequences and tachistoscopic identification, *J. gen. Psychol.,* 50, 1954, 129-139.

[3] Miller, The magical number seven, plus or minus two: Some limits on our capacity for processing information, *Psychol. Rev.,* 63, 1956, 81-97.

prevent rapid and efficient identification of recurrent regularities in the environment. We shall confine ourselves exclusively to regularities of a simple kind that are well within the capacity of people to identify when no interference is present. To illustrate the approach, let us imagine a sequence of events, each of which may take one of two forms—for example, a sequence of binary digits. The sequence is made up of a series of recurrent, fixed, sub-series, such as 001001001 . . . or 100110011001 . . . , and the subject's (*S*'s) task is to predict each event before it occurs. His task is to identify the regularity by constructing such models as '001 repeated' or '1001 repeated' and thereby to predict subsequent events in the series perfectly. At the outset he knows neither that there is a recurrent sequence nor that it is of any particular length. He is told only whether each of his single predictions is correct or not.

What are the factors that prevent adequate construction of a model in such a simple task? In general terms, we can specify three.

(a) Stimulus-interference. If the sequence is interfered with by single events that do not conform to the recurrent series, if an element of randomness is added to the series, it is likely that the task of model construction will be difficult in proportion to the amount of randomness introduced. This is a seemingly banal point, but it will appear otherwise when examined later in connection with other matters. Moreover, the variation of irrelevant aspects of the stimuli that do *not* interfere with the series will have the effect of slowing model construction. Such variation may require *S* to take time and trials to get information regarding such irrelevancies sufficient to eliminate them from the prediction of the series—as in the case, for example, where the binary digits are presented in different colors.

(b) Interference by responses and hypotheses. Heidbreder proposed some years ago a distinction between 'spectator'-behavior and 'participant'-behavior in problem-solving, the former being simple watching or observing without hypotheses or responses, the latter involving the formulation and test-by-response of hypotheses about the nature of the events before one.[4] Participant-behavior in this sense serves to generate response-produced stimuli that may interfere with or mask the recurrent regularity in a problem to be solved. In a gambling experiment with a pattern of pay-off of RLRL . . . , we had one *S* who responded for a long series of trials with an alternating response out of phase with the rewarded choice, LRLR, and who finally announced earnestly that he guessed the machine was broken! To use a phrase designed to describe the disrupting effects of premature hypotheses in tachistoscopic recognition, there is a "liability in an hypothesis."[5]

(c) Organismic interference. There is obvious interference from inappropriate sets, such as assuming that a series of recurrent regularity is a random series and

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[4] Edna Heidbreder, An experimental study of thinking, *Arch. Psychol.,* 11, 1924, No. 73, 1-175.

[5] D. F. Wyatt and D. T. Campbell, On the liability of stereotype or hypothesis, *J. abnorm. soc. Psychol.,* 46, 1951, 496-500.

acting accordingly, but it is not these that are intended here. Rather, we take these as instances of response-interference. Our concern here is with non-specific states of the organism, non-specific with respect to the recurrent pattern with which an *S* must deal. A trivial example is inattention or fatigue, or a toxic state, but beyond such examples of 'white noisy' organismic variables, one may also expect to find some 'colored noises' operating.

Chief among these is the set of factors usually labelled motivation. Riopelle found, for example, that a reward with food introduced at the beginning of each of a series of discriminative problems had the effect of distracting monkeys from the mastering of later discriminative tasks.[6] It was as if growing preoccupation with the reward interfered with looking for appropriate cues.[7] We should prefer to narrow the issue somewhat by considering the effect of the cost and value of making false and true predictions of events in the sequence. If the cost of error is high relative to the value of correct prediction, it is conceivable that the anxiety generated by the task may be high enough either to goad the person into good performance or to disrupt him. Similarly, a high value for correct prediction relative to the cost of erroneous prediction may serve to concentrate a problem-solver on his own response and to distract him from proper attention to stimulus-events. Indeed, the point can and has been made that a 'problem' exists only when the cost of error reaches a certain level.[8]

Obviously, the three kinds of interference just described interact. Errors that are costly may lead the individual to test and eliminate more rapidly hypotheses about irrelevant features of a sequence; but these are empirical matters to which we must turn now.

To sum up, to construct a model adequate to predict a recurrent environmental regularity of a length well within immediate memory span, an *S* must overcome sources of disruption and interference of three kinds: stimulus-interference, response-interference, and organismic interference.

EXPERIMENT

The *S*s employed in the experiment, 92 in number, were summer students at Harvard. They were given the basic task of predicting whether a light would appear in the left or the right window of an apparatus. They did this by pressing a key beneath one or the other window. Whichever button was pressed, the light would then appear in one or the other window for 4 sec. and then be extinguished by the automatic programmer that controlled the sequence of lights.

Instructions. *S* was told that his task was to predict as many times as he could on which side the light would appear, and that he would be permitted to bet each time, indicating his wager by the button he pressed. He was told that he would have 40 tries at predicting and that the number of points he earned would determine how much more than his base pay of one dollar he would earn.

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[6] A. J. Riopelle, Rewards, preferences and learning sets, *Psychol. Rep.*, 1, 1955, 167-173.

[7] So too in a study of J. S. Bruner, Jean Matter, and M. L. Papanek (Breadth of learning as a function of drive level and mechanization, *Psychol. Rev.*, 62, 1955, 1-10), where drive-state seemed to prevent the rats from picking up 'superfluous' cue-regularities in the learning-situation which might be used later.

[8] D. M. Johnson, *The Psychology of Thought and Judgment*, 1955. 1-515.

The following variations in procedure were used with our eight groups.

(1) Degree of stimulus-interference. Half of the groups received the sequential recurring pattern LLR for 40 trials (one trial more than 13 cycles) with no interference. The remaining groups received the same pattern but with some uncertainty added. That is to say, 2 of the 40 appearances of the light, the 14th and the 25th, were out of the pattern. Thus the 5th cycle of the LLR pattern became LRR, and the 9th cycle RLR. In the remaining 11 cycles, the basic pattern was left intact.

(2) Degree of response-interference. Half of the groups began immediately by responding—pressing the button. The other half was permitted to watch the sequence of lights without responding for the first three presentations. For these *S*s, the light-pattern was exhibited by the *E*'s pressing the right hand button three times in a row. They were also instructed by *E* to pay attention to the lights, rather than to their button-pressing activities. The other *S*s were given no such instructions.

TABLE I

COMPOSITION AND SIZE OF THE EIGHT GROUPS IN THE EXPERIMENT

N = stimulus-noise; S = no stimulus-noise; R = responding from first;
W = waiting at first; H = high error-cost; L = low error-cost.

Group	No. *S*s	Group	No. *S*s
SWH	12	SRH	11
SWL	12	SRL	11
NWH	12	NRH	11
NWL	12	NRL	11

(3) Cost of error. Above each button were placed two numbers, the upper one of which indicated how many points *S* would win if he predicted that side correctly, the lower indicating the cost in points if he predicted wrongly. Half of the groups worked under high error-cost: an error on either side costing five points, a correct prediction earning one point. The groups operating under low error-cost earned five points for a correct prediction, lost one point for an error.

RESULTS

The eight groups are summarized in Table I. The first and most general result is that the larger the number of sources of disruption operative in a group, the greater the difficulty in identifying the recurrent regularity of the series. The simplest way to represent this is to consider the number of erroneous predictions in the 40 presentations of the lights in the one group operating with all three possible sources of disruption (stimulus-interference, response-interference, and high cost for error), the groups with two sources of disruption, with one source, and with none. The median errors for these groups are, respectively, 15.0 errors in 37 presentations, 9.5 errors, 6.0, and 1.0.[9] Fig. 1 presents the results in graphic form.

A better picture is provided by considering the eight groups separately and comparing levels of performance among them. Necessary data on errors and trials for such comparisons are contained in Tables II and III.[10]

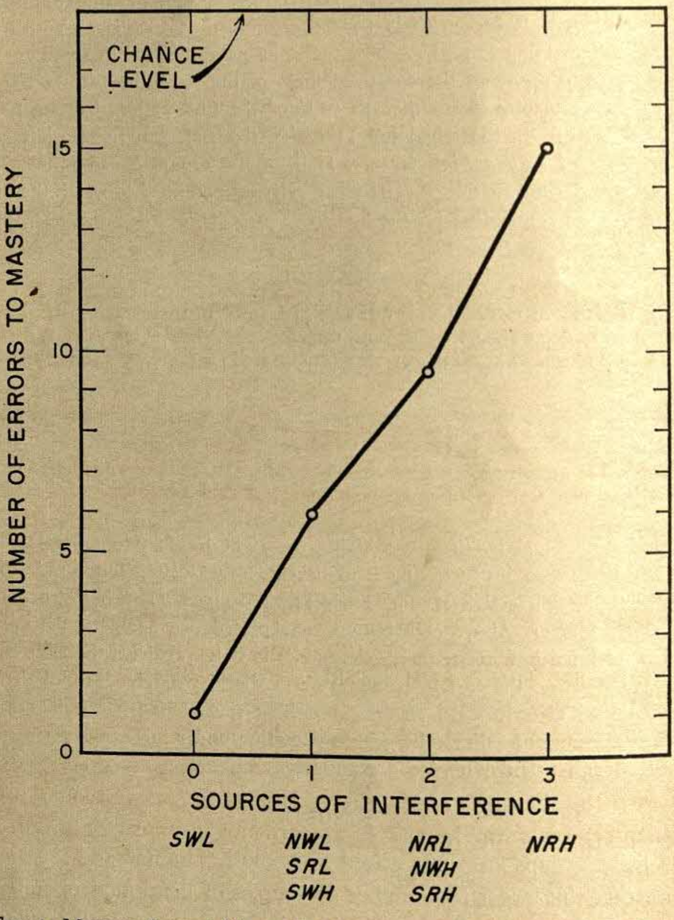

FIG. 1. MEDIAN ERRORS MADE BY GROUPS WITH DIFFERENT AMOUNTS OF POTENTIAL INTERFERENCE

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[9] Error-computations are based on the last 37 of the 40 presentations, for the groups that did not respond on the first three presentations had no opportunity for error on these presentations.

[10] We see in Table III that the learning-speed for groups SRH and SRL in the present experiment is considerably more rapid than that in a related experiment reported by J. J. Goodnow and T. F. Pettigrew (Some sources of difficulty in solving simple problems, *J. exp. Psychol.*, 51, 1956, 385-392). We believe this difference is

(1) Effect of stimulus-interference. Comparison of the first row of Table II with the second row indicates that in all four comparisons, the group without stimulus-interference performed with fewer errors. In three of the four comparisons, the difference obtained is highly reliable, the exception being the comparison of the two groups under conditions of response-interference (no wait) and low error-cost.

TABLE II

MEDIAN ERRORS IN EACH OF THE EIGHT GROUPS MADE DURING 37 FINAL PRESENTATIONS OF LIGHTS

	High cost		Low cost	
	no wait	wait	no wait	wait
Stimulus-noise	15.0	10.5	12.0	9.0
No noise	3.0	1.5	8.0	1.0

NRH vs. NWH, NRL vs. NWL, and SRH vs. SRL are significant at the 5% level; NWH vs. SWH, NRH vs. SRH, NWL vs. SWL, and SRL vs. SWL are significant at the 1% level. Computations made by the Mosteller-Bush version of the Mann-Whitney test, see Gardner Lindsey's (ed.) *Handbook of Social Psychology*, 1, 1954, 315 ff.

TABLE III

MEDIAN TRIALS UNTIL THE LAST ERROR WAS MADE, IN EACH OF THE EIGHT GROUPS
If an *S* in a 'wait' group makes no errors, his trial of last error is taken as 3.

	High cost		Low cost	
	no wait	wait	no wait	wait
Stimulus-noise	37.0	37.0	35.0	33.0
No noise	11.0	4.5	25.0	4.5

SRH vs. SRL is significant at the 5% level; NWH vs. SWH, NRH vs. SRH, NWL vs. SWL, NRL vs. SRL, SRH vs. SWH, and SRL vs. SWL are significant at the 1% level.

The first and not very surprising conclusion, then, is that even a small amount of noise thrown into a regularly recurrent pattern seriously interferes with the identification of that regularity or, as we would prefer to put it, interferes with the task of constructing a model of this recurrent regularity.

In what does the interference consist? Fig. 2 shows graphically the learning-curves over the 40 trials of all eight groups. Side by side are curves for groups differing only in whether the stimulus-series contained two off-pattern stimuli. The introduction of the off-pattern lights on Trials 14 and 25 sharply decreases the percentage of

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due to the fact that in the present experiment *S* always receives direct information about the correct side since the light appears on the correct side after each response, whereas in Goodnow and Pettigrew's experiment *S* receives indirect information when he errs, since errors are indicated by a chip's failing to drop down a chute in the panel's center.

Ss predicting correctly, following which there is more or less rapid recovery. Up to the point of the first interference, the parallel groups show very similar functions.

Interestingly enough, the interference by the first off-pattern stimulus-sequence disrupts the group with an initial period of observation more than the group without it. The explanation is not far to seek; an off-pattern stimulus-sequence is more disruptive to an *S* who has already perfected a model of what the stimulus should be. Table IV shows this difference clearly. The observing groups are doing better than the responding groups by Trial 14 and consequently make more errors when the

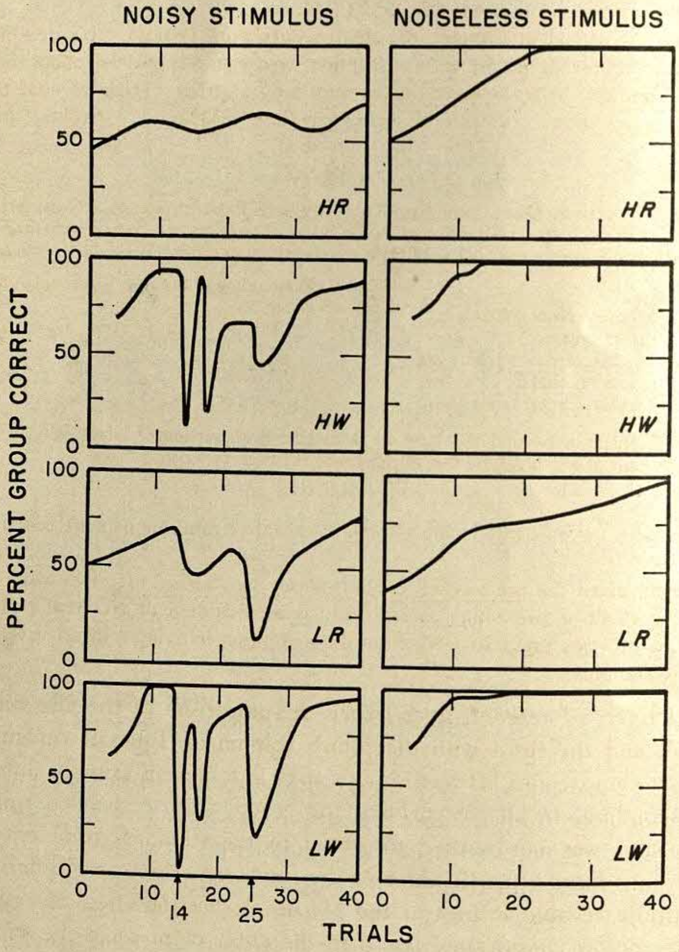

FIG. 2. LEARNING-CURVES FOR EACH OF THE EIGHT GROUPS
NEIGHBORING PAIRS ARE EQUATED ON ALL CONDITIONS SAVE THE NATURE OF THE STIMULUS-SERIES TO WHICH EXPOSED.

required response is changed, 96% as opposed to 59%, a statistically reliable difference.

Now for the recovery from the first interference. There is no reliable difference between the groups, the responding and the observing *S*s, between the first and second disruptions, errors being almost identical (Table IV).

Comes the second interfering, off-pattern sequence on Trial 25. This time there is nothing to choose in the degree of disruption: errors on this trial are about the same for both groups. Now, however, in the remaining 15 trials (Trials 26-40), the observing group shows a statistically reliable increase in mastery, or recovery, in comparison with the noise-producing responding group. Again, the data are contained in Table IV.

TABLE IV

COURSE OF LEARNING, DISRUPTION, AND RECOVERY IN *S*s PRESENTED WITH A NOISY STIMULUS-SERIES WHO INITIALLY RESPONDED TO OR SIMPLY OBSERVED THE STIMULUS-SERIES

Responding group = NRL + NRH = 22; observing group = NWL + NWH = 24

	Responding group	Observing group
Median errors: trials 4–13*	5.0	2.0
Errors on trial 14†	59%	96%
Median errors: trials 15–24	4.0	3.0
Errors on trial 25	68%	75%
Median errors: trials 26–40*	5.5	3.0

* Significant at or beyond 1% level by Mosteller-Bush version of Mann-Whitney test.
† Significant at 1% level, by test of difference between two percentages.

In sum, given the presence of stimulus-noise, the addition of response-noise may have the effect of preventing as rapid initial identification of recurrent regularity, and then prevents rapid recovery from the disruption introduced by an irregular or surprise stimulus.

(2) Effect of response-interference. A comparison of the first with the second, and the third with the fourth column of Table II contains the relevant comparisons between groups alike in treatment save for difference in responding. In all comparisons, the group which responded from the outset and was not warned to attend to the stimulus made erroneous predictions more often. In three of the four comparisons, the difference is statistically reliable at least at the 5% level; but the effect of response-interference is considerably less than the effect of noise in the stimulus-series. The mean difference in errors of groups with and without noise in the stimulus-series is 8.2, the difference when groups paused to observe three trials was 4.0.

Fig. 2 suggests that the effects of not observing were persistent throughout the learning. The observing group shows a more sharply rising learning-curve; the responding group reached perfect performance more gradually.

(3) Effect of error-cost. A comparison of the right and left halves of Table II reveals immediately that error-cost does not operate as a simple source of interference, if indeed it may be regarded as a source of interference at all. Neither these figures, nor the forms of the learning-curves in Fig. 2 suggest that there is anything interfering about high error-cost. The only significant difference between matched groups runs counter to the prediction that high error-cost would interfere with identification of a recurrent regularity. Indeed, we are inclined to favor the view that in this condition—where the stimulus-series was without interference and *S* had to overcome the interference of his own responses—high error-cost provided incentive to more careful scanning of the stimulus-situation.

DISCUSSION

We have presented here what is, in effect, a demonstrational experiment. The point of view may be briefly recapitulated: Much of learning and problem-solving can be viewed as a task in identifying recurrent regularities in the environment and this requires either the construction of a model of this regularity or the employment of a model that has previously been constructed by the person. Identification of recurrent regularity is the recognition of pattern complicated by one of two possible factors. Either the recurrent regularity is of a complexity that exceeds the limited cognitive span of the individual, or there are sources of interference either in the stimulus-input, in the required pattern of responding, or in the organism that in effect mask the recurrent regularity to be recognized. Learning, given these interferers, consists, in part, in separating the recurrent regularity to be identified from these interferers. The greater the number of interferers—the more noise there is in the stimulus, the more masking are the responses, the more difficult will identification of regularity be. When these sources are minimized, we have shown, learning is a matter of immediate recognition, provided the pattern can be handled in immediate memory-span. No stimulus-noise and minimal response-interference lead to recognition of a pattern almost immediately.

It seems unlikely to us that the explanation of how a person cuts through the interfering properties of the environment when such exist and when identification is not immediate is to be found in theories of reinforcement. Rather, the answer probably lies in how the organism learns to use techniques for weighing the relevance of different features of input for regularity-to-be-discovered. The simple instruction "Pay attention to the stimulus and disregard your past responses," for example, is highly effective advice to oneself in identifying environmental regularities that are invariant with respect to the overt responses of the problem-solver. It is conceivable that *after* such techniques have been mastered a simple law of frequency might work, but not before. Essentially, as in the case of concept-attainment, the first steps in identifying a recurrent regularity require the development of strategies for utilizing potential information from the environment.[11] The greater part of contemporary learning theory has tended to overlook this feature of the learning-process or has treated such information-using strategies as just another kind of response that gets reinforced, if it happens to be followed by tension-reduction. It should be said, however, that those who, in recent years, have worked more directly with problem-solving behavior and thinking have become increasingly more mindful of the importance of methods of information-utilization.

This study gives evidence, then, that learning and problem-solving may be more profitably viewed as identification of temporally or spatially extended patterns and that the process of learning or problem-solving be viewed as the development of means for isolating such regularities from the flow of irrelevant events that originate either in the environment, in the organism, or are produced by the organism's response to the environment.

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[11] J. S. Bruner, J. J. Goodnow, and G. A. Austin, *A Study of Thinking*, 1956, 1-330.

SOME INVARIANT FINDINGS UNDER THE METHOD OF SUCCESSIVE INTERVALS

By LYLE V. JONES, University of North Carolina

Invariance of results of scaling is an essential attribute of a method of scaling. The worth of a method is established only after it is demonstrated that results remain constant over a range of empirical situations with differing incidental conditions. As Stevens has remarked, "the scientist seeks measures that will stay put while his back is turned."[1]

The purpose of this paper is to specify a range of conditions over which estimates of scale-parameters obtained by the method of successive intervals have been demonstrated to remain relatively invariant. The method of successive intervals applies to data where each individual of a group assigns to each of a set of stimuli a categorical rating. Stimuli might be consumer items to be rated according to preference. They might be artistic productions to be rated according to esthetic value. They could be verbal statements rated according to degree of belief; or, the ratings might pertain to stimuli with clear physical counterparts, such as pitch or loudness of tones, brightness of visual forms, or heaviness of objects. The purpose of the method of successive intervals is so to transform the rating categories in accordance with the scaling-model that the numerical values may be taken to characterize responses to the stimuli on an equal-interval scale. It is important to distinguish scales derived by the method of successive intervals from what Stevens has called "category rating-scales."[2] As will be demonstrated, results from ratings by successive intervals are markedly different from those which depend upon arbitrary assignment of integers to categories of a rating-form.

Total invariance. Total invariance is achieved, when anchoring phrases and number of rating categories are changed, not only if the parameters of

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[1] S. S. Stevens, Mathematics, measurement and psychophysics, in S. S. Stevens (ed.), *Handbook of Experimental Psychology*, 1948, 21.

[2] Stevens, On the psychophysical law, *Psychol. Rev.*, 64, 1957, 153-181.

a scale associated with a set of marker stimuli remain unchanged but also if the scale-unit and the scale-origin remain fixed.

Consider the findings exhibited in Figs. 1 and 2. The names of 20 food-items were presented on each of two categorical rating-forms. One of these forms contained nine ratings, the other six. Descriptive adjectives served as labels for the categories. Each form was administered to a different sample of approximately 900 Army enlisted men, with instructions to check categories which best indicated their degree of like or dislike for each food-item.

Data from each of the two samples were independently scaled by the method of successive intervals to determine scale-values and discriminal dispersions (*i.e.* estimates of means and standard deviations of the hypothetical preferential distributions).[3] A scale-value of 0.00 was assigned the midpoint of the rating-form, representing a judgment of "Neither like nor dislike."

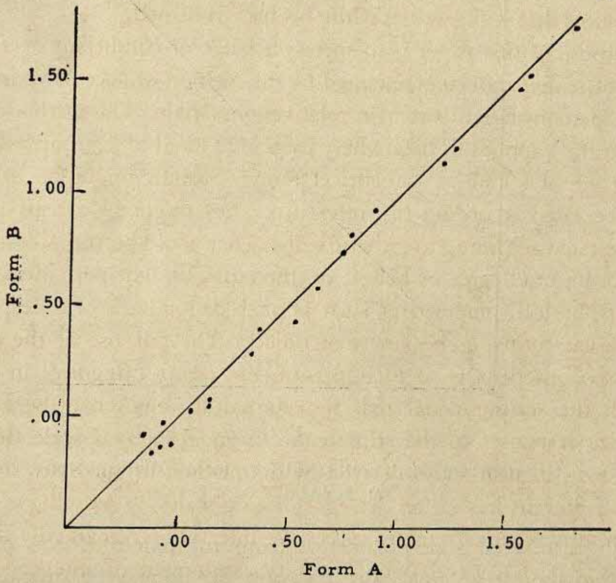

FIG. 1. ESTIMATED VALUES OF 20 FOOD-ITEMS ON TWO SUCCESSIVE INTERVAL RATING-FORMS

Form A, 912 *Ss*, 9 ratings: (1) dislike extremely; (2) dislike very much; (3) dislike moderately; (4) dislike slightly; (5) neither like nor dislike; (6) like slightly; (7) like moderately; (8) like very much; (9) like extremely.

Form B, 909 *Ss*, 6 ratings: (1) dislike extremely; (2) dislike very much; (3) mildly dislike; (4) mildly like; (5) like very much; (6) like extremely.

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[3] The present results stem from application of a graphical solution (see L. V. Jones and L. L. Thurstone, The psychophysics of semantics: An experimental investi-

The relation between the two sets of independently determined scale-values, Fig. 1, not only is linear, but clearly is fitted well by a line of unit slope and zero intercept, thus indicating total invariance of the two sets of scale-values. The same can be said of the discriminal dispersions, Fig. 2. While there is considerably greater variation about the line of best fit for estimates of dispersion, that line of best fit shown in Fig. 2 is given by the equation $\sigma_B = \sigma_A$.

These relations, between results derived from two forms of rating-scales, are not simply isolated examples. Such total invariance is also found when comparing estimated scale-parameters from any pair of forms of rating-scales from the 10 systematically investigated.[4] It may be concluded that,

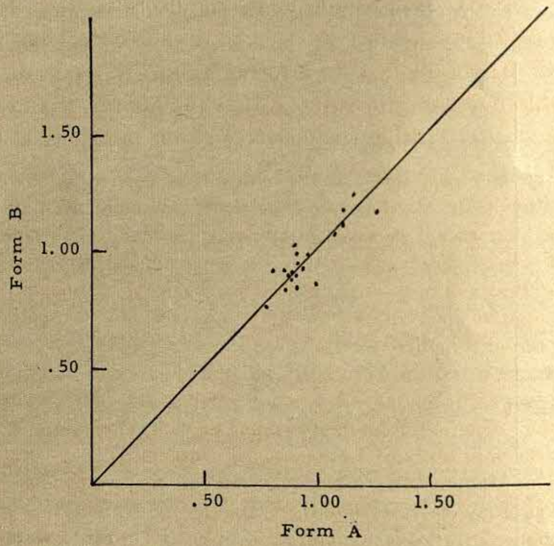

FIG. 2. ESTIMATED DISCRIMINAL DISPERSIONS CORRESPONDING TO THE SCALE-VALUES OF FIG. 1

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gation, *J. appl. Psychol.*, 39, 1955, 31-36) for both scale values and discriminal dispersions. There is ample evidence (L. V. Jones and R. D. Bock, Principles of Thurstonian scaling. Report No. 4, Food Acceptance Branch, Quartermaster Food & Container Institute, DA19-129-QM-774, Sept. 30, 1957, 1-202) that essentially the same results would be produced by a least squares solution (*e.g.* Harold Gulliksen, A least squares solution for paired comparisons with incomplete data, *Psychometrika,* 21, 1956, 125-134) or by other analytic forms of solution (*e.g.* R. H. Burros, The estimation of the discriminal dispersion in the method of successive intervals, *Psychometrika,* 20, 1955, 299-306 and H. J. A. Rimoldi and M. Hormaeche, The law of comparative judgment in the successive intervals and graphic rating scale methods, *Psychometrika,* 20, 1955, 308-318.)

[4] L. V. Jones, Optimal characteristics of a food preference schedule, Progress Report No. 8, Food Acceptance Branch, Quartermaster Food & Container Institute, (DA 44-109-QM-1370), January 31, 1953, 29-72.

when the same stimulus-items are presented to different random samples from the same population, results of scaling by the method of successive intervals are identical over changes in the number of categories on the rating-form and changes in the descriptive labels of the categories. Neither of these forms of invariance is found for statistics computed from arbitrary integers assigned to the categories.

Invariance when method is changed. The method of successive categories, as typically applied, assumes univariate normality of each hypothetical preferential distribution, taken over subjects. Rozeboom and Jones have demonstrated analytically that the error introduced by moderate departures from normality is slight.[5] Moreover, Adams and Messick recently have suggested certain direct testable consequences of the normality assumptions.[6]

An alternative empirical check on the normality-assumption, depending upon an alternative scaling-model developed by Thurstone,[7] has been investigated by Jones and Thurstone.[8] Let each *S* be asked to give repeated ratings on a rating-form. Assume that his successive attempts to rate the same stimulus-items reflect a normal distribution of errors over the hypothetical, psychological continuum. As with the method of successive intervals, we do *not* assume that the distributions have the same variance for all stimuli.

In particular, let us ask the *S*s to rate a set of stimulus-items on two distinct occasions. By varying order of the items and the appearance of the two forms, it is possible to reduce objections from the *S* to doing the task twice.

The basic data to be analyzed are conditional distributions of response to items on the second administration, given a particular response to corresponding items on the first administration. With a seven-category questionary, we obtain seven such conditional distributions for every stimulus-item. In general, where n is the number of stimulus-items and k is the number of categories, we would obtain nk distributions. Estimates of scale-values of interval boundaries are obtained from that transformation which is most effective in simultaneously normalizing each of the nk distributions.

This procedure was carried out with two seven-category forms, each containing the names of the same 20 food-items, and each being administered to a group of approximately 250 Army enlisted men. The 20 foods were identical to those scaled from ratings of 900 Army enlisted men, presented in the preceding section. When the scale-values from the successive intervals are plotted against the scale-values obtained from repeated administration, the results are those exhibited in Fig. 3. The two sets of scale-values are invariant except for the slope of the relationship; the different unit of measurement for the two scaling-methods is the only source of systematic divergence of results. From Fig. 4 it is apparent that the same relationship holds for estimates of standard deviations of the preference-distributions, but to a less precise linear fit.

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[5] W. W. Rozeboom and L. V. Jones, The validity of the successive intervals method of psychometric scaling, *Psychometrika,* 21, 1956, 165-183.

[6] Ernest Adams and Samuel Messick, An axiomatic formulation and generalization of successive intervals scaling, *ibid.,* 23, 1958, 355-368.

[7] L. L. Thurstone, Some new psychophysical methods, *Proc. N. R. C. Symposium on Food Preference Testing Methodology,* 1954, 100-104.

[8] Jones and Thurstone, Psychophysics and the normality assumption: An experimental report, *Proc. N. R. C. Symposium on Food Preference Testing Methodology,* 1954, 105-112.

In another study, the same procedures were applied to ratings for nine-category preferential schedules. Not only were the forms of results the same as those represented in Figs. 3 and 4; the slope of the resulting relationship was indistinguishable for the two studies. This strongly suggests that the absolute invariance under changes in form of rating-scale, already noted, applies to the method of scaling from repeated administration as well as to the method of successive intervals.

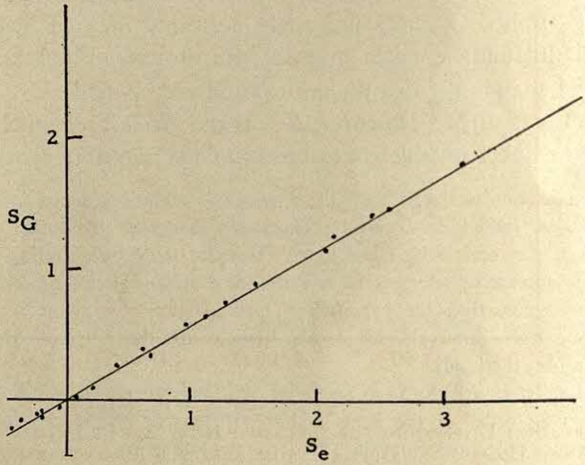

FIG. 3. SCALE VALUES FROM THE METHOD OF SUCCESSIVE INTERVALS (S_G) PLOTTED AGAINST VALUES DERIVED FROM REPEATED ADMINISTRATION (S_e)
(Twenty food-items rated on seven-category form; $N = 253$.)

These comparisons of the method of successive intervals with a method of repeated administration clearly support a generality of the former. It has been demonstrated that when the assumption of normally distributed preferences is replaced by the innocuous assumption of normality of errors from repeated administration, resulting estimates of scale-parameters are invariant up to a linear multiplier, *i.e.* except for unit of measurement.

Invariance inferred. The relation of predictive accuracy to invariance is analogous to that of validity to reliability of tests. Thus, a necessary condition for a scaling-model accurately to predict classes of behavior, other than that from which it was derived, is that the scale results remain invariant over all such classes of behavior.

Several sets of findings attest to the predictive power of the method of successive intervals. It has been demonstrated in a number of studies that results from successive intervals serve to predict results obtained by the method of paired comparisons, when the same stimuli are involved in both methods.[9] Further, from the results of the method of successive intervals, the prediction of choice of one stimulus from three or more stimuli is accurately achieved.[10] The scaling-model can even be extended to allow satisfactory predictions, from parameters of preference estimated by the method of successive categories, of the relative frequency of purchase of consumer items.[11] Such predictive success demonstrates that the scale-results are not transient and are not highly dependent upon incidental conditions of measurement but rather are invariant constructs applicable over a wide range of behavior, for only if the model serves to reflect rather stable aspects of distributions of preference could it have the generality to which these results attest.

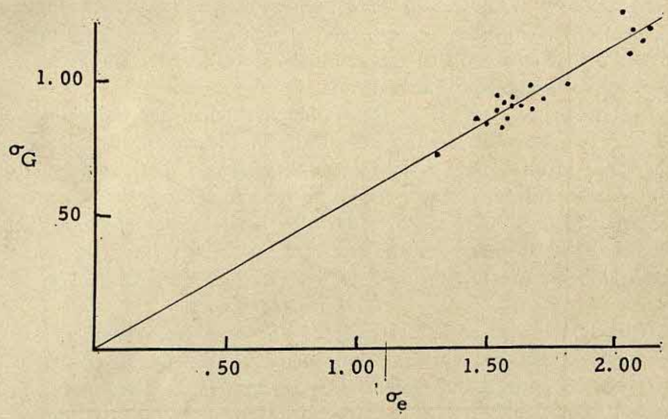

FIG. 4. DISCRIMINAL DISPERSIONS FOR SUCCESSIVE INTERVALS (σ_G) PLOTTED AGAINST DISCRIMINAL DISPERSIONS DERIVED FROM REPEATED ADMINISTRATION (σ_e)
(Twenty food-items rated on seven-category form; $N = 253$.)

Invariance when stimuli and Ss are changed. For some practical situations, it is convenient to have a 'yardstick' for psychological measurement which remains stable over time and which can be repeatedly applied for a given measuring purpose. Such a situation is found, for example, in studies of consumer-acceptance. It may be desirable to measure acceptance of different consumer-products at varying times (using different samples of consumers), but with the assurance that results are comparable from one survey to another.

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[9] A. L. Edwards and L. L. Thurstone, An internal consistency check for scale values determined by the method of successive intervals, *Psychometrika,* 17, 1956, 125-134; M. A. Saffir, A comparative study of scales constructed by three psychophysical methods, *ibid.*, 2, 1937, 179-198.

[10] R. D. Bock, A generalization of the law of comparative judgment applied to a problem in the prediction of choice, *Amer. Psychologist,* 11, 1956, 442 (abstract).

[11] Jones, An experiment in the prediction of purchase, *Amer. Psychologist,* 11, 1956, 443 (abstract).

A nine-category hedonic scale has been used for several years by the Quartermaster Food & Container Institute for the Armed Forces to assess the acceptance of food among troops.[12] While results have been summarized primarily in terms of the assignment of the arbitrary integers, one through nine, to responses in the categories, it has been recognized that the resulting 'scores' cannot always legitimately be considered to fall on an equal-interval scale.

A deterrent to the routine use of the scaling method of successive intervals for food-acceptance has been the relative complexity of the computational procedures. While the labor involved need not be great, it is always more complex than the alternative of arbitrarily assigning numbers to the rating-categories. This deterrent would be removed if it were discovered that a single scale-transformation could legitimately be used to determine the equal-interval scale-values representative of the categorical boundaries on the hedonic scale for all national surveys of food acceptance.

To investigate the degree of invariance of one scale from another, data were obtained from six national surveys administered by the Quartermaster Food & Container Institute between February and September, 1951. The surveys involved different but comparable lists of 54 food-items each. Preferential ratings were obtained from 2500 to 4000 *Ss*. The sizes of the samples are given in Fig. 5.

The results from each survey were separately treated by the method of successive intervals to determine widths of the hedonic categories in terms of an equal unit-scale. The widths of the categories appear in Fig. 5. The inequality of widths of Categories 2-8 is apparent. Noteworthy is the stability of estimates of the width of each category from one survey to the next. It is to be remembered that the surveys involve distinct samples of respondents and also that they differ with respect to the foods evaluated by the respondents.

In view of the high degree of stability of the widths of the categories for the various surveys, a standard scale for category boundaries seems warranted.[13] Utilizing the standard for any of the samples studied would provide estimates for scale-values and dispersions of the food-items nearly identical with those obtained directly from the data of that sample.

The case against categorical scales. Recent papers by Stevens and by Stevens and Galanter may be interpreted as presenting a strong case against the use of the so-called category-scales.[14] This method entails arbitrary assignment of successive integers to the successive categories of a rating-form. Direct interpretation of categorical ratings in terms of assigned

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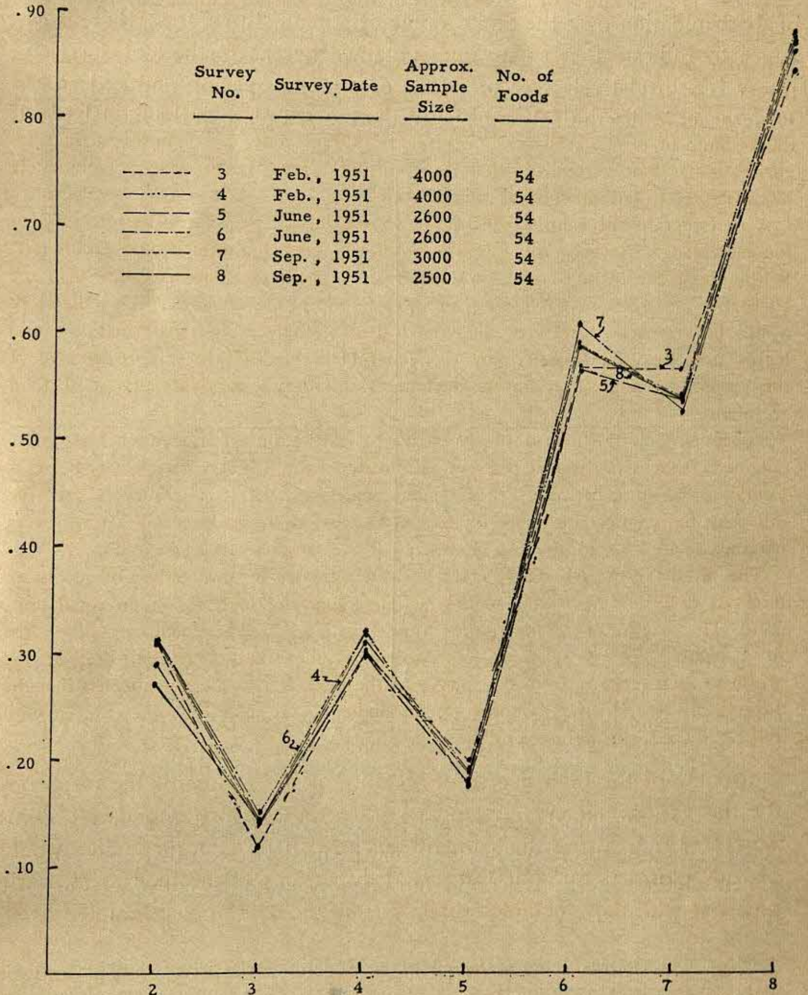
[12] B. W. Polemis, Summary and analysis of data obtained from first national survey of food preferences in the armed forces, Interim Report No. 3, Quartermaster Food & Container Institute, 7-84-15-007, February, 1951, 1-48.

[13] Jones, A standard equal interval scale for Quartermaster national food acceptance surveys, Report No. 6, Food Acceptance Branch, Quartermaster Food & Container Institute, (DA 19-129-QM-272) September 30, 1956, 23-27.



Survey No.	Survey Date	Approx. Sample Size	No. of Foods
3	Feb., 1951	4000	54
4	Feb., 1951	4000	54
5	June, 1951	2600	54
6	June, 1951	2600	54
7	Sep., 1951	3000	54
8	Sep., 1951	2500	54

FIG. 5. WIDTH OF CATEGORIES 2–8 OF THE QUARTERMASTER'S HEDONIC SCALE
Determined from the last six of eight preferential surveys, the first two of which were omitted as they were not comparable to the others.

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[14] Stevens, On the psychophysical law, *Psychol. Rev.*, 64, 1957, 153-181; S. S. Stevens and E. H. Galanter, Ratio scales and category scales for a dozen perceptual continua, *J. exp. Psychol.*, 54, 1957, 377-411.

integers, with or without equal-interval instructions to *S*s, is severely limited by lack of invariance. Ratings are clearly sensitive to the form of distribution of stimuli, particularly to stimulus-spacing. Ratings also are greatly affected by end effects of the rating-form and by non-uniform ease of discrimination over the entire range of stimuli. In contrast, there is evidence to indicate that scale-values obtained by the method of successive intervals are relatively free of effects of stimulus-spacing and end effects. Differences in discrimination have, furthermore, a place in the model, affecting size of discriminal dispersions, but not scale-values. It also should be noted that while successive intervals may be used successfully to predict consumers' choice, direct prediction from category-scales has not proven successful.

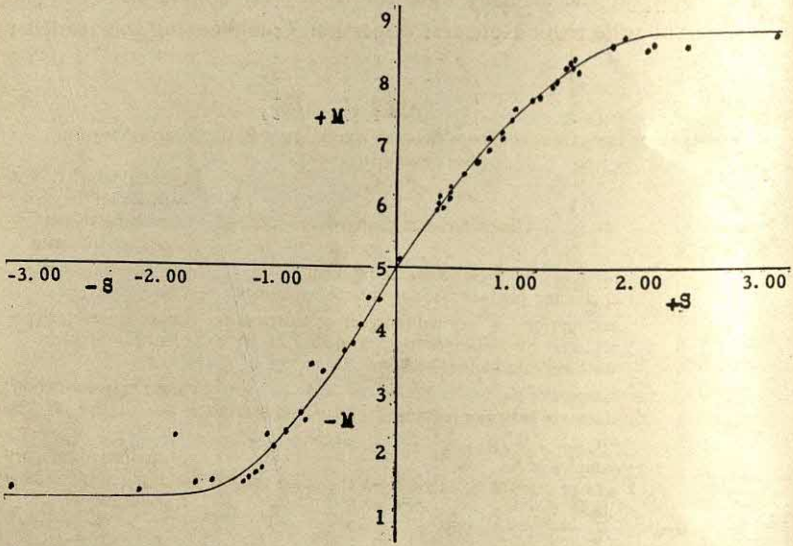

FIG. 6. CATEGORICAL MEANS (M) PLOTTED AGAINST SCALE-VALUES OBTAINED BY THE METHOD OF SUCCESSIVE INTERVALS
(Fifty-one adjectives rated on a nine-category form; $N = 905$.)

For a wide range of stimuli, an observed relation between category-means and scale-values of successive intervals is shown in Fig. 6. The stimuli are 51 descriptive adjectives rated along a meaning continuum from a denotation of greatest dislike to greatest like.[15] For either half of the continuum, like or dislike, the relationship is monotonic but concave toward the abscissa. Similar relations are found for preference, but since stimuli generally assessed are more homogeneous, the relationship may show less marked departure from linearity.[16]

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[15] Jones and Thurstone, The psychophysics of semantics: an experimental investigation, *J. appl. Psychol.*, 39, 1955, 31-36.

A more striking discrepancy of category-results from successive intervals-results appears in regard to measures of dispersion. Typically, a near-zero correlation is found between standard deviations of stimulus-distributions over the category-scale and discriminal dispersions found by successive intervals. This finding would appear to be a complex result of several factors. A component of negative correlation is due to end effects, curtailment of dispersion on the category-scale for those very stimuli which on an equal-interval scale exhibit greatest dispersion. Counteracting this tendency is a general positive relation between the two sets of estimates of dispersion for stimuli in the moderate range of the scale. Speaking more generally, the finding of zero correlation between standard deviations on the category-scale and discriminal dispersions on the equal-interval scale may be attributed to the unequal widths of the categories.

TABLE I

SUMMARY OF INVARIANCES DEMONSTRATED FOR SCALING RESULTS FROM METHOD OF SUCCESSIVE INTERVALS

Situation	Characteristics changed	Degree to which (i) scale values and (ii) discriminal dispersions are invariant*
1	(a) number of categories on rating form (b) anchoring phrases associated with categories	identity-transformation
2	assumption of normal subjective distribution replaced by assumption of normality of errors with repeated administration	linear transformation, identical intercept
3	(a) samples of *S*s. (b) classes of behavior regarding the same set of stimuli	linear transformation
4	(a) samples of *S*s. (b) sets of stimuli (selected from the same general class)	invariance of category widths to an identity-transformation

* Except, as noted, for Situation 4.

Summary and conclusions. The findings regarding invariance of the results of scaling are summarized in Table I.

By the method of successive intervals, two parameters usually are estimated to represent each stimulus on an equal-interval, psychological scale, the scale-value or mean of the subjective distribution and the discriminal dispersion or standard deviation. It has been demonstrated (Situation 1) that, under changes in the number of categories and anchoring phrases used to define categories of a rating-form, these estimates are related by an identity-transformation. Results are invariant up to a linear multiplier when the assumption of normality of subjective distribution is replaced by an assumption of normal error-distribution of differences in subjective values obtained from repeated ratings (Situation 2). Invariance of results up to a linear transformation was inferred from the predictive power of those results (Situation 3). Invariance also was displayed (Situation 4) when the same scale-form was used for ratings of different sets of stimuli and was administered to distinct samples of respondents. In contrast to these findings, with direct interpretation of category-ratings using so-called category-scales, many of these forms of invariance do not appear.

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[16] Jones and Thurstone, Psychophysics and the normality assumption: An experimental report, *Proc. N. R. C. Symposium on Food Preference Testing Methodology,* 1954, 105-112, see p. 109.

The invariance of results from the method of successive intervals over a wide range of conditions supports the adequacy of that scaling-model and its potential usefulness for predictive purposes.

A STUDY OF MEMORY FOR VISUAL FORM

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In spite of a great deal of work on memory for visual form, we still have no adequate information about the fate of the memory-trace. Considerable effort has been expended in the direction of eliminating objectionable flaws in the procedure used by Wulf in his attempt to verify the gestalt theory of memory-change,[1] and it is only now that a proper experimental design has been evolved.[2] For the most part the results obtained offer little comfort to those who believe that the trace changes in the direction of a more *prägnant* organization, but even the latest investigations suffer from one or more of the following limitations: (1) the time-interval tested is not sufficiently great;[3] (2) too many figures are used;[4] (3) *S* is told he will later be tested for his memory of the figure;[5] (4) the test of recognition follows the test of reproduction;[6] (5) the recognition-test is too sensitive, most alternatives being highly similar to the original;[7] (6) the recognition-test does not provide a *measure* of change.

The present study was not designed to test Wulf's hypothesis in the direct sense of selecting figures which were asymmetrical and whose traces might, therefore, be expected to become more *prägnant*. Nor was an effort made to include alternatives on the recognition-tests which were better

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[1] Friedrich Wulf, Tendencies in figural variation, in W. D. Ellis (ed.), *A Source Book of Gestalt Psychology,* 1941, 136-148.

[2] N. G. Hanawalt, Memory trace for figures in recall and recognition, *Arch. Psychol.,* 31, 1937 (No. 216), 5-89; Erich Goldmeier, Progressive changes in memory traces, this JOURNAL, 54, 1941, 490-503; D. O. Hebb and E. N. Foord, Errors of visual recognition and the nature of the trace, *J. exp. Psychol.,* 35, 1945, 335-348.

[3] Hebb and Foord, *op. cit.,* 335-348. Recently, however, the time-interval was extended somewhat with similar results, See F. H. George, Errors of visual recognition, *J. exp. Psychol.,* 43, 1952, 202-206.

[4] Hanawalt, *op. cit.,* 5-89; Goldmeier, *op. cit.,* 490-503.

[5] Hebb and Foord, *op. cit.,* 335-348.

[6] Hanawalt, *op. cit.,* 5-89; Goldmeier, *op. cit.,* 490-503. It has recently been shown that the act of reproducing interferes with the accuracy of recognition. See M. B. Turner and E. A. Craig, The effect of figural reproduction on recognition, *J. Psychol.,* 1954, 38, 265-270.

[7] Goldmeier, *op. cit.,* 490-503; Hebb and Foord, *op. cit.,* 335-348; Hanawalt, *op. cit.,* 5-89.

figures in a direction in which change might be predicted. Rather, the experiments attempted to answer the question whether the trace of a visual form, insofar as it represents the aspect of shape, changes in *any* fashion over time or remains unchanged. More emphasis was placed on shape than in some previous studies where very minor changes in one part of a figure (for example the size of a gap in a broken circle) were studied; and the design precluded consideration of possible changes in size or orientation. Evidence of no change in the trace *qua* shape would, however, argue against Wulf's thesis. The undesirable features of his original procedure, such as testing by the method of successive reproduction, were eliminated and, in addition, steps were taken to eliminate the other shortcomings described above.

Another purpose of the present study was to compare results obtained using recognition with those obtained using reproduction. Granted that only the results of recognition can be taken as evidence concerning the nature of the trace insofar as it represents remembered shape, the nature of reproduction as a process remains of interest.

EXPERIMENT I

The main features of the design of Experiment I are as follows:

Procedure. Serving under the impression that he was participating in an experiment on extrasensory perception, *S* was shown a figure (No. 10 in Fig. 1) for 20 sec. He was then dismissed and as far as he knew the experiment was at an end. After a predetermined time-interval—15 sec., 1 day, 1 wk., or 3 wks.—he was recalled and asked either (a) to reproduce the figure as accurately as possible, or (b) to select it from among those shown on the recognition-blank (Fig. 1). No time-limits were imposed on *S* in either case. For approximately half the *Ss* in (b)—the recognition-test—the time required for them to make a choice was measured.

The basis for selecting the figures shown in Fig. 1, was as follows: It was thought desirable to have a range of 10 levels of similarity to the figure exposed which ranged from identity (rated 10) to a figure so different in shape that the only point of similarity was in its being a closed rectilinear form (rated 1). This sort of test does not contain so many alternatives as to tax *S*'s patience and thereby detract from attention to each, nor does it contain so few that it is insufficiently sensitive or fails to cover the range from 'quite different' to 'quite similar.' The figures finally used were chosen by four judges from among a large group of actual reproductions made by our *Ss*. In this way we hoped to include alternatives most likely to epitomize trace changes, if such there be, as indicated by reproductive results. The judges agreed in the rating of these figures on a 10-point scale (10 being identity) within ±1 rank.

Interpretation of the recognition-choices is based on the assumption that *S*'s choice on the test mirrors the state of the trace; choice of the correct figure means that the trace is closer to that one than to the next most similar one (rated 9). Since the latter figure is itself very little changed from the original, the trace must be sufficiently good to allow *S* to make this fine discrimination; it must be more like the original than this very similar figure, which means, in this case, that it really cannot have changed at all. The same argument applies to the selection of the alternative rated 9 over that rated 8 and so on. It is of course true that the results depend upon the particular alternatives available. A given *S*'s trace may change in a particular way for which no alternative is available which matches it or even approximates it. (Indeed, from this point of view it would be impossible ever to design a multiple-choice test which would include the appropriate items for all *Ss*, no matter how many alternatives were presented.) If this happens, one would have to predict random choice. On the other hand, beyond-chance selection of the correct figure rules out such an interpretation. Change in a direction not represented among the alternatives (except perhaps for qualities other than shape, as, for example, size) could not be expected to lead to choice of the correct figure except occasionally by chance.

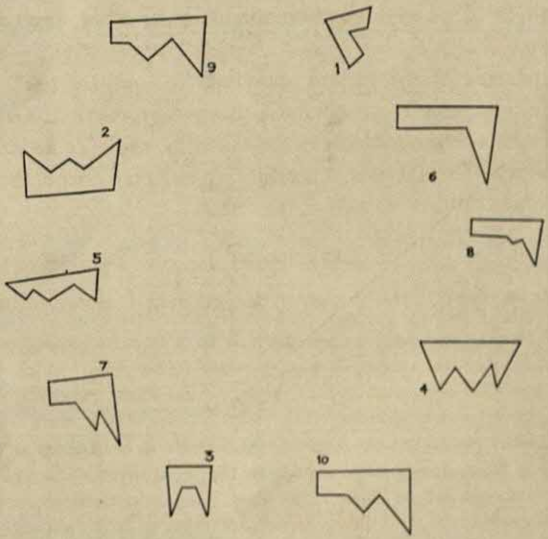

FIG. 1. TEST OF RECOGNITION IN EXPERIMENT I
(The numbers represent the average rating of similarity to the presented figure—numbering 10—but were not present on the test.)

The procedure for the scoring of reproductions was as follows. Each reproduction was rated by several judges (five for the individual data and three for the group data) working independently. The judges were not aware of the time-interval at which the reproductions were obtained. They were instructed to rate on a scale identical to the one used for the recognition-test, and the figures of the recognition-test with the corresponding rank-scores were given and remained in view as a reference. The final score for each reproduction was the mean of all judges' ratings.

Subjects. Since the *Ss* at each of the four time-intervals were tested for either recognition or reproduction, there were eight different conditions. There were approximately 20 *Ss* in each condition, half of whom were tested individually and half in groups. The *Ss* tested individually were students at the New School for Social Research; those in groups were undergraduate students in psychology at the City College of New York. All the *Ss* were uninformed as to the purpose of the experiment.

Results. Fig. 2 shows the median scores for reproduction and recognition for all the conditions studied.[8] Medians rather that means are given because it is questionable whether the steps along the 10-point scale can be considered equal to one another.

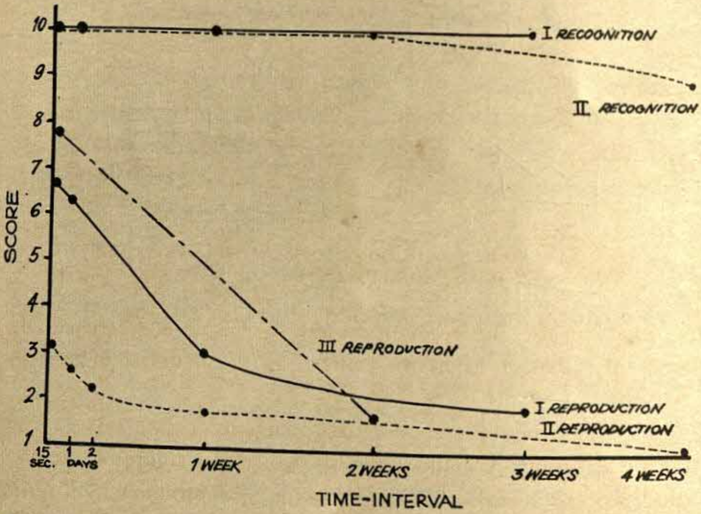

FIG. 2. CURVES OF FORGETTING FOR RECOGNITION AND REPRODUCTION
(The Roman numerals indicate the experiment.)

As can be seen from the graph, the median recognition-score was 10 at each interval. Only 3 of the 17 *Ss* scored less than 10 at the 3-week interval. It is, therefore, clear that the trace must remain relatively unchanged as far as shape-representation is concerned. The Kolmogorov-Smirnov two-sample test, comparing the results at the first and the last time-interval, gives no suggestion of significance. It is also clear from Fig. 2 that the accuracy of reproduction is considerably less than the accuracy of recognition.[9]

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[8] The results for *Ss* given the training figure individually and in classes are combined. Although the level of performance is not always identical under these two conditions, the general trend is about the same.

Strictly speaking, the scores for reproduction and recognition are not directly comparable because the same judges did not rate both sets of figures and because the two sets may have generated different subjective scales. Because all judges are given the same instructions, and because the judges of reproduction use the recognition-test figures as a reference, there is, however, little danger of doing violence to the material in making such comparisons.

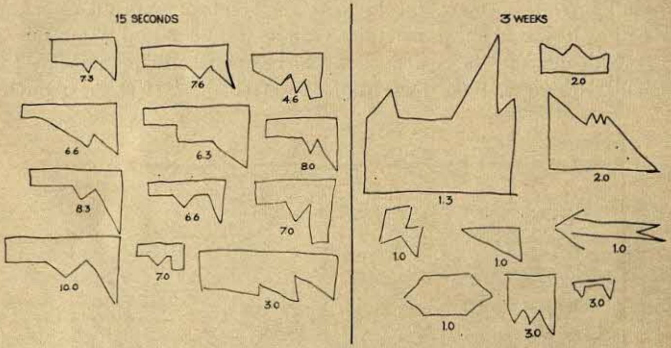

FIG. 3. SOME SAMPLE REPRODUCTIONS WITH THEIR CORRESPONDING AVERAGE RATINGS BY JUDGES FOR THE 15 SEC. AND 3-WK. INTERVALS

Fig. 3 shows some representative reproductions for the 15-sec. group and for the 3-wk. group. A glance at these two sets of figures (and the corresponding scores) reveals that the accuracy of reproduction declines with time. Between one day and three weeks, the median value drops from 6.6 to 2.0. The four conditions differ very significantly according to the median test.

EXPERIMENT II

Experiment II was undertaken to determine whether the results of Experiment I could be reproduced with another figure—one of a different type. It also provided an opportunity to introduce a few additional improvements in design.

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[9] In this and the two subsequent experiments, the agreement among judges' ratings of reproductions was quite good. From among the 22 conditions that were separately judged in the three experiments, a random sample of 6 were chosen and Kendall's Co-efficient of Concordance (*W*) was determined for each. The resultant *W*s were significant in each case (most at less than the 1% level), the average being 0.73.

Procedure. The procedure followed that of Experiment I with the following differences. (1) A curvilinear figure was used (No. 10 in Fig. 4). It was thought that a figure of this type lends itself less to the employment of cognitive aids than the one used in Experiment I. (2) Instead of disguising the procedure as an experiment in *ESP*, which may actually have encouraged *S* to think about the nature of the figure in spite of instructions, a different technique was used. The inspection was disguised as an experiment on visual after-imagery. (3) Upon completion of the reproduction from memory, the *Ss* in this experiment were asked to copy the original figure. It was thought that direct copies would afford a measure of the extent to which difficulty in reproduction can be thought to be a motor (or perceptual-motor) difficulty. Since these copies were obtained after the reproductions were made, there was no danger of contaminating the main results in any way. (4) Two different multiple-choice blanks were prepared (one of which is shown in Fig. 4) embodying two different arrangements of the figures. Half the *Ss* were tested with one blank and half with the other. This procedure controls for a possible position-preference in choosing one of the figures. (5) Four judges ranked the reproductions and also the direct copies for the individual data and five did so for the group data.

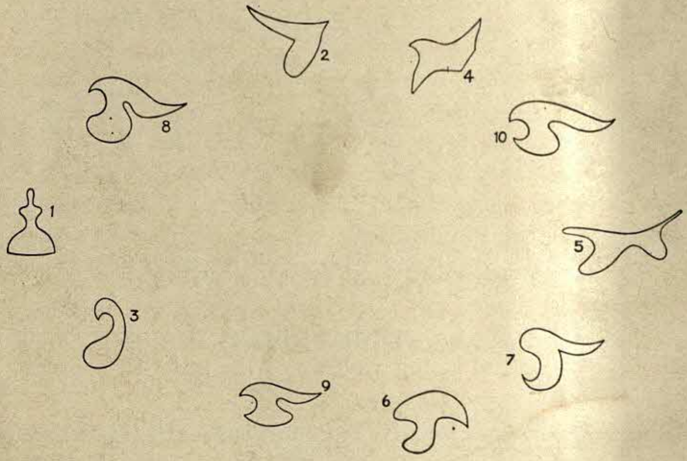

FIG. 4. TEST OF RECOGNITION IN EXPERIMENT II

Subjects. For recognition, the following time-intervals were tested: 15 sec., 1 d., 1 wk., 2 wk., 4 wks. For reproduction, these and one additional interval, 2 d., were tested. There were thus 11 different conditions with approximately 20 *Ss* in each, half tested individually and half in classes. The *Ss* tested individually were drawn from an undergraduate class at Brooklyn College and those tested in groups were undergraduate students of psychology at the City College. All *Ss* were naïve and they were enjoined not to discuss the experiment with their classmates.

Results. Fig. 2 shows the curve of recognition for the five time-intervals. It is apparent that the trace, insofar as it represents shape, has not undergone very much change within the period covered. The last interval-report does differ significantly from the first but no interval differs significantly from its neighbor (Kolmogorov-Smirnov Test). In fact, 72 of the 102 *S*s selected the identical figure in spite of the fact that a few other test-figures are very similar to it even in direct comparison. Even after four weeks, 9 *S*s selected the correct figure and 6 *S*s the one rated 9. Thus the greater subtlety of this figure than the one used in Experiment I does not seem to lead to any greater difficulty in accurate preservation of its trace.

The accuracy of reproduction is even poorer than in Experiment I. After only 15 sec., the median reproduction-score is 3.2, a value the significance of which can best be understood by looking at the recognition-figure rated 3. The poorer initial performance in reproduction in this experiment is no doubt due to the increased difficulty in reproducing from memory the more subtle curvilinear figure. As a result, one cannot expect as much decline thereafter for this figure as for the one employed in the first experiment. The poor reproduction-scores must be viewed in relation to the excellent scores obtained by these *S*s for their direct copies of the original figure. The lowest such score for any *S* at any interval tested was 6.0 and the median value for the 118 *S*s was 8.25. There was no appreciable difference in the median scores of these copies for each of the six groups.

The medians for the reproductions of the six groups suggest that there is a decline in accuracy between the 15-sec. period and the 4-wk. period in this experiment. The difference between groups is significant at the 1% level according to the median-test.

In Experiment III still another figure was employed, testing reproduction only, after 15 sec. and 2 wk., using the procedure of Experiment II. The median scores were 7.7 and 1.7 respectively. The difference is highly significant according to the median-test.

DISCUSSION

The results of these experiments are clear as far as the question of accuracy of retention of perceived shape is concerned. The excellence of recognition with little decrement over time means that on the whole the trace, as far as it preserves phenomenal shape, remains unchanged. Since the majority of selections are of the correct figure, it cannot be argued that for many *S*s the trace had changed to the representation of a shape not present among the test-alternatives.

This conclusion seems to contradict that of Crumbaugh, who recently studied this problem using the method of identical stimuli developed for the study of time-errors and first used for the present purpose by Irwin and

Seidenfeld.[10] His results suggest that the trace undergoes certain changes—depending upon the figure employed—in the first 12 sec. after the figure is seen. Actually one can only say that there is a change in the number of 'more symmetrical' reports over time, which may or may not indicate the magnitude of the presumed change. We suspect that these changes are either so small as to leave the general shape more or less the same, or that they are transitory. Hanawalt did not obtain evidence of directional change when he used this technique with intervals up to one week, although it is true his study suffers from the use of several figures for each *S*.[11]

The finding that reproduction does deteriorate over time is interesting in the light of the above. It suggests that although its shape does not change, the trace does tend to become less available as time goes on. It is well known that recognition may succeed when recall or reproduction has failed. The arousal of a trace which underlies recognition occurs easily apparently because the perceptual process (which takes place when the same object is seen again) is able to exercise a selective influence in some as yet unexplained fashion. The presence of the various choices on the test renders it unnecessary for the trace to rise to consciousness as an image. It need only 'approve' or 'veto' each alternative. In the case of reproduction, however, a percept is not given by the presence of a stimulus-object, and if *S* is to have any remembered impression of the shape it must be because the trace yields some conscious manifestation of itself. This is obviously a difficult matter, as is evidenced by the poor performance of our *S*s. That this interpretation is correct is supported by the finding that the difficulty is not on the side of the motor execution of the drawing. When *S* has a form given in direct perception, he can copy it quite accurately; apparently then, the poor performance in reproduction from memory is due to the fact that the previously seen form does not appear with sufficient clarity before the mind's eye to allow *S* to draw it. The evidence that reproduction deteriorates over time suggests that this process of calling up an image or mental impression becomes more difficult as time goes on. The trace becomes less available for evocation of imagery. The somewhat poorer performance in reproducing the curved figure used in Experiment II than in reproducing the simple, rectilinear one used in Experiment I also supports the above interpretatio because it is plausible that it would be more difficult to bring to mind complex or subtle figure than a simple one. The results from recognitio tend to show that in both cases the trace itself is preserved equally well.

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[10] J. C. Crumbaugh, Temporal changes in the memory of visually perceived form, this JOURNAL, 67, 1954, 647-658; F. W. Irwin and M. A. Seidenfeld, The application of the method of comparison to the problem of memory change, *J. exp. Psychol.*, 21, 1937, 363-381.

[11] Hanawalt, The method of comparison applied to the problem of memory change, *J. exp. Psychol.*, 43, 1952, 37-42.

There were a number of reproductions which could be considered to more regular, symmetrical, or, in short, better figures than the origin in all three experiments, and, consistent with expectations based upon th declining accuracy of reproduction, there were more such instances at th longer time-intervals. Had we tested reproduction only, this finding mig be interpreted as support for Wulf's hypothesis. Since we know from th results of the test of recognition that there was virtually no change in th trace as such, we can conclude that these cases have to do with the difficul in the resurrection of the trace when it must be aroused for reproductio Thus it is suggested that much of the evidence, which does seem to suppo Wulf's hypothesis is explicable as a function of the reproductive proces

With the longer time-intervals, not quite as many *S*s succeed in selectin the identical figure, particularly in Experiment II. Is this to be interprete as evidence that for these *S*s the trace had changed such that it resemble more the figure selected than the original? This is improbable because up one week there is no such trend evidenced. A more plausible interpretatio is that there is an increasing unavailability of the trace which eventual makes itself felt even in recognition, in that *S* is no longer certain whic alternative is correct. This view is supported by the fact that there is a unmistakable increase in the average time required to make a choice this test in both experiments as the length of interval increases, althoug such measures were only obtained for about half the *S*s and are not by an means precise.

SUMMARY

Figures were shown to *S*s who did not know they would later be teste and at varying time-intervals thereafter they were asked to select the figur previously seen from among alternatives (recognition) *or* to reproduce from memory (reproduction). Each *S* was tested at one interval only. Th results for recognition showed virtually no decline in accuracy in two experi ments up to three or four weeks. The results for reproduction in the tw experiments showed a marked decline in accuracy over time. These finding were interpreted to mean that although the trace, insofar as it represent shape, does not change, it does become increasingly unavailable over time

INCIDENTAL AND INTENTIONAL LEARNING OF ISOLATED AND CROWDED ITEMS

By IRVING J. SALTZMAN and TERESA S. CARTERETTE, Indiana University

The earliest study comparing the learning of isolated and crowded materials was performed by Von Restorff.[1] She found that an isolated item of one type, *e.g.* a number, embedded in a series of items of a different type, such as nonsense-syllables, was recalled much better than the homogeneous, crowded items. Her interpretation of this effect was in terms of gestalt memory-traces and perceptual organization. Von Restorff's procedure, however, was soon criticized because of the confounding of the absolute numbers of the two types of items with the degree of perceptual isolation.[2] This confounding meant that the findings could be interpreted in terms of intra-list interference as well as in terms of the degree of perceptual isolation. To control the absolute frequencies of crowded and isolated items, investigators now use lists in which the two types of material occur the same number of times but still appear in varying degrees of isolation.[3]

A recent study by Postman and Phillips was devoted to a comparison of intentional and incidental learning of crowded and isolated materials.[4] The results of their study were surprising. Not only did they find that isolated items were not retained better than crowded items by the incidental learners, but also that, in one of their two experimental situations, the intentional learners failed to show an effect of isolation. The experiments to be described here were also concerned with a comparison of intentional and incidental learning of crowded and isolated materials. Although several procedural differences exist between our study and theirs, the findings are in essential agreement. Since the procedural changes were made in the hope that they would increase the probability of finding effects of isolation on incidental and intentional learning, our negative findings are even more surprising than those of Postman and Phillips.

In our first experiment, we used the same stimulus-material that Postman and Phillips used, a list of 20 items—10 nonsense-syllables and 10 three-digit numbers. This list is the same one that other investigators have used to compare the learning of isolated and crowded material.[5] Since the mean number of items learned in the Postman and Phillips study was quite low, we attempted to get better learning by presenting the list twice, instead of once, and exposing each item for 6 instead of 4 sec. One other difference between our procedures should be mentioned. In our

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[1] Hedwig von Restorff, Ueber die Wirkung von Bereichsbildungen, im Spurenfeld, *Psychol. Forsch.*, 18, 1933, 299-342.

[2] P. S. Siegel, Structure effects within a memory series, *J. exp. Psychol.*, 33, 1943, 311-316.

[3] Leo Postman and L. W. Phillips, Studies in incidental learing: The effects of crowding and isolation, *J. exp. Psychol.*, 48, 1954, 48-56; E. V. Saul and C. E. Osgood, Perceptual organization of materials as a factor influencing ease of learning and degree of retention, *ibid.*, 40, 1950, 372-379; Siegel, *op. cit.*, 311-316.

[4] Postman and Phillips, *op. cit.*, 48-56.

[5] Saul and Osgood, *op. cit.*, 372-379; Siegel, *op. cit.*, 311-316.

study, one investigator, the junior author, ran all of the *Ss*, both incidental and intentional, individually. Postman and Phillips employed the procedure previously used by Jenkins, where the *Ss* were run in pairs, with one acting as *E* (incidental learner) for the other who served as *S* (intentional learner).[6]

In our second experiment, the learning material, a list of 14 two-digit numbers, differed from that used by Postman and Phillips. We hoped to be able to achieve a greater degree of isolation with our material than Postman and Phillips did with theirs.

EXPERIMENT I

Method: The *Ss* in Experiment I were 60 Indiana University undergraduates, randomly divided into two groups of 30 each, an incidental and an intentional learning-group. The learning-material consisted of two 20-item lists, each composed of the same 10 nonsense-syllables and 10 three-digit numbers. The two lists are shown in Table I. The syllable-number positions in one list were reversed in the other. Half of the *Ss* in each group were shown one list and the other half, the other list. In this manner the influence of the kind of isolated item, syllable or number, was controlled. Items 3 and 13 were the most isolated in the list, the nearest item of the same type being at least three items removed. The list was presented by means of an electrically operated memory-drum. The rate of presentation was 6 sec. per item. The list was shown to each *S* twice with a 1-min. interval between trials. The arrangement of items was the same on both trials.

TABLE I

STIMULUS-LISTS

Serial position	List 1	List 2	Serial position	List 1	List 2
1	GUB	341	11	341	GUB
2	KEV	258	12	258	KEV
3	406	SIH	13	SIH	406
4	DAC	179	14	179	DAC
5	RUL	417	15	417	RUL
6	HOF	562	16	562	HOF
7	763	FIP	17	FIP	763
8	VOM	738	18	738	VOM
9	581	TER	19	TER	581
10	WAJ	269	20	269	WAJ

Instructions: The following instructions were read to the intentional learning-group:

> I am trying to develop a procedure for selecting clerical workers. I hope to be able to predict whether or not an individual will make a good filing clerk. This is what we are going to do. Some nonsense-syllables and some three-digit numbers will appear one by one in this slot in a mixed order. There will be 20 items altogether. As soon as you see what is in the slot, I wish you to write whatever it is on this sheet of paper three times, side by side, here, here, and here with this pencil. [A lined sheet of paper, 8½ by 13 in., was located on the table between *S* and the memory-drum. The pencil wrote blue at one end and red at the other.] You will write the next item right under this one, *here, here,* and *here,* and the next *here,* etc. Now, if the item is a nonsense-syllable, I wish you to write it in red with this end

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[6] J. G. Jenkins, Instruction as a factor in 'incidental' learning, this JOURNAL. 45, 1933, 471-477.

of the pencil. If the item is a number, I wish you to write it in blue with this other end of the pencil. [For half of the *S*s these color-item combinations were reversed.] This card will remind you if you forget. [The card, placed on the table beside the sheet of paper, had printed on it: "Number—Red; Syllable—Blue," or the reverse.] Also, while you are doing this, in addition, I wish you to try to learn as many of the items as you can, both the syllables and the numbers. There will be a test for the items when we have finished to see how many you have learned. The numbers and syllables will appear quite rapidly, one every few seconds, so you will have to work fast. If you fall behind, don't get upset. Just try to work faster and keep up. You will know when the next item appears by the loud click the machine will make. It will take a few seconds now before the first item appears.

As soon as the last item on the list disappeared from the slot, the drum was stopped, and the sheet of paper was removed. One minute later *S* was given a second sheet of paper and told that he was to do the same thing again. As soon as the second trial was over, the drum was stopped, the sheet of paper was removed, and *S* was given a third sheet of paper and an ordinary lead pencil and asked to write down as many of the items as he could recall, regardless of order. Two minutes were allowed for this recall test.

The procedure and instructions used with the incidental learning group were exactly the same except that the two sentences giving the instructions to try to learn the items and the information about the recall test were not read.

Results and discussion. The number of incidental learners and the number of intentional learners who correctly recalled each of the items on the recall were determined. An item had to be reproduced perfectly in order to be considered correct. The expected isolation-effects were not found. None of the *S*s, either incidental or intentional, recalled Item 3. As for Item 13, the other isolated item, 12 other less isolated items were recalled with greater frequency by the incidental learners and six other less isolated items were recalled with greater frequency by the intentional learners. It will be recalled that in one of the experiments in their study, Postman and Phillips also found that isolated items were not retained better than crowded ones by either incidental or intentional learners.[7] In their other experiment, however, they did find an effect of isolation. Isolated items were retained better than crowded ones, but only by the intentional learners, not by the incidental learners.

It appears that the present study and the one by Postman and Phillips, the only studies performed thus far which are concerned with the relationship between isolation and incidental learning, are in agreement in showing that under conditions of incidental learning the factor of isolation does not affect what is learned. There appears to be little agreement, however, as to how this factor of isolation affects intentional learning. In the present study, no effect was found. In the Postman and Phillips study, an effect was found in one situation but not in another. If we look at the other studies in this area, the picture remains confused. Whereas Siegel did find clear evidence for the superior learning of relatively isolated items as contrasted with relatively crowded items, Saul and Osgood did not.[8] In a more recent study, Green found an effect with the first isolated item, but not with the second.[9] Accordingly, the present state of our knowledge about the effect of isolation on intentional learning is quite equivocal.

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[7] Postman and Phillips, *op. cit.*, 48-56.

[8] Saul and Osgood, *op. cit.*, 372-379; Siegel, *op. cit.*, 311-316.

[9] R. T. Green, Surprise as a factor in the Von Restorff effect, *J. exp. Psychol.*, 52, 1956, 340-344.

The mean number of items recalled by the incidental learners in the present study was 2.03, by the intentional learners, 2.93. The difference between these means is significant ($t = 2.44$, $p = 0.02$). It is interesting to note that in spite of the fact that two trials were given (instead of one) and that the exposure-time was 6 (instead of 4) sec., these scores are of the same magnitude as those obtained by Postman and Phillips, 2.78 and 2.39 for incidental learning and 3.89 and 2.86 for intentional learning. One of their differences between incidental and intentional learning was significant, the other was not. It appears that the interference introduced in our experiment by the *orienting task* counteracted whatever advantage to learning that might have resulted from the increased contact by the *S*s with the stimulus-material.[10]

EXPERIMENT II

Since the recent studies comparing the learning of isolated and crowded materials are far from being definitive, the authors, whose primary interest is in investigating the variables which influence incidental learning, decided to compare incidental and intentional learning of isolated and crowded items, using a slightly modified version of Von Restorff's procedure, where no effort was made to control for absolute number. We were quite confident that with such a procedure we would find an effect of isolation on intentional learning; we wished to discover whether there would be an effect on incidental learning.

Method. The *S*s in Experiment II were 80 Indiana University undergraduates, randomly divided into four groups of 20 each, two incidental and two intentional learning-groups. The learning-material was a list of 14 two-digit numbers selected from a table of random numbers.[11] The list was presented to each *S* four times by means of an electrically operated drum at a rate of 6 sec. per number. A 10-sec. period intervened between successive presentations of the list. A different random order of the numbers was used on each of the four presentations.

Instructions.

The *S*s in the incidental learning groups were read the following instructions:

> We are interested in finding out the best way to code numbers. Some numbers will appear in this slot one by one. As soon as a number appears, we wish you to circle that number on a coding matrix. There are 14 numbers ranging from 11 to 99. We have four different kinds of matrices that we wish to try, so we will repeat the procedure four times, using a different matrix each time. There will be a short rest-period after each matrix is used. During the rest-period you will be able to inspect the next matrix to see how the numbers are set up.

The matrices were mimeographed sheets containing the numbers from 11 to 99, inclusive, arranged in sequence. On one matrix the numbers were arranged in 10 columns; on the second, in 9 columns; on the third, in 5 columns; and on the fourth, in 4 columns. The numbers were arranged horizontally on two of the matrices and vertically on the other two.

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[10] I. J. Saltzman, The orienting task in incidental and intentional learning, this JOURNAL, 66, 1953, 593-597; Comparisons of incidental and intentional learning with different orienting tasks, this JOURNAL, 69, 1956, 274-277.

[11] The list of numbers is the same one that has been used repeatedly by the senior author to study incidental learning (Edith Neimark and I. J. Saltzman, Intentional and incidental learning with different rates of stimulus presentation, this JOURNAL, 66, 1953, 618-621; Saltzman, *op. cit.,* 274-277; I. J. Saltzman and R. L. Atkinson, Comparisons of incidental and intentional learning after different numbers of stimulus presentations, this JOURNAL, 67, 1954, 521-524.)

The instructions read to the intentional learning-groups were essentially the same except that the *Ss* in these groups were told that the purpose of the experiment was to see how well they could learn the numbers. Like the incidental learners, they were required to perform the orienting task of circling the numbers on the matrices.

For one of the incidental and for one of the intentional learning-groups, 2 of the 14 numbers were circled in red. The remaining 12 numbers were uncircled. For the other two groups, all 14 of the numbers were circled in red. The two circled numbers, which we shall call the 'critical numbers,' were isolated items for the former two groups and comparison, crowded items for the latter two groups. The critical numbers were the same for all *Ss*. On the four trials, one of the critical numbers appeared ninth, fourteenth, seventh, and eleventh and the other one appeared thirteenth, fifth, fourteenth, and fourth.

TABLE II

FREQUENCY OF RECALL OF CRITICAL NUMBERS BY INTENTIONAL LEARNERS

Stimulus-conditions	Critical numbers recalled			Chi-square (df.=2)
	both	either	neither	
isolated	11	6	3	3.862
crowded	5	11	4	

TABLE III

FREQUENCY OF RECALL OF CRITICAL NUMBERS BY INCIDENTAL LEARNERS

Stimulus-conditions	Critical numbers recalled			Chi-square (df.=2)
	both	either	neither	
isolated	8	10	2	5.333
crowded	7	5	8	

Immediately following the fourth presentation of the list, all the *Ss* were given a recognition-test. The 14 numbers of the learning-series plus 42 additional two-digit numbers were randomly arranged in four columns on a mimeographed sheet. The *S* was given 2 min. in which to circle all the numbers he recognized as numbers that appeared in the drum.

Results and discussion. Tables II and III show the numbers of intentional and incidental learners, respectively, who recalled either both of the critical numbers, only one of the critical numbers, or neither of the critical numbers, when the critical numbers were isolated and when they were crowded. Chi-square tests comparing the two samples under each of the learning-conditions indicate that the differences are not statistically reliable. In this study, as in our first study, we found that items are not recalled better when they are isolated than when they are crowded. This is true whether the learning is incidental or intentional.

As was pointed out at the beginning of this paper, the results of our study were not expected. The explanation proposed by Postman and Phillips to account for their failure to find an effect of isolation may be applied to our findings.[12] They hypothesize that isolation will favor retention only to the extent that the stimulus-features producing isolation are relevant to the learner's task. Since, however, a technique for assessing the extent of such relevance is not as yet available, we are reluctant to adopt this as an explanation of our results. Rather than accept an *ad hoc* explanation, we prefer to call into question the whole relationship between isolation and ease of learning. The factors which determine when isolation will affect learning and when it will not should be ascertained.

To compare the total scores on the recognition-test, a correction for chance was applied to the data. Corrected scores were derived by subtracting from the number of correctly circled numbers the number that might be expected to be correct by chance. One of every four numbers circled was considered to be correct by chance. For example, if *S* circled 12 numbers, 9 of which were correctly circled, his corrected score would be 9 minus 3 $(12/4 = 3)$, or 6. The means of the scores derived in this fashion are shown in Table IV. An analysis of variance was performed on these data. The only *F* which was significant was the one for learning-conditions (0.01 level). Neither the *F* for stimulus-conditions nor the *F* for interaction was significant. These results are in agreement with previous findings by the senior author.[13] At a slow (6-sec.) rate of presentation, and after four trials, intentional learning is superior to incidental learning even though the orienting task, required of the intentional learners as well as the incidental learners, is one which interferes with the learners' efforts to learn.

TABLE IV

MEANS OF DERIVED RECOGNITION-SCORES

Stimulus-conditions	Learning-conditions	
	intentional	incidental
isolated	6.96	4.72
crowded	5.45	4.46

SUMMARY

Two experiments were performed to compare the incidental and intentional learning of isolated and crowded materials. In Experiment I, the learning-material was a 20-item list of 10 nonsense-syllables and 10 three-digit numbers. In Experiment II, the learning-material consisted of 14 two-digit numbers. In the first experiment, an isolated item was an item of one type embedded in a homogeneous sequence of items of the other type, *e.g.* a syllable preceded and followed by several numbers. The frequency of occurrence of the two types of items was the same. In the second experiment, 2 of the 14 members in the list were isolated by circling them in red. No significant effect of isolation was found on either incidental or intentional learning. Intentional learning, as was expected, was found to be superior to incidental learning in both experiments. A reëxamination of the relationship presumed to exist between isolation and ease of learning is suggested.

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[12] Postman and Phillips, *op. cit.*, 48-56.

[13] Neimark and Saltzman, op. cit., 618-621; Saltzman, *op. cit.*, 593-597, and 274-277; Saltzman and Atkinson, *op. cit.*, 521-524.

VISUAL DISCRIMINATION OF STRAIGHT AND CURVED LINES

By JOHN GAITO, Wilkes College

There is some discrepancy in the literature on visual perception as to the most readily perceived geometric form. Gestalt psychologists maintain that the circle is the most easily perceived, and this conclusion finds some empirical support,[1] but there are contradictory findings.[2] Of the studies cited in the literature, there are more which report the superiority of straight-line figures (triangle, rectangle, square) over curved line forms than the reverse.

It would appear that this problem might best be attacked on an elementary level, by determining whether a single straight line is more perceptible than a curved one. Then the number of lines could be increased gradually until closed figures were obtained. This procedure would allow a systematic analysis of form-perception with figures ranging from very simple ones to more complex ones. Yet it appears that there has been relatively little concern with simple forms such as straight and curved lines. Fry found that saw-tooth and wavy borders appear straight at low intensities of illumination.[3] Gibson reported that curved lines were more apt to reproduced as straight lines than the reverse.[4] These studies suggest that a straight line would be perceived at a lower threshold than a curved line.

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* Received for publication May 9, 1958. The research was done at the Air Crew Equipment Laboratory, Philadelphia, under Project TED NAM AE—7047, Human Engineering Investigations of Aircraft Cockpit Visual Displays; Part 18: Size and Shape of Numerals and Letters for Displays.

[1] J. E. Hochberg, Henry Gleitman, and P. D. McBride, Visual threshold as a function of simplicity of form, *Amer. Psychologist*, 3, 1948, 341-342 (abstract); M. E. Bitterman, John Krauskopf, and J. E. Hochberg, Threshold for visual form: A diffusion model, this JOURNAL, 67, 1954, 205-219; John Krauskopf, R. A. Duryea, and M. E. Bitterman, Threshold for visual form: Further experiments, this JOURNAL, 67, 1954, 427-440.

[2] R. C. Casperson, The visual discrimination of geometric forms, *J. exp. Psychol.*, 40, 1950, 668-681; Harry Helson and E. V. Fehrer, The role of form in perception, this JOURNAL, 44, 1932, 79-102; H. E. King, Carney Landis, and Joseph Zubin, Visual subliminal perception where a figure is obscured by the illumination of the ground, *J. exp. Psychol.*, 34, 1944, 60-69; Nathaniel Kleitman and Z. A. Blier, Color and form discrimination in the periphery of the retina, *Amer. J. Physiol.*, 85, 1928, 178-190; N. L. Munn and G. A. Geil, A note on peripheral form discrimination, *J. gen. Psychol.*, 5, 1931, 78-88.

[3] G. A. Fry, The relation of the configuration of a brightness contrast border to its visibility, *J. opt. Soc. Am.*, 37, 1947, 166-175.

[4] J. J. Gibson, The reproduction of visually perceived forms, *J. exp. Psychol.*, 12, 1929, 1-39.

Attneave's informational treatment of visual form-perception suggests that a straight line presents less information than a curved line and consequently is more readily perceived inasmuch as, in general, threshold increases with increasing information presented by a figure.[5] Accordingly, a combination of straight or curved lines would present more information and have a higher threshold than a single line. In Fig. 1, for example, an *x* is placed at the contour-points which convey information to *O*. The straight line (A) contains information at two points; all other aspects of the form are redundant. The curved line (B) presents information at the same points as the straight line, but has one peak of curvature which gives added information. The obtuse angle or 'triangular' form (C) has three points of information, while the 'box'-shaped figure (D) has four. In accordance with an informational analysis of these four forms, one would think that the four forms would fall into three populations: A in a population of low threshold, B and C in a population with moderate threshold, and D in the population with the highest threshold. The purpose of the present investigation was to test these predictions.

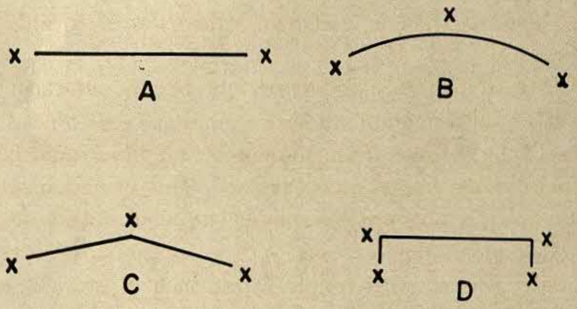

FIG. 1. THE FORMS USED IN THE TWO EXPERIMENTS
(Points of informational value are shown by *x*.)

EXPERIMENT I

Apparatus. An electronic tachistoscope (described in detail elsewhere[6]), with white matte surfaces on the interior, was employed at a brightness-level of 0.02 ft.-L. for both flash- and normal fields. The stimulus-material consisted of a straight and a curved line (A and B in Fig. 1), equal in stroke-width and perimeter, prepared by a professional draftsman. The straight line was 1/8 in. thick and 5/8 in. long. The drawings were photographed and the negative photographic print (white figure on a black background) was secured to a blank 5 × 8 in. index card. The stimulus-cards were so inserted into a holder in the rear of the tachistoscope with a 4 × 6-in. aperture that the stimuli as perceived by *O* were white figures surrounded by black, with the matte surface of the tachistoscope furnishing a white background for the stimulus-card itself. Brightness-measurements without filters were made with a Spectro Brightness Meter by two *O*s and then two Kodak neutral-density filters, 2.0 and 0.3, were used to bring the brightness-level to 0.02 ft.-L.

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[5] Fred Attneave, Some information aspects of visual perception, *Psychol. Rev.*, 61, 1954, 183-193.

[6] Rosalie Noble, Human engineering investigations of aircraft cockpit visual displays; Part 19: Design and development of an electronic tachistoscope, Air Crew Equipment Laboratory Report NAMC-ACEL-290, October 29, 1956.

Observers. A preliminary experiment with eight enlisted men at the Air Crew Eqiupment Laboratory had indicated no differences attributable to form or orientation, but, the experimental error was large because of the *naïveté* of the *O*s; the mean threshold for the curved line was greater on all trials and suggested that a form-effect was extant but obscured by a large experimental error. Three individuals who had, therefore, served previously as practice-*O*s and were familiar with the procedure were used in an effort to reduce the experimental error. The number of trials for each *O* was nine.

Procedure. O sat 3 in. in front of the tachistoscopic aperture, approximately 33 in. from the stimulus-card, and was presented the two forms in four orientations—horizontal, vertical, 45° left, 45° right—with order and sequence of these eight combinations arranged randomly for each *O*. An ascending series was used with the exposure-time increased at successive presentations until the *O* correctly identified the form. *O* was dark-adapted for 5 min. before presentation of the forms.

The visual angle subtended at the eye for the straight line was approximately 13′ vertically and 1°5′ horizontally. The visual angle for the curved line was approximately the same, since it had the same dimensions as the straight line but was curved into a slight arc. Experiment I thus was concerned with foveal vision.

Results. The results were treated by analysis of variance, with duration of exposure as the dependent variable. The only significant sources of variation were *O*s and forms. The mean threshold for the straight line was less than for the curved line for each of the three *O*s.

The temporal course of the perceptual process was studied by means of a content-analysis procedure in which misperceptions were recorded. If *O* reported a form incorrectly more than once prior to correct perception, only one of the responses was tallied unless two or more different forms were reported. These results indicated that the tendency to perceive a curved line as straight was much greater than the reverse tendency—71 as compared to 9.

EXPERIMENT II

Eight professional personnel of the Air Crew Equipment Laboratory who reported normal or corrected vision were used as *O*s in Experiment II.

Procedure. The physical arrangement was the same as in Experiment I, but two forms (both straight-line figures) were added, and the figures were presented only in one orientation. The new forms are C and D in Fig. 1. C represents a triangle and D a rectangle, each with the base deleted. Four of the *O*s were shown the forms without instruction as to what forms would be presented. Four other *O*s were shown diagrams of the stimuli to be presented and told to refer to A as 'straight,' B as 'curved,' C as 'triangular,' and D as 'box.' The *O*s had three blocks of trials in each of which eight forms, two of each kind, were randomized.

The forms in this experiment were prepared by a professional draftsman and photographed as black stimuli on a white background; all four were equal in stroke-width and perimeter. The stimuli in the tachistoscope appeared as black lines within a white rectangle (the outline of the card) against the white matte background of the tachistoscope. The straight line was 1/16 in. wide and 1½ in. long. The curved line had the same dimensions as the straight line but was bent into a slight arc, with the height of the arc ¼ in. The 'triangle' was composed of two ¾-in. lines meeting at the apex, which was ¼-in. high. The 'box' consisted of a 1-in. straight line with a ¼-in. line dropping vertically at each end. The visual angles subtended at the eye by these four forms ranged from approximately 6′ to 2°36′. This experiment was concerned with parafoveal vision.

Results. The data were treated by the analysis of variance in which the mean of six observations (duration of exposure) was the dependent variable. The only significant sources of variance were forms and *O*s. Duncan's Multiple Range Test was used to separate the means of the four forms.[7] The curved line represented an overlapping of the population containing the straight line and the one containing the other two forms. The content-analysis is given in Table I. The straight line was misperceived 3 times—always as a curved line; the curved line, 20 times—most often as a straight line; the 'triangular' figure, 51 times—mainly as a curved line; the 'box' form, 58 times—most often as a straight line.

TABLE I

FREQUENCY OF CORRECT AND INCORRECT REPORTS IN EXPERIMENT II

	Form				
Report	Straight	Curved	Triangular	Box	Total error
Straight	(48)	13	13	44	70
Curved	3	(48)	36	11	50
Triangular	0	6	(48)	3	9
Box	0	1	2	(48)	3
Total error	3	20	51	58	

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[7] D. B. Duncan, Multiple range and multiple F tests, Virginia Agricultural Experiment Station, Contract DA-36-034-ORD-1477 with Office of Ordnance Research, U. S. Army, June 1954.

DISCUSSION

The results were similar with the white-on-black and the black-on-white forms for the single straight and curved lines. Furthermore, there seem to be definite indications that the four forms fall into three populations with the straight line having the lowest threshold; the 'box' and 'triangular' forms having the largest; and the curved line intermediate. In Experiment I, the threshold was significantly lower for the straight line than for the curved. Furthermore, the perceptibility of the straight line was very evident upon analysis of the misperceived data. In Experiment II, Duncan's Multiple Range Test was unable clearly to separate the four forms; the straight line was significantly different from the 'box' and 'triangular' figures but not from the curved line. The curved line fell in an overlap-position between the two populations, but the content-analysis of the misperceptions seemed to indicate three separate populations.

It is interesting to note that the two multi-straight-line figures were misperceived many times as the more simple ones, curved or straight lines, while the reverse misperception was almost non-existent. This result appears to indicate a type of visual form-regression under experimentally impoverished conditions. With regard to the informational hypotheses, it appears justifiable to state that the straight line is the more perceptible figure in foveal and parafoveal vision. Other hypotheses were not, however, confirmed; the 'curved' line and 'triangular' form seem to fall into separate populations; the 'box' figure has a higher threshold than the curved line although not higher than the 'triangular' figure.

These results indicate that there are parameters involved in the development of form perception other than the amount of information in a figure. A major parameter appears to be the size or extent of critical detail contained in a figure. The smaller the size of the critical detail (*e.g.* the apex of the 'triangular' form and the legs of the 'box') and the closer the form approaches the limiting cases (*e.g.* with the 'triangular' form, horizontal and vertical straight lines) the more apt is it to be omitted in perception. This is essentially in agreement with the results of Bitterman, *et al.*[8]

Bitterman and his coworkers have presented an interesting diffusion model to explain visual form-perception. This model takes as a starting point the Köhler-Wallach theory of satiation and predicts that, with solid figures, loss by diffusion should be least in a circle, and that this form should not be altered because the pattern of diffusion from a circular region is circular. In other solid figures, furthermore, gaps should tend to close; corners, to round; and small details, to be deleted.[9] The results of this experiment with unclosed figures would appear to be in keeping with diffusion-predictions, especially the tendency for the multi-straight line forms to be perceived as the two simple forms, straight and curved lines. With regard to the results with the single straight and curved lines, diffusion predictions are not certain. One would guess, however, that the diffusion-processes around the edges of a one-dimensional figure would maintain the form of the figure and no obliteration of critical detail would occur, inasmuch as critical detail is minimal. Diffusion-processes around the edges of a two-dimensional figures would affect critical detail, *e.g.* the arc would tend to flatten in the center of the curve.

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[8] Bitterman, Krauskopf, and Hochberg, *op. cit.,* 215; Krauskopf, Duryea, and Bitterman, *op. cit.,* 438-440.

Pertinent to these data is a case of visual agnosia presumably resulting from occipital or parieto-occipital lesions reported by Adler.[10] This patient had great difficulty in recognizing figures composed of curves. After five years of observation, the patient still was unable to recognize patterns consisting of curves, but had improved remarkably with straight-line forms.

One result of the two experiments appears to contradict somewhat the statement of Hyman and Hake, and of Hake, that recognitive thresholds depend on the number of alternative forms and little generality is possible from study to study.[11] The first part of the statement certainly is true; however, the comparison between the straight and curved line in Experiment I, in which they were the only stimuli, and in Experiment II, in which two other forms were included, also gave the same result in threshold and misperceived data, indicating that some generality is possible. Yet the separation between the two stimuli was more clear in Experiment I than in Experiment II.

In conclusion, these results indicate the greater perceptibility of a straight line for both foveal and parafoveal vision. The curved line seems, however, to be more perceptible than the two and three straight-line forms. Yet it is not certain that a single curved line would be superior perceptually to a two-straight-line form if the obtuse angle was close to 90°, and to the three-straight-line figure if the 'legs' were longer than those included in

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[9] Bitterman, Krauskopf, and Hochberg, *op. cit.*, 207-217.

[10] Alexandra Adler, Course and outcome of visual agnosia, *J. nerv. ment. Dis.*, 111, 1950, 41-51.

[11] Ray Hyman and H. W. Hake, Form recognition as a function of the number of forms which can be presented for recognition, Wright Air Development Center Report 54-164, May 1954; Hake, Contributions of psychology to the study of pattern vision, Wright Air Development Center Technical Report 57-621, October 1957.

Experiment II. The two multi-straight-line forms used in this study were approaching the limiting configuration and this would appear to be more conducive for the occurrence of misperceptions. These studies should be continued with multiple straight and curved line forms to give more definitive answers.

SUMMARY

Two experiments with single and multiple straight and curved lines were conducted in an attempt to evaluate predictions of an informational approach to the problem of form-threshold. In Experiment I, the perception of a single straight line was compared to that of a single curved line. The latter had a significantly greater threshold and was misperceived as a straight line 71 times, while the reverse misperception occurred 9 times. In Experiment II, two multi-straight-line forms were added. The results indicated again that the straight line was more perceptible than the curved line, and that the two multi-straight-line forms tended to be perceived as the two simple forms. The predictions from the informational approach appeared adequate only if supplemented by consideration of extent of critical detail. Predictions from a diffusion theory appeared more adequate.

ON THE NON-CLASSICAL NATURE OF AVOIDANCE-BEHAVIOR

By J. D. KEEHN, American University of Beirut

If, as has been claimed,[1] avoidance-behavior is nothing more than antedating escape-behavior, both these kinds of activity would be essentially alike in any particular case. In a conventional avoidance-training experiment, it was shown that avoidance- and escape-responses could be quite dissimilar.[2] It is now clear, however, that avoidance-learning is but a special case of postponing behavior, and that the conventional avoidance-learning experiment has at least one important drawback.

Influenced by rigid Pavlovian procedures, experimenters usually have restricted their observations in studies of avoidance-behavior to signalled responses. Yet unsignalled, or interval-reactions were by no means rare.[3] Generally, steps were taken to minimize these responses, but when they were allowed to become instrumental by postponing the next trial, they tended to occur more frequently.[4] Moreover, even when no special efforts were made to reinforce interval-responses, they sometimes have been reported to have occurred with sufficient frequency to give the appearance of learning where none had actually taken place.[5]

Since there is no easy way of distinguishing such pseudo-learning, the data collected in most conventional experiments on avoidance-learning are suspect. The present experiment sets out to investigate the possibility of establishing incompatible escape and postponing responses, using the improved avoidance-training procedure.[6] That the conventional avoidance-response is but a special case of postponing responses can be seen by regarding the former as an interval-response (between shocks) which occurs in the presence of an external signal.

Apparatus. The appearance consisted of an ordinary activity-wheel, 14 in. in diameter and 5 in. wide, with a grid floor wired for shock. Timer, relay, and switching circuits, housed in sound-reducing boxes, were so arranged that an electric shock was delivered to the grid at 20-sec. intervals unless the wheel were revolved about

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* Received for publication February 17, 1958. This study was carried out while I was on leave as a Ford Fellow at the University of Illinois during 1956-57. I am grateful to the Ford Foundation for its support, and to Professor O. H. Mowrer for his encouragement and advice.

[1] B. R. Bugelski, *The Psychology of Learning,* 1956, 61-68.

[2] O. H. Mowrer and R. R. Lamoreaux, Fear as an intervening variable in avoidance conditioning, *J. comp. physiol. Psychol.,* 39, 1946, 29-50.

[3] M. A. May, Experimentally acquired drives, *J. exp. Psychol.,* 38, 1948, 66-77; Mowrer and Lamoreaux, Conditioning and conditionality (discrimination), *Psychol. Rev.,* 58, 1951, 196-212; H. W. Coppock, and O. H. Mowrer, Inter-trial responses as 'rehearsal': A study of 'overt thinking' in animals, this JOURNAL, 60, 1947, 608-616.

[4] May, *op. cit.,* 66-77; M. Sidman, Avoidance conditioning with brief shock and no exteroceptive 'warning signal,' *Science,* 118, 1953, 157-158.

[5] L. J. Kamin, Traumatic avoidance learning: The effects of CS-US interval with a trace conditioning procedure, *J. comp. physiol. Psychol.,* 47, 1954, 65-72.

[6] Sidman, *op. cit.,* 157-158.

1 in. in the meantime. In that case, the timer reset and another 20-sec. period of grace began. Although shock came on, or was postponed, automatically, it was terminated by *E* contingent upon the behavior of *S*.

Connected to the activity-wheel by means of a pulley was a recording drum across which a pen was driven at constant speed. Movement of the activity-wheel turned the drum relative to the pen, thus producing a cumulative index of response (running).

Shocks, recorded as lateral spurs on the graphs, are not always clearly shown, for if *S* runs with the shock on, the graph remains displaced slightly to the right. When the escape-response terminates the shock, the graph is then displaced back to the left.

Procedure. Two albino rats, aged 120-140 days, were trained individually in the activity-wheel which was free to revolve in either direction.[7] Training consisted in giving *S* electric shocks (175-v., 250,000-Ω limiting resistor) through the grid floor of the activity-wheel at 20-sec. intervals unless it behaved as described below.

To escape from shock, *S* had to *turn about* in the cage and face in the opposite direction. If it happened to *run* after turning and terminating a shock, the next shock was postponed until 20 sec. after running had ceased. If the shock came on, and *S* ran *before* turning around, nothing was accomplished; the shock remained on until the animal terminated it by facing about. Running, therefore, never served to escape from shock; only facing about did that. On the other hand, running did result in the postponement of shock, and a short run every 20 sec. was enough to avoid shock altogether.

The experiment was carried out in a dimly lighted room. No special precautions were taken to control the illumination, except that a mirror was so placed at one end of the wheel that *S*'s visual fields would be more or less similar whichever way it was facing.

Each *S* spent three hourly adaptation-periods and four half-hourly training-periods in the wheel. On the adaptation-trials, *S* was left in the apparatus and records of running-activity taken. On the training-trials, the procedure described above was utilized. Sessions occurred once daily for seven successive days.

Controls. That running by rats in a situation such as this is not a function of shock *per se* is shown in a study in which shocks were given independently of *S*'s behavior.[8] Running, in that case, quickly extinguished as interval-behavior.

Results. As both *S*s performed in similar fashion, results for only one of them are reported. A record of most of the running-activity of this animal is given in Fig. 1. Line A3 of Fig. 1 was recorded on the third adaptation-day. Virtually no running occurred on this occasion, *S* lying quietly in the cage for the most part. Running on the first training-day is shown by Curve T1. On receiving the first shock, *S* ran forward a short way (turning the wheel counterclockwise) as is shown by the descending curve. It then faced about, the shock went off, but *S* went on to run, turning the wheel clockwise. On the second shock, *S* faced about more promptly to terminate the shock, but then turned around again and ran in the direction it was facing prior to being shocked. Similar behavior occurred for 11 more shocks before *S* made one turn about and then ran in the new (counter-clockwise) direction.

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[7] As the study is really a demonstration, a single animal would have been sufficient. The second animal essentially replicates the demonstration. Since both animals demonstrated the phenomenon under investigation so quickly and so dramatically, further replication was deemed unnecessary.

[8] O. H. Mowrer and J. D. Keehn, How are inter-trial 'avoidance' responses reinforced?, *Psychol. Rev.*, 65, 1958, 209-221.

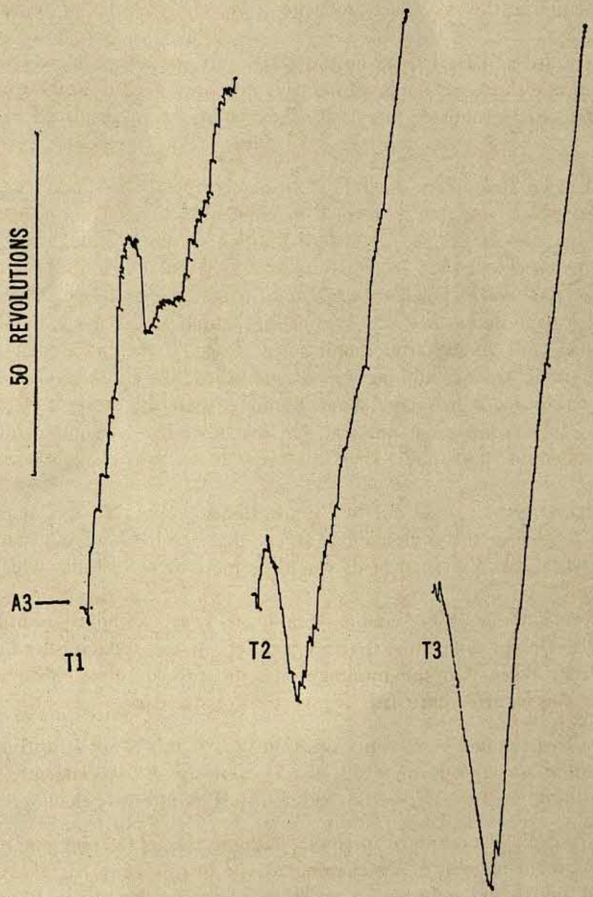

FIG. 1. CUMULATIVE RUNNING CURVES FOR AN *S* IN FOUR DAILY SESSIONS
Line A3 represents activity during the last portion of the third adaptation-hour; curves T1, T2, and T3 show running in the first three half-hour training sessions. Lateral spurs to the right indicate shocks.

On receiving the next shock, *S* faced about again but ran only a little. Twenty-two more shocks were received by *S* during this session, all of which were terminated by facing about. At this stage very little interval-running took place. Instead, *S* tended to turn round once in each interval to face again in the before-shock direction. It is for this reason that the response-curve shows a continuous rise rather than a saw-toothed effect.

Session T2 is similar except that there is more running. The number of shocks received dropped from 36 to 12. *S* continued to run after a single turnabout to shock on seven occasions, but most running was such as to turn the wheel clockwise. Interval-turnabouts still were frequent, as is evidenced most clearly by the behavior to the last three shocks received. On each occasion *S* paused for 20 sec., was shocked, terminated the shock by turning around, then promptly turned around again and began running.

In session T3, the number of shocks received fell to three and the escape-from-shock responses were more prompt, giving a clearer record. *S* began by making a short clockwise run (the initial rise in the curve). It then turned around spontaneously and ran in the other direction. After a 20-sec. pause, it was shocked. This caused *S* to turn about and run a little before again turning spontaneously to run in short bursts counterclockwise. After about 10 min., it again paused long enough to be shocked, whereupon it faced about and began to run in the clockwise direction. Finally, there were two more spontaneous (interval-) about-faces and enough interval-runs to postpone shocks for the remainder of the session. It should be pointed out that apart from the non-functional facing about intervals that can be deduced from the record, both *Ss* exhibited a marked tendency to make surrogate turnabouts, or 'looks,' during many pauses between runs.

The record for *T4* is not shown. Again three shocks were given *S*, each being followed by prompt facing-about and running. By now, turning around in intervals was rare, and *S*'s initial preference to run in the clockwise direction no longer was apparent.

Discussion. The outcome of the present study clearly demonstrates that postponing responses need not be similar to escape-responses and, indeed, may even be incompatible with them. The postponing or avoidance-response used is quite unlike the escape-response and hence cannot be interpreted in terms of antedating.[9] In fact, such an interpretation has no meaning in the context of the present experiment.

If postponing behavior is not classically conditioned, we must seek its source of reinforcement. There is no difficulty with the escape-response; it terminates shock. Two alternative possibilities for the explanation of the postponing responses are available from the literature. First, there is the extension of Mowrer's view that postponing responses are reinforced by escape from fear.[10] As there is no external stimulus in this case that could mediate the induction and reduction of fear, we must refer instead to interoceptive stimulation.[11] Schoenfeld regards avoidance-behavior as the 'residue' after all other behavior has been depressed through pairing with shock.[illegible] As an additional source of reinforcement, Schoenfeld has pointed to the secondary reinforcement accorded to the 'avoidance'-response by virtue of its elimination of the internal stimuli left by other (punished) behavior. The first part of Schoenfeld's hypothesis has been supported by an experiment showing that, given two responses, one of which postpones shock for a longer time than the other, animals will come to utilize the response producing the longer delay of shock.[illegible]

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[9] B. R. Bugelski and R. A. Coyer, Temporal conditioning vs. anxiety reduction in avoidance learning. *Amer. Psychologist,* 5, 1950, 264-265 (Abstract). Bugelski, *op. cit.,* 1956.

[10] Mowrer and Lamoreaux, *op. cit.,* 29-50.

[11] Mowrer and Keehn, *op. cit.,* 215-218.

The second part of Schoenfeld's hypothesis has much in common with Mowrer's view. Both assume that stimuli which precede electric shock take on aversive properties, and that the elimination of such stimuli is rewarding, although Schoenfeld does not talk about 'fear' as Mowrer does. They differ in that Mowrer sees the avoidance-response as being evoked by fear conditioned to the absence of its own 'trace,' whereas Schoenfeld regards it as reinforced through the termination of the 'traces' of other responses. The present experiment offers no sure way of deciding between these two viewpoints, although it does serve effectively to eliminate a third (Pavlovian) interpretation of postponing behavior from the field.

Summary. Interpretations of avoidance-behavior in terms of classical conditioning are still common despite the demonstration some years ago that avoidance- and escape-responses can be quite dissimilar. In the present experiment, the problem of interval-response was eliminated by the use of 'postponing' behavior. It was possible to train animals to react differently to escape and to postpone shock. The interpretation of postponing behavior as antedating escape-behavior therefore, is inadequate.

[illegible] W. N. Schoenfeld, An experimental approach to anxiety, escape and avoidance behavior, In P. H. Hoch and J. Zubin (eds.), *Anxiety*, 1950,

[illegible] Sidman, Delayed punishment effects mediated by competing behavior, *J. comp. physiol. Psychol.*, 47, 1954, 145-147.

INTEROCULAR TRANSFER IN THE DISAPPEARANCE OF STABILIZED IMAGES

By JOHN KRAUSKOPF and LORRIN A. RIGGS, Brown University

When a target is imaged in such a way that it remains fixed in position on the retina, it is clearly visible at first, but it disappears with prolonged viewing. Riggs and his associates in this country, and Ditchburn and his coworkers in England, have independently developed techniques by which an image can be stabilized, *i.e.* made to remain motionless on the retina in spite of eye-movements.[1] The present experiments were designed to shed light on the mechanisms underlying the disappearance of stabilized images.

The disappearance of stabilized images might be accounted for on the assumption that, under constant stimulation, a photostationary state is set up in the receptors, and, consequently, that the neural activity due to the stimulus subsides with prolonged viewing.[2] Evidence from psychophysical experiments on interocular transfer which has been available for more than 35 yr. suggests, however, that retinal factors are not sufficient to account for visual sensitivity. Dunlap demonstrated in 1921 that a dim spot of light presented peripherally to one eye disappeared after a short viewing-time.[3] If that same spot was then presented to the corresponding area of the other retina, it was not seen, although it would have been without previous stimulation of the first eye. Later, Guilford conducted more extensive experiments on this interocular-transfer phenomenon with similar results.[4] Experiments in which the course of dark-adaptation in one eye has been measured with various levels of light-adaptation in the other eye have produced mixed results. Wolf and Zigler found interocular effects, but Mitchell and Liaudansky were unable to measure any.[5]

The purpose of the present study was to determine whether the disappearance of

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* Received for publication June 2, 1958.

[1] R. W. Ditchburn and B. L. Ginsborg, Vision with a stabilized retinal image, *Nature,* 170, 1952, 36-38; L. A. Riggs, F. R. Ratliff, J. C. Cornsweet, and T. N. Cornsweet, The disappearance of steadily fixated test-objects, *J. opt. Soc. Amer.,* 43, 1953, 495-501. See also, A. L. Iarbus, A new method of studying the activity of various parts of the retina, *Biophysics* (Biofizika), 2, 1957, 165-167. Although rather complicated apparatus is required for detailed experiments on stabilized images, one can confirm the main findings with simple equipment (F. R. Ratliff, Stationary retinal image requiring no attachments to the eye, *J. opt. Soc. Amer.,* 48, 1958, 274-275). Further evidence is found in the well-known fact that, though we do not ordinarily 'see' the shadows of our retinal blood-vessels, we can become aware of them if we cause them to vary by flooding the eye with diffuse flickering light, or with light reaching the retina from various parts of the pupil.

[2] Riggs, Ratliff, Cornsweet, and Cornsweet, *op. cit.,* 500.

[3] Knight Dunlap, Light spot adaptation, *Amer. J. Physiol.,* 55, 1921, 201-211.

[4] J. P. Guilford, 'Fluctuations of attention' with weak stimuli, this JOURNAL, 38, 1927, 534-583.

[5] Ernst Wolf and M. J. Zigler, Course of dark adaptation under various conditions of pre-exposure and testing, *J. opt. Soc. Amer.,* 45, 1955, 696-702; R. T. Mitchell and L. H. Liaudansky, Effect of differential adaptation of the eyes upon threshold sensitivity, *ibid.,* 45, 1955, 831-834.

stabilized images could be accounted for on a purely monocular basis. The results of Dunlap and Guilford in the peripheral retina may not apply to the fovea, but they suggest an appropriate line of experimentation. The specific question asked was whether interocular transfer would occur such that a stimulus-patch presented to the fovea of one eye would disappear more readily following presentation of the same object to the corresponding region of the other eye.

Method. The technique for producing stabilized images is illustrated schematically in Fig. 1, taken from an earlier paper by Riggs and his coworkers.[6] Light from a projector forms an image in front of the eye after being reflected by a mirror embedded in a contact lens worn by *O*. The image is viewed by *O* through an indirect path twice as long as the path from the mirror of the lens to the image. The image moves in direct correspondence with the movements of the eye, but with double the angular movement of the eye. The doubled return-path compensates for the angular doubling, and the image ultimately formed on the retina remains fixed relative to the retina. The optics actually used differed from those schematized here in several important respects. There were two such systems, one for each eye. Fixation-objects, which were not stabilized, were presented by means of beam-splitters located close to the eyes in the return paths. Both the stabilized targets and non-stabilized fixation-fields were presented stereoscopically. *O* was supplied with controls which allowed him to adjust the positions of the images on his retinas.

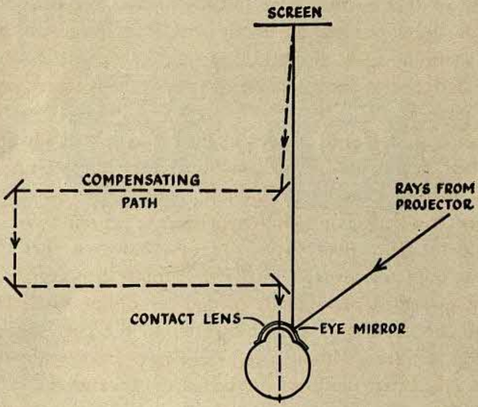

FIG. 1. DIAGRAM ILLUSTRATING PRINCIPLE FOR PRODUCING STABILIZED RETINAL IMAGES

The stimulus-objects are illustrated in Figs. 2 and 3. In Fig. 2, they are shown as they appeared to *O*. Consider first the left configuration. The annulus and the central dot (actually a bright fixation-point) were present at all times. The disk with the bar superimposed on it was presented to *O* as a stabilized image, and its position was so adjusted that it would appear in the center of the annulus. The separate stimuli are presented in Fig. 3. At the bottom are shown the stabilized components and at the top the normally viewed fixation-annulus. (which was presented stereoscopically to both eyes). The stimuli presented for the first 30 sec. of a 1-min. viewing period on experimental trials are on the second line. The stimuli which were presented during the first 30 sec. on control trials are on the third line. The stimuli which were presented during the second 30 sec. of all trials are on the fourth line. The diameter of each disk subtended a visual angle of 1° and the brightness was 8 ft.-L. The bars were 24′ high and 6′ wide. The inner edges of the bars were 6′ from the center of the disk. In one experiment, the bars were darker than the disks, the contrast being 0.77/1, and in another experiment the bars were lighter than the disks, the contrast being 1.7/1. The inner diameter of the annulus was 1° 42′ and the outer diameter was 2° 6′. There was also a small bright spot at the center of the annulus. The brightness of the annulus was the same as that of the disk; namely, 8 ft.-L. The composite appearance of the stimuli is illustrated in Fig. 2. On the left the stimuli are shown as they appeared to *O* in the first 30 sec. of the experimental trials, and in the last 30 sec. of all trials. On the right they are shown as they appeared during the first 30 sec. of the control trials. Thus, the conditioning and test-stimuli fell on non-corresponding parts of the two retinae on the control trials and on corresponding parts of the two retinae in the experimental trials. It should be emphasized that the test-stimuli were the same on experimental and control trials.

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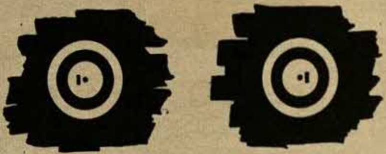
[6] Riggs, Ratliff, Cornsweet, and Cornsweet, *op. cit.*, 497.


EXPERIMENTAL CONDITIONING STIMULI & TEST STIMULI

CONTROL CONDITIONING STIMULI

FIG. 2. COMPOSITE APPEARANCE OF THE STABILIZED TARGET-FIELD CENTERED WITHIN THE FIXATION-ANNULUS

An ABBA-order was used in the presentation of experimental and control trials with the view to treating the data by the Wilcoxon paired-replicates test. This procedure seemed advisable, since not all the data could be gathered within a single session and since there might be temporal variations in the amount of disappearance. In each experiment, 20 trials were given, but in two cases records were not usable due to technical difficulties.

Results. During the test-period, *O* signaled with a key to indicate when he could see the bar. Depression of the key activated a galvanometer, deflecting the trace on a moving film. The developed film thus showed the time-course of *O*'s report of seeing during the 30-sec. test-period. It might be expected that transfer-effects would be confined to the first few seconds of the test-period. The records were therefore first analyzed for such temporal inhomogeneities. As no such patterns were found, it was decided to use the total time that the bar was seen during the test-period as the dependent variable. Results of four experiments are summarized in Table I. The mean times in seconds that the bars were reported seen during the control and experimental test-periods and the mean differences in time seen are shown for two *O*s for the cases of the dark and light bars. The differences are not large, but all are in the direction of an interocular transfer of the disappearance-effect, and all are significant beyond the 5% level of confidence. This pattern of small but significant differences also was found in similar preliminary experiments. The effect is of the order of a 5–10% difference in the total seeing time.

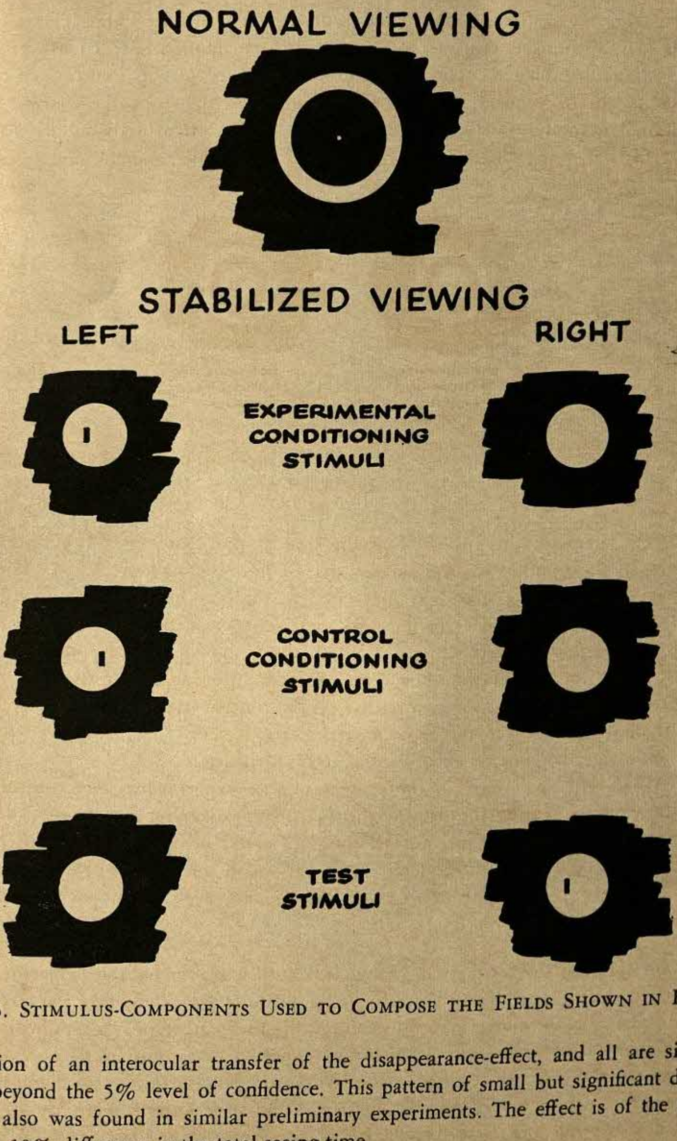

NORMAL VIEWING
STABILIZED VIEWING
LEFT
RIGHT
EXPERIMENTAL CONDITIONING STIMULI
CONTROL CONDITIONING STIMULI
TEST STIMULI

FIG. 3. STIMULUS-COMPONENTS USED TO COMPOSE THE FIELDS SHOWN IN FIG. 2

Discussion. Since the experimental results obtained with dark bars and light bars were similar, and since there is no theoretical reason to think that different processes are involved, it is probably appropriate to interpret the results of the four experiments together. The differences obtained in the separate experiments were significant at the 5% level of confidence but, because of their replication we can place greater confidence in the reliability of the interocular transfer-effect. Specifically, the results indicate that the disappearance of a stabilized image presented to one eye is significantly enhanced following stimulation to the corresponding region of the other eye.

Conceivably, these results might be accounted for in terms of the addition of the negative after-image from the first eye to the stimulus presented to the second eye. We do not think that such an explanation is plausible. Using the same techniques of stimulation and recording, we attempted to measure the duration of the negative after-image when the second stimulus was replaced with a blank field. In these experiments, *O* never reported seeing the after-image after the first 3 sec. of the 30-sec. test-period. The after-image hypothesis would seem to lead to the prediction that the differences between the experimental and control conditions would last for only the first few seconds, but, as mentioned earlier, the obtained differences were sustained for the whole 30-sec. period.

TABLE I

MEAN VISIBILITY-TIMES (SEC.) FOR BARS DURING TEST-PERIOD

O	Bars	Control	Experimental	Diff.*
LAR	dark	19.3	17.7	1.6
	light	20.4	18.1	2.3
JK	dark	20.3	18.3	2.0
	light	23.7	22.3	1.4

* All differences significant beyond the 5% level of confidence by Wilcoxon's test for paired replicates.

Several neural mechanisms might account for our results. The current work on the centrifugal control of peripheral activity suggests the dim outline of one interpretation.[7] Perhaps the most attractive hypothesis is that, in addition to retinal photochemical adaptation-processes, there is some degree of neural adaptation. To fit the present results, the neural adaptation would have to occur at or beyond the level at which the impulses from the two eyes interact. Our data do not enable us to choose among these possible interpretations.

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[7] Ragnar Granit, *Receptors and Sensory Perception,* 1955, 81-112.

LEARNING-SETS WITH ONE- AND TWELVE-TRIAL ODDITY-PROBLEMS

By MARVIN LEVINE and HARRY F. HARLOW, University of Wisconsin

A review of the literature on object-discrimination has suggested to Harlow that the formation of a learning-set is a function of the number of trials per problem (*tr/p*).[1] He hypothesizes that the transfer between problems varies with the amount of learning within problems. A large number of *tr/p*, he predicts, should be advantageous early in learning but wasteful later on, while a small number of *tr/p* should make for difficulty early in learning but for greater efficiency later on. In the present experiment, the role of *tr/p* in the development of oddity-learning was investigated. Two groups of seven monkeys each were trained on a series of oddity-problems. One group was given 12 *tr/p*; the other group was given only one *tr/p*. The Moon-Harlow 'two-position oddity' problem (odd object never in the middle position) was used.[2]

Method: Subjects. Fourteen female rhesus monkeys were randomly assigned to the two groups. Prior to this investigation, these animals had reached near-asymptotic performance in the course of approximately 5000 trials in an experiment on the discrimination of objects.

Apparatus. All *S*s were run in the Wisconsin General Test Apparatus, which previously has been described in detail.[3] The only special feature required by the oddity-task was a three-hole test-tray with a transparent plastic screen, 6 in. wide, fixed about 3 in. in front of the center-hole. The screen made response to the middle object impossible, but had no apparent disturbing effect on any of the animals.

The stimuli were multidimensional objects selected from the laboratory collection of 1000 different pairs. The pairs were randomly grouped into sets of two, each such set comprising the stimulus-objects for one problem.

Procedure. All *S*s were given 36 trials per day. The 12 *tr/p* group, therefore, had 3 problems per day. For the 1 *tr/p* group, 36 sets of objects were required each day, and, when all the objects in the collection had been used, the experiment was continued with the same pairs randomly regrouped in sets of two. Such re-pairing was necessary approximately every 13 days.

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* Received for publication July 20, 1958. This work was supported in part by the Research Committee of the Graduate School with funds supplied by the Wisconsin Alumni Research Foundation, and in part by a grant from the Ford Foundation.

[1] H. F. Harlow, Learning set and error factor theory, in Sigmund Koch (ed.), *Psychology: A Study of Science,* Vol. II.

[2] L. E. Moon and H. F. Harlow, Analysis of oddity learning by rhesus monkeys, *J. compar. physiol. Psychol.,* 48, 1955, 188-194.

[3] Harlow, The formation of learning sets, *Psychol. Rev.,* 56, 1949, 51-65.

All trials of the experiment were conducted as follows: With the forward opaque screen lowered, the appropriate left or right foodwell was baited with a raisin or a peanut. The odd object was placed over the baited foodwell, one non-odd object was placed behind the center plastic screen, and the other over the empty foodwell. The one-way-vision screen then was lowered, the opaque screen raised, and the tray moved forward within the *S*'s reach. After *S* displaced one of the objects and (if the response was correct) took the reward, the tray was withdrawn. The forward screen was lowered, the one-way-vision screen raised, and a record was made of the response. The noncorrection procedure was used throughout.

The odd object was placed on the left or right on any trial according to standard Gellermann-sequences.[4] Six different sets of such sequences were employed, one each day, with all six used per week. It was necessary also to stipulate which of the two object-pairs would be odd on any one trial. This was accomplished by labeling each pair of the set as *A* or *B* and then developing six sets of random *AB*-sequences of 36-item length. These sequences were so constructed as to contain the Gellermann sequence-restrictions and were paired with the position-sequences. There was then a cycle of six position-sequences and six object-sequences which was repeated each week. The experiment lasted for 14 weeks, each *S* receiving 3024 trials.

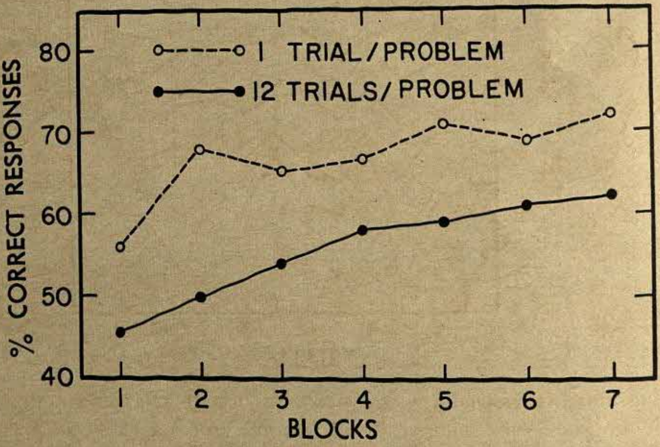

FIG. 1. LEARNING-SET FUNCTIONS FOR THE 1-TRIAL AND 12-TRIAL GROUPS (Each point is the mean percentage correct for 7 *S*s during a block of 432 trials.)

Results. Fig. 1 shows that the learning-curves for the two groups have much the same slope, and that the performance of the 1 *tr/p* group is consistently superior to that of the 12 *tr/p* group. Evaluated by Alexander's test for trend, the difference in slope did not reach significance at the 5% level, but the difference in level of performance was significant at the 1% level.[5] One other detail to be noted in Fig. 1 is that the first point for the 12 *tr/p* group is below chance (46%), whereas the corresponding point for the 1 *tr/p* group is above chance (56.5%). Evaluated by *t*-tests, each point deviated significantly from 50%.

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[4] L. W. Gellermann, Chance orders of alternating stimuli in visual discrimination experiments, *J. genet. Psychol.*, 42, 1933, 207-208.

The within-problem learning-curves for the 12 *tr/p* group are presented in Fig. 2 as solid lines. The curve for each successive block of problems is displaced upward for convenience of inspection. The most important characteristic of these curves is that no within-problem learning appears for any block of problems. The only within-problem variation is the marked trial-to-trial fluctuation which occurs in the early blocks. It will be shown below that these fluctuations are attributable to systematic rather than to random factors.

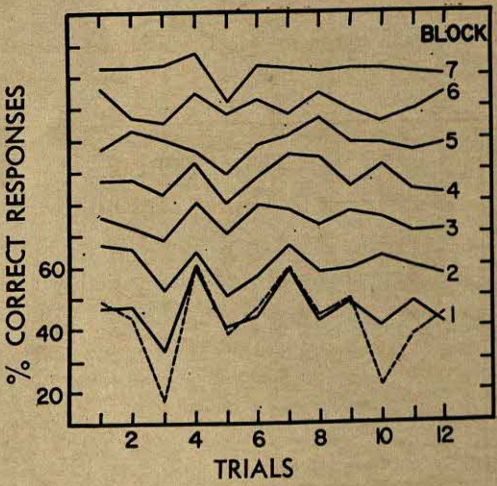

FIG. 2. INTRAPROBLEM PERFORMANCE OF THE 12-TRIAL GROUP
(The solid lines show intraproblem performance for each block of 36 problems. The points for Block 1 are plotted in the usual manner, relative to the indicated vertical scale. The curve for each succeeding block is displaced vertically. The curve for Block 2 has been raised 10 percentage-points, that for Block 3 has been raised 20 points, and so on, through Block 7, which has been raised 60 percentage-points. The dashed line is explained in the text.)

Discussion. The similarity of learning-rate between the two groups does not fit Harlow's hypothesis. An obvious basis for this discrepancy is that the results which suggested the hypothesis were obtained in experiments on learning to discriminate objects, while the present results were obtained in an experiment on oddity-learning. In both cases, one problem lasts for as long as one set of objects is present, and changing the objects introduces a new problem. The change made in the stimulus-structure of the experiment as one problem succeeds another, however, is of a different order in the two types of training. In object-discrimination, the object-qualities within any single problem are relevant. One of the objects always is rewarded, while the other object never is rewarded. When the problem is changed, a new object-food relation must be learned, and one may speculate, as Harlow does, that the amount of transfer between problems depends on the amount of learning within problems. In oddity, however, object-quality is irrelevant. No specific object-food relation exists. To improve, *S* must respond in terms of oddity, which accounts for the lack of learning within problems by the 12 *tr/p* group; response to oddity is learned so gradually that no improvement is manifested over sets of twelve adjacent trials. If it is assumed, furthermore, that change in the irrelevant dimension of object-quality does not interfere with learning about oddity, then the frequency of these changes, *i.e.* the number of *tr/p*, should have no effect on rate of learning. One might, in fact, expect no difference at all between the two groups of this experiment.

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[5] H. W. Alexander, A general test for trend, *Psychol. Bull.*, 43, 1946, 533-557.

How then is the inferiority of the 12 *tr/p* group to be understood? It may be demonstrated that this inferiority stems from the response-tendencies of sophisticated *Ss* rather than from the experimental variable *per se*. Moon and Harlow have shown that prior experience with object-discrimination generates a special response-pattern which is transferred to the oddity-task.[6] This pattern, which will be referred to as *object-discrimination responding (ODR)*, is defined as follows. When the response on Trial *n* is to the rewarded object, the response on Trial $n + 1$ will be to the same object whether or not it is odd; if the response on Trial *n* is to the nonrewarded object, the response on Trial $n + 1$ will be to the other object whether or not it is odd. If it is assumed that *S* responds in accordance with this rule on every trial, and if the response on the first trial is known, then the response-outcomes on every trial of the problem can be predicted from the object-sequences. Suppose, for example, that Object *A* is odd on the first trial of a problem, *B* is odd on the second trial, and so on, with the following odd-object sequence for a 12-trial problem: *A, B, B, A, A, A, B, A, B, B, B, A*. Whether the response of *S* on the first trial is correct or not, the definition of *ODR* predicts the following sequence of successes and failures on trials 2 through 12: — + — + + — — — + + —. The hypothetical response-outcomes for all six object-sequences obtained in this manner permit the calculation of an hypothetical within-problem learning curve, shown by the dashed line in Fig. 2. The similarity between this curve and the empirical curve for the first 12-day block indicates both the strength of the *ODR*- pattern and its role in determining most of the fluctuations in the early blocks of problems.

The over-all percentage of correct response in a rationally generated *ODR*-sequence is 41.2%. This value is significantly different from 50%, and suggests that the Gellermann-sequences, which were designed to counterbalance outcomes for certain other systematic patterns of response, failed to counterbalance outcomes for the *ODR*-pattern. If these sequences are used when *ODR* is strong, performance will be depressed, sometimes below the chance-level, as in the case of the 12 *tr/p* group on Block 1. It is reasonable to hypothesize that the combination of Gellermann-sequences and *ODR* suppressed the performance of the 12 *tr/p* group throughout the experiment. A confirmation of this hypothesis was obtained at the end of the experiment by shifting the 12 *tr/p* group to the 1 *tr/p* condition. Since it is necessary to have two trials with the same pair of objects before *ODR* can be manifested, the change should produce an immediate improvement in performance, which, as Fig. 3 shows, is exactly what happened.

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[6] Moon and Harlow, *op. cit.*, 188-194.

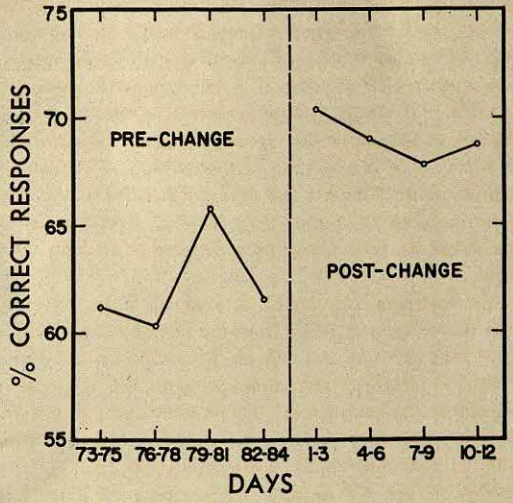

FIG. 3. PERFORMANCE BY THE 12-TRIAL GROUP ON THE LAST 12 DAYS OF THE EXPERIMENT PROPER (PRE-CHANGE) AND ON THE FIRST 12 DAYS OF A SUBSEQUENT SERIES OF 1-TRIAL PROBLEMS (POST-CHANGE)

Conclusion. It may be concluded that the difference between the two groups of the present experiment would have been reduced or entirely eliminated had unbiased object-sequences been employed or had naïve *S*s been used. If this conclusion is accepted, Harlow's hypothesis clearly is inapplicable to oddity-learning. This learning appears to depend only on the number of trials given, irrespective of how they are organized into problems.

A COMPARISON OF THE DECISION-BEHAVIOR OF RATS AND OF HUMAN SUBJECTS

By JOHN E. OVERALL and W. LYNN BROWN, University of Texas

A 'decision' can be defined operationally as the choosing of one of alternative courses of action. Thus, the behavior of a wide variety of organisms while making choices can be described as decision-making. This experiment was undertaken to compare the decisions of rats and human *Ss* in response to identical sequences of stimulus-events.

Applied almost exclusively to human behavior, decision-models that have developed from game-theory assume that the *S* is rational and that the response he selects maximizes utility.[1] *S* is assumed to weigh the various alternatives and, other things being equal, to choose that course of action which has the greatest probability of leading to the desired outcome. The present writers have demonstrated that a modified theory can be employed to predict the behavior of rats in a simple two-alternative situation.[2]

It is recognized that, in many situations, the decision of neither men nor rats conforms to the 'rational' model. Brunswik demonstrated that probability of response approaches the probability of reward for rats in a simple two-alternative situation in which reward is sometimes on the right and sometimes on the left.[3] Humphreys, Grant, Hake, and Hornseth, and many others have demonstrated a similar matching of probability in the behavior of human *Ss*.[4] Whereas models of decision-theory have great difficulty in rationalizing this apparently irrational behavior, the recent stochastic models for learning have been developed to account for probability-matching.

The fact that probabilities of response tend to approach the probabilities of 'pay-off' for both man and lower animals has become a central feature in the models developed by Estes, Estes and Burke, and by Bush and Mosteller.[5] These investigators employ in common a linear operator in constructing their mathematical models. The probability of response on any trial $n + 1$ is a function of the proba-

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* Received for publication May 16, 1958. This study was made possible by a grant-in-aid from the Research Institute of the University of Texas.

[1] I. D. J. Bross, *Design for Decision,* 1953, 1-276. Ward Edwards, The theory of decision making, *Psychol. Bull.,* 51, 1954, 380-417; J. von Neumann and O. Morgenstern, *Theory of Games and Economic Behavior,* 1944, 1-625.

[2] J. E. Overall and W. L. Brown, Recency, frequency, and probability in response prediction, *Psychol. Rev.,* 64, 1957, 314-322.

[3] Egon Brunswik, Probability as a determiner of rat behavior, *J. exp. Psychol.,* 25, 1939, 175-197.

[4] L. G. Humphreys, Acquisition and extinction of verbal expectations in a situation analogous to conditioning, *ibid.,* 25, 1939, 294-301; D. A. Grant, H. W. Hake, and J. P. Hornseth, Acquisition and extinction of a verbal conditioned response with differing percentages of reinforcement, *ibid.,* 42, 1951, 1-5; M. E. Jarvik, Probability learning and a negative recency effect in the serial anticipation of alternative symbols, *ibid.,* 41, 1951, 291-297; H. W. Hake and Ray Hyman, Perception of the statistical structure of a random series of binary symbols, *ibid.,* 45, 1953, 64-74; J. J. Goodnow and Leo Postman, Probability learning in a problem-solving situation, *ibid.,* 49, 1955, 16-22.

[5] W. K. Estes, Toward a statistical theory of learning. *Psychol. Rev.,* 57, 1950, 94-107; Individual behavior in uncertain situations: An interpretation in terms of

bility of response on trial *n* plus a term representing the events of that trial. Where R_1 and R_2 are alternative responses followed by the alternative stimulus-events E_1 and E_2, each occurrence of E_1, it is predicted, will increase the probability of response R_1 and, *pari passu,* decrease the probability of response R_2. Thus, the stochastic models predict matching of probabilities, and they are very explicit in deriving it from a 'positive recency-principle,' *i.e.* probability of a response, it is predicted, will change each trial in the direction of the most recently occurring event.

Changes from trial to trial in probability of response have recently been investigated. Although the behavior of rats appears to conform to the positive recency-principle implicit in the stochastic models for learning, Jarvik noted that human *S*s tend to pattern their responses according to a negative recency-principle.[6] Goodnow and Pettigrew, Hake and Hyman, and others have established the generality of this finding in a variety of experimental situations.[7] These results suggest that, while average levels of probability of response may be similar, the trial-to-trial changes are quite different for the two species.

The present investigation was undertaken in an effort to compare directly the trial-to-trial changes in probabilities of response for rat and human *S*s. It was conducted in two parts: Part I with rats; Part II with human *S*s. Each part involved a simple two-alternative choice-situation; and, to enhance comparability, the pattern of alternative events E_1 and E_2 was identical in the two parts. Half of the events in each sequence were of type E_1 and half were of type E_2. The order was constructed at random with the exception that runs of more than three consecutive similar events were not permitted.[8] It was hypothesized that the direction of trail-to-trial changes in probability of response would be different for the two species.

Part I: Subjects. Fifteen experimentally naïve male albino rats, approximately 120 days old, were used as *S*s. They were housed in separate living-cages and maintained on a 23 hr. deprivation-schedule beginning one week before pretraining started.

Apparatus. The apparatus employed was a simple, elevated T-maze 4 ft. long an

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statistical association theory, in R. M. Thrall, C. H. Coombs, and R. L. Davis (eds.), *Decision Processes,* 1954, 127-138; Theory of learning with constant variable, or contingent probabilities of reinforcement, *Psychometrika,* 22, 1957, 113-132; W. K. Estes and C. J. Burke, A theory of stimulus variability in learning, *Psychol. Rev.,* 60, 1953, 276-286; R. R. Bush and Fred Mosteller, A model for stimulus generalization and discrimination, *ibid.,* 58, 1951, 313-323; A model for stimulus generalization and discrimination, *idem,* 413-423; *Stochastic models for Learning,* 1955, 1-365; R. R. Bush, Fred Mosteller, and G. L. Thompson, A formal structure for multiple-choice situations, in Thrall, Coombs, and Davis (eds.), *Decision Processes,* 1954, 99-126.

[6] Jarvik, *op. cit.,* 291-297.

[7] J. J. Goodnow and T. F. Pettigrew, Effect of prior patterns of experience upon strategies and learning sets, *J. exp. Psychol.,* 49, 1955, 381-389; Hake and Hyman, *op. cit.,* 1953, 64-74; Brown and Overall, Implications of recency effects for probability learning theories, *J. gen. Psychol.,* in press.

[8] Goodnow and Pettigrew have shown that short lengths of run enhance the negative recency-effect in human behavior, *op. cit.,* 1955, 49, 16-22. The negative offect is, however, also observed in long runs; see Jarvik, *op. cit.,* 291-297, and Brown and Overall, *op. cit.,* in press.

2 in. wide, with starting section 2 ft. long and 2 in. wide. The maze surface was 15 in. above the floor. Two identical goal-boxes 9 × 9 × 9 in. were placed at the ends of the maze, and the maze and goal boxes were painted a uniform gray. An enclosed restraining-compartment 9 × 9 × 9 in. was placed near the start of the maze.

Procedure. Each *S* was given several days of pretraining on a straight runway until it ran the alley and ate without hesitation. During the experimental period, each rat was given 25 trials per day for two consecutive days with reward sometimes on the left (E_1) and sometimes on the right (E_2). Between trails, *S* was returned to the enclosed restraining-compartment for approximately 30 sec. while reward was placed for the next trial. The noncorrection method was employed throughout.

Part II: Subjects. Twenty-eight college students from an introductory course in psychology were voluntary *S*s in this experiment. They had not participated in psychological experiments prior to this one.

Procedure. The *S*s participated as a group. A technique invented by Jarvik was followed, using the stimulus-sequence which had previously been employed in the experiment with rats.[9] The *S*s were instructed to predict, for each trial, whether *E* would say "plus" (E_1) or "check" (E_2). Predictions were written on the answer sheet provided each *S*, after which *E* made the appropriate response. Approximately 15 sec. intervened between successive trials.

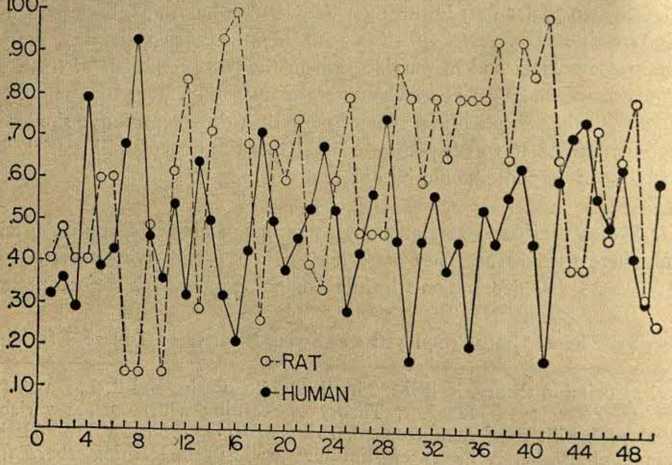

FIG. 1. COMPARISON OF TRIAL-TO-TRIAL RESPONSES OF RATS AND OF HUMAN *S*S TO IDENTICAL TWO-CHOICE ALTERNATIVE SEQUENCES

Results. The results with the rats and human *S*s are presented for comparison in Fig. 1. The open circles represent the proportion of rats, the closed circles the proportion of humans, that chose alternative A_1 on each of the 50 trials. The rats were consistent in positive recency-behavior, *i.e.* there was an *increase* in the tendency to respond to A_1 following occurrences of reward at A_1 and a *decrease* in the tendency to respond to A_1 following occurrences of reward at A_2. Human *S*s, on the other hand, evidenced a strong negative recency-effect. They showed an *increase* in the tendency to predict E_2 following occurrences of E_1, and vice versa. In general, there is a matching of probability evident in the behavior of both rat and human *S*s. Even so, however, the two opposed recency-mechanisms result in a difference in general trends. Even though probability of response for both rats and humans approximates the probability of the stimulus-events, it is obvious that the mechanism determining this matching is quite different in the two cases.

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[9] Jarvik, *op. cit.*, 291-297.

The results presented above differ in many respects, though they do employ the same sequence of stimulus-events and a single model for learning has been applied to experiments similar to those presented.[10] The results of rats and men are compared here to emphasize the need for caution in applying single principles of learning to the very different behaviors observed with different species in different learning-situations. It is suggested that a cognitive approach offers the best possibility for integrating comparative research at one level of discourse. Goodnow and Pettigrew have investigated the strategies employed by human *Ss* in a variety of experimental situations.[11] The problem might well be broadened to comparison of the strategies employed by different species in different types of decision-making situations.

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[10] Brunswik, *op. cit.,* 175-197. Jarvik, *op. cit.,* 291-297.

[11] Goodnow and Pettigrew, *op. cit.,* 16-22; for the comparative instance, see Overall and Brown, *op. cit.,* 314-322.

REFLEX AND CONDITIONED RESPONSES AS A FUNCTION OF MANIFEST ANXIETY

By WILLIAM F. PROKASY, JR., Pennsylvania State University,
and CHARLES B. TRUAX, University of Wisconsin

In recent years, considerable interest has been centered around anxiety as a variable in learning. The most common attempt to delineate experimentally its effects has been through the use of psychometrically defined anxiety, such as that measured by the Manifest Anxiety Scale (*MAS*).[1]

Although many studies have shown a positive relationship between level of performance in eyelid-conditioning and *MAS*-scores, relatively little attention has been paid to another response that occurs in the interval between onset of the conditioned stimulus (*CS*) and onset of the unconditioned stimulus (*UCS*).[2] That response is the reflex blink to the tone or light *CS*. Grant has briefly discussed this response, labelling it the 'alpha' response.[3] Typically it has been found that frequency of alpha responses decreases as time in the conditioning-session increases.[4] If alpha-frequency can be taken as an index of over-all adaptation to the novelty of the conditioning session, then it might be postulated that individuals with high *MAS*-scores will emit more alphas than individuals with low *MAS*-scores.

Besides providing further information on the anxiety-conditioning correlation, the present study is designed to explore the relationship between anxiety and alpha response-frequency.

Apparatus. The basic apparatus is described in detail elsewhere.[5] The *CS* was an abrupt change in brightness of a milk-glass screen from 7 to 100 mL., lasting 0.5 sec. The *UCS* was an airpuff delivered to the left cornea by means of a fall of a 150 mm. column of mercury 0.5 sec. after the onset of the *CS*. Response-latencies were recorded with the use of an electronic differentiator which permitted fairly precise estimation of latency based upon the first time-derivative of the response.

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* Received for publication January 2, 1958.

[1] J. A. Taylor, A personality scale of manifest anxiety, *J. abnorn. soc. Psychol.*, 48, 1953, 285-290.

[2] K. W. Spence and R. S. Beecroft, Differential conditioning and level of anxiety, *J. exp. Psychol.*, 48, 1954, 399-403; K. W. Spence and I. E. Farber, Conditioning and extinction as a function of anxiety, *ibid.*, 45, 1953, 116-119; Relation of anxiety to differential eyelid conditioning, *ibid.*, 47, 1954, 127-134; K. W. Spence, I. E. Farber and Elaine Taylor, The relation of electric shock and anxiety to level of performance in eyelid conditioning, *ibid.*, 48, 1954, 404-408; K. W. Spence and Janet Taylor, Anxiety and strength of UCS as determinants of amount of eyelid conditioning, *ibid.*, 42, 1951, 183-188; The relation of conditioned response strength to anxiety in normal, neurotic and psychotic subjects, *ibid.*, 45, 1953, 265-272; J. A. Taylor, The relationship of anxiety to the conditioned eyelid response, *ibid.*, 41, 1951, 81-92.

[3] D. A. Grant, Sensitization and association in eyelid conditioning, *J. exp. Psychol.*, 32, 1942, 201-212; A sensitized eyelid reaction related to the conditioned eyelid response, *ibid.*, 35, 1945, 393-402.

[4] W. F. Prokasy, Extinction and spontaneous recovery of conditioned eyelid responses as a function of varying amounts of acquisition and extinction, Unpublished doctoral dissertation, University of Wisconsin, 1957.

[5] Prokasy, *idem.*

Subjects and procedure. The *S*s were 69 volunteers from introductory psychology courses at the University of Wisconsin. These volunteers came from a pool of *S*s who had previously taken the *MAS* inventory.[6] Each *S* was given 20 pairings of the *CS* and *UCS*, with inter-trial intervals varying randomly between 25 and 35 sec., following neutral instructions. Responses with latencies between 66 and 140 m.sec. were classed as alphas, while those occurring with latencies between 200 and 500 m.sec. were classed as *CR*s. The *S*s were divided into three groups of 23 each, the groups having mean *MAS*-percentiles of 85, 53, and 25.

Results. Fig. 1 shows mean *CR*-frequency over four blocks of five trials with anxiety-level as parameter. An analysis of variance did not permit us to reject the null hypothesis with respect to the main effect of anxiety. Since this result is not in keeping with previous research, it was decided to select the 10 most extreme *S*s on each end of the *MAS* distribution for analysis. These two groups are composed of *S*s who scored at or below the twenty-fifth percentile and those who scored at or above the eightieth percentile of the *MAS*. The resulting performance-curves are plotted as the dashed lines in Fig. 1. Although there is a slight exaggeration of the trend in a direction opposite to that found by other investigators, the effect of anxiety is still not significant.

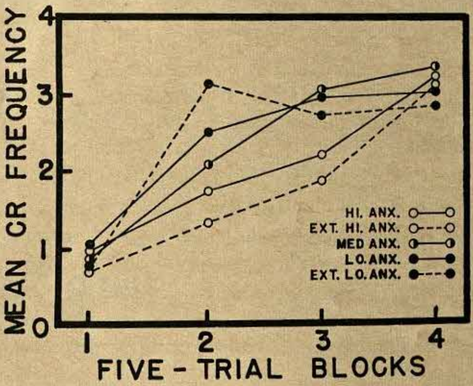

FIG. 1. CR FREQUENCY OVER FIVE-TRIAL BLOCKS, WITH ANXIETY LEVEL AS THE PARAMETER

Extreme high and low anxious Ss are designated by dashed lines.

It is doubtful that the fact that the present investigators did not remove voluntary responders can account for the discrepancy in results.[7] Not only are there relatively few *S*s that are classed as voluntary responders (approximately 10%), but since they are found equally often in the anxious and non-anxious groups there would be no bias.[8]

In research from the State University of Iowa *S*s are given a 'ready, blink' signal from 2 to 4 sec. before each trial; this was not done in the present study.[9] There is some evidence that this difference may be an important factor in the results. In those studies where the differences between the anxious and non-anxious *S*s are most marked, the percentage of *CR*s by the second trial is at or above 10% for the anxious *S*s and less than 5% for the non-anxious.[10] This unusually high response-frequency in the high *MAS* groups by the second trial may indicate that the effect of anxiety is not limited solely to its influence by way of the *CS-UCS* presentations. It is possible, for instance, that the *MAS* distinguishes those individuals who are reactive to an instructional set, and that this manifests itself in the conditioning scores.

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[6] The authors wish to thank Dr. Barclay Martin for making these scores available.

[7] J. A. Taylor, *op. cit.*, 1951, 81-92.

[8] Spence and Farber, *op. cit.*, 1954, 127-134.

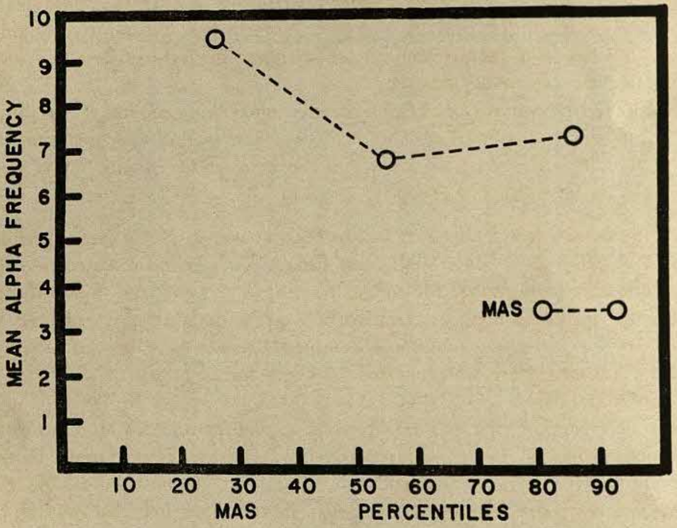

FIG. 2. ALPHA FREQUENCY AS A FUNCTION OF MEAN *MAS* GROUP-PERCENTILES

A significant difference was found among the three anxiety-levels in average number of alpha-responses. The obtained difference, however, is in the direction opposite to that predicted. Fig. 2 shows that the less anxious group averaged higher in frequency of alpha-responses than did the highly anxious and the medium groups. No significant difference in number of *CR*s was found as a function of the frequency of alpha-responses.

The findings with respect to the alpha-response and anxiety-level appear to merit further investigation, since such a difference in molecular behavior among groups defined in terms of a personality variable is not a common phenomenon. It may be that the alpha-response (*i.e.* reflex response to a flash of light) has potential value as a clinical tool. This is because, as a reflex response, it could prove to be a fairly reliable performance measure.

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[9] Note that this is an optimal foreperiod in reaction-time experiments; see R. S. Woodworth and H. E. Schlosberg, *Experimental Psychology*, 2nd ed., 1954, 29.

[10] Spence and Beecroft, *op. cit.*, *J. exp. Psychol.*, 48, 1954, 399-403; J. A. Taylor, *op. cit.*, 1951, 81-92.

SUBLIMINAL STIMULATION IN HUMAN LEARNING

By JACK A. VERNON and DAVID H. BADGER, Princeton University

One is tempted to dismiss without further consideration the claims concerning the effectiveness of subliminal stimulation currently being made by advertisers and 'motivational researchers.'[1] Certainly there is little reason to believe that the claimants are competent to design appropriate experiments or even that they clearly grasp the implications of the concept of *limen*. Nevertheless, the positive findings which have appeared from time to time in the scientific literature suggest that some further study of the problem may be useful.[2]

Subjects. Ninety-four students of beginning psychology were pre-tested for ability to learn a 12-item list of adjectives. The method of anticipation was used, and the words were presented by a memory-drum. On the basis of the results, 60 *S*s were selected to make up three matched groups of 20 each.

Task I. The first task set the *S*s was to learn a nine-item list of paired adjectives selected for low associative value. Subliminal stimulation was incorporated into the task in the following way. Before the formal training was begun, the stimulus-word of each pair was projected upon a screen for a 2-sec. period. After it had been on for 1 sec., a second word was subliminally flashed adjacent to it and just above a fixation-point. This procedure was repeated 10 times for each pair of associates, a total of 90 subliminal trials.

For the *S*s of Group A, the subliminal word accompanying each stimulus-word was the response-word to be learned subsequently. For the *S*s of Group B, the subliminal words were those subsequently to be used as response-words, but the pairing was different; the subliminal word was in each case one that was to be associated with another stimulus-word. For the *S*s of Group C, the subliminal presentations were 'blanks.'

Subliminality was based on threshold measurements. Under conditions of constant illumination, fixed presentation-distance (7 ft.), fixation-point, and immobilization

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* Received for publication May 22, 1958.

[1] Herbert Brean, New subliminal gimmicks now offer blood, skulls, and popcorn to movie fans, *Life,* March 31, 1958, 102-114; John Brooks, The little ad that isn't there—a look at subliminal advertising, *Con. Res. Rep.,* Jan., 1958, 7-10; N. F. Defoe, Subliminal ad try in Canada flops, *Christ. Sci. Mont.,* Feb. 17, 1958, 8-13; Vance Packard, *The Hidden Persuadors,* 1957, 275; G. P. Talese, Most hidden persuasion, *N. Y. Times Magazine,* Jan. 12, 1958, 57-60.

[2] See, for example, a group of studies on the Müller-Lyer illusion with invisible wings. Knight Dunlap, The effect of imperciptible shadows on the judgement of distance, *Psychol. Rev.,* 7, 1900, 435-453; E. B. Titchener and W. H. Pyle, The effect of imperceptible shadows on the judgement of distance, *Proc. Amer. Philos. Soc.,* 46, 1907, 94-109; Joseph Bressler, Illusion in the case of subliminal visual stimulation, *J. gen. Psychol.,* 5, 1931, 244-250. The results of Dunlap and Bressler were positive, although those of Titchener and Pyle were negative. The literature on 'perceptual defense' also is relevant, although critical studies have all but undermined the concept.

of the head, the exposure-time necessary for the identification of words similar to those to be learned was determined by the method of limits. The tachistoscope used permitted continuous variation of exposure-time from 1/400 to 1 sec. The threshold-functions obtained were rather steep, and slight variations in time could very easily shift the presentations from subliminal to liminal. The functions also differed markedly from *S* to *S*, and it was not possible to pick a mean value which would work for all. *Subliminal* was defined as the 25%-threshold for each *S*, and this value was used throughout.

At the completion of the subliminal presentations, *S* was required to learn the list of paired associates. The pairs were presented in different sequential orders from trial to trial to rule out serial learning. The words were presented in a memory-drum with a 2-sec. interstimulus interval and a 5-sec. interlist interval. The criterion of learning was one errorless trial.

TABLE I

RESULTS FOR THE THREE GROUPS

Task	Measure	Group A	Group B	Group C
I	Correct responses	74	76	72
	Efficiency-ratio	0·77	0·87	0·80
	Response-failures	45	49	50
	Oscillations	18.	17·7	17·8
II	Trials to criterion	14·7	17·4	14·9
	Correct responses	8·3	8·9	7·3

Task II. The second task involved a simple discrimination. Three different geometric forms were presented—a circle, a square, and a triangle. On each trial, all three appeared, but the positions varied from trial to trial. On each trial, *S* was asked: "Which form is correct?" His response was called 'right' or 'wrong,' and then further trials were given to the criterion of five consecutive errorless responses.

The subliminal presentation came just prior to each trial. It was done in the following way: Suppose that a particular *S* had to get the concept *all squares*. Just before each trial, 'all squares' would be flashed subliminally to him if he was in Group A, or 'all triangles' if he was in Group B, or a blank if he was in Group C. All the *S*s were instructed that the correct answer would be subliminally given prior to each trial. Groups A, B, and C were treated as before, except that the *S*s were so rotated that the previous Group A became Group B, and so forth.

Results. The means of various measures of performance in the two tasks are presented in Table I. Measures such as efficiency-ratio, oscillation, and the like were used on the chance that there might be effects of subliminal presentation too subtle to be detected otherwise.[3] In no case, however, did the *t*-test or Wilcoxon's test for paired replicates indicate a statistically significant difference between any pair of groups. That is, the performance of Group A was not facilitated, nor was the performance of Group B impaired.

Conclusion. No evidence for the effectiveness of subliminal stimulation in simple learning appears in this experiment.

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[3] J. A. Vernon and T. E. McGill, The effects of sensory deprivation upon human learning, this JOURNAL, 70, 1957, 637-639.

APPARATUS

A RESPONDENT-CONDITIONING APPARATUS FOR THE STUDENT LABORATORY

BY RUSSELL M. CHURCH and CARL PFAFFMANN, Brown University

Operant conditioning procedures have been successfully adapted to the typical undergraduate psychology section. Their widespread use is probably due, in no small measure, to their tested reliability in 'getting results.' We have found this to be so at Brown by using a simplified Skinner lever-pressing box for the rat. Any undergraduate can establish the conditioned operant and demonstrate many of its basic properties for himself. The situation with respondent-conditioning, on the other hand, has been most unsatisfactory. We can now report that we have a reliable apparatus with which the student experimenter (*E*) can individually establish and study the conditioning and extinction of the eyelid-response in *S*, his laboratory partner (see Fig. 1). In addition, it is possible to study the effect of variations in the conditioned-unconditioned stimulus-interval (*CS-US*) and in the intensity of the conditioned stimulus (*CS*), and also to demonstrate the phenomena of generalization and differentiation. The present device uses a 60 ~ tone as the *CS* followed by an air-puff to the eye as the unconditioned stimulus (*US*). *E* watches *S*'s eyes and records whether *S* gives a wink (*W*), a partial wink (*PW*), or no response (*NR*).

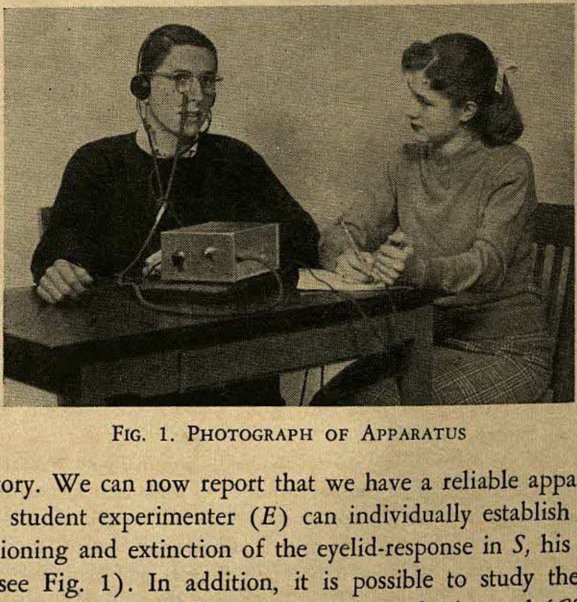

FIG. 1. PHOTOGRAPH OF APPARATUS

Apparatus. The device, mounted in a 5 × 4 × 3-in. standard aluminum radio-box (Premier AMC 1009) is powered from the 110-v. AC lines. *E* presents the *CS* by pressing a silent hand-switch, (*A* in Fig. 2) that closes the circuit through the normally closed contacts of the double throw time-delay relay *B* (Heinman AN

522 XAX relay ¼ sec.). This circuit leads to the pilot light and through the normally closed toggle switch *C* to the primary coil of the transformer *D* (AP Foster 2004 transformer). The output of the secondary coil of the transformer leads through a series volume control *E* (Clarostat A-10-5 K control) to the earphones *F* (Trimm #25 headset). From ¼–1 sec. later, depending upon the setting of the series resistance *G* (Clarostat A-10-1 K control), the time-delay relay *B* breaks the circuit to the transformer primary (terminating the tone) and closes the circuit to the solenoid *H*. This actuates the rubber tipped plunger of the solenoid (Filtrol 52 C 115-60 Solenoid) which compresses a sphygmomanometer bulb *I*. The puff of air so produced is led by rubber tubing with dust filter *J* to a small nozzle wired to the lower rim of a pair of spectacle frames *K*. This provides the *US* for the eyewink.

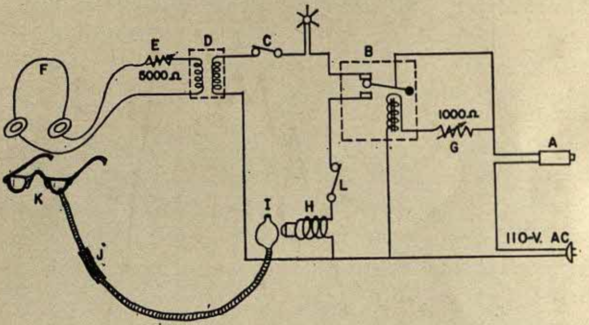

FIG. 2. DIAGRAM OF APPARATUS
(See text for description of components.)

The air-puff (*US*) can be presented without tone (*CS*) by opening the circuit to the transformer primary at the toggle switch *C*. This is necessary only at the beginning of the experiment to test the effectiveness of the *US*. Test-trials of *CS* alone can be presented by depressing the normally closed foot-switch *L* (Linemaster T-S1-S) thus breaking the circuit to the solenoid. The use of a foot-switch leaves *E*'s hand free to record responses.

The cost of a single unit is approximately $25, excluding labor. The device is relatively simple to build and we have constructed 20 such pieces of equipment with the aid of our assistants in the several courses using this device. In our model, the solenoid producing the air-puff is somewhat noisy. We have reduced the noise to some extent by floating the solenoid on rubber grommets and by placing the whole box on a felt typewriter pad to reduce the sounding board effect of the table-top.

Procedure. During the laboratory sessions, the students work in pairs; one serves as *S* and the other as *E*. *E* operates the apparatus, and on each trial he records the eyelid-response as either a wink (*W*), partial wink (*PW*), or no response (*NR*). The first 10 pre-test trials show the degree to which the *CS* alone elicits eyelid-closure. The puff of air is given alone on the next 5 trials to demonstrate its effectiveness as a *US* for eyelid-closure. The conditioning trials are given in blocks of 5; 4 are pairings of the *CS* and *US* and 1 is a test-trial of the *CS* alone. Each *S* is given 15 blocks of acquisition-trials followed by 15 blocks of extinction-trials (*CS* alone). *E* is instructed to present no more than 2 trials in any minute and to space the trials randomly within this interval.

Results. The results for one group of 24 *S*s during acquisition and extinction are plotted in Fig. 3 which shows the percentage of *S*s giving full winks on each test-trial. Only 3% of the pre-test trials were full winks (seven out of 240). The number of *W*s (full winks) showed a gradual increase during the 15 test-trials of acquisition and a decrease during the 15 extinction-trials. Conversely, the *NR*s (no responses) showed a decrease during acquisition and an increase during extinction. It is interesting to note, however, that the *PW*s (partial winks) showed an increase and then a decrease during acquisition (maximal on the seventh trial) and also an increase followed by a decrease during extinction (maximal on the seventh trial). Thus it appears that the response gradually moved from *NR* through *PW* to *W* during acquisition, and in the reverse direction during extinction. Fig. 4 gives the results for an *S* plotted as a cumulative curve of the number of winks on successive trials.

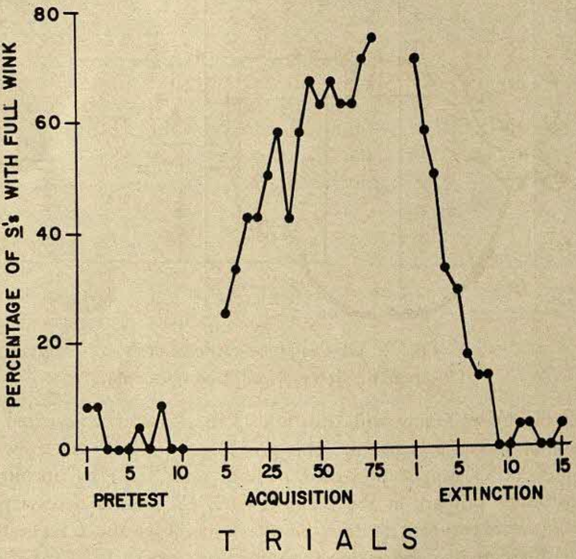

FIG. 3. ACQUISITION AND EXTINCTION OF CONDITIONED EYELID RESPONSE
Group-data from a class of 24 *S*s. In acquisition the points are based on 1 test-trial per block of 5 trials.

The interval between the *CS* and the *US* is usually set at ½ sec., presumably the optimal interval for conditioning. One group of *S*s was, however, successfully conditioned at the minimal *CS-US* interval (0.3 sec.), but a second group obtained only a low level of acquisition and extremely fast extinction with the maximal *CS-US* interval (1.0 sec.). These preliminary data suggest that the present apparatus is suitable for demonstrating on the effect of the *CS-US* interval, but that a time-delay relay with a wider range would be desirable for systematic study of this relation.

In another experiment, the *S*s were conditioned to a tone of medium intensity, and then tones of high, medium, and low intensity were used for discrimination-training; only the medium tone received reinforcement, irregularly interspersed among test-trials of all *B* tones. The results indicated that the number of winks to the three tones was initially similar, but while the percentage of *S*s giving winks to the tone of medium intensity stayed constant, the number of winks to the tone of high intensity and, especially, to the tone of low intensity decreased during the trials of discriminative training. These results indicate that the apparatus is adequate to demonstrate generalization, discrimination, and possibly, the influence of *CS*-intensity.[1]

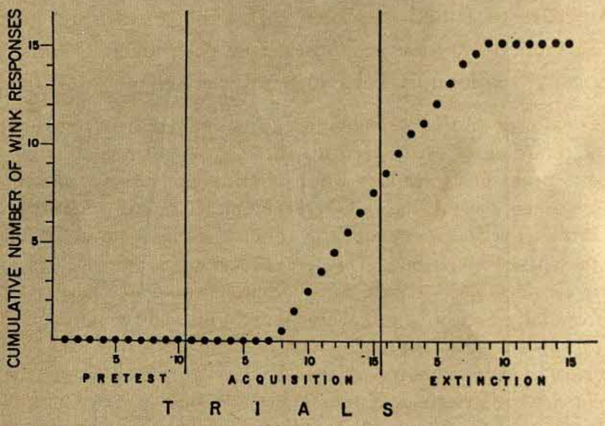

FIG. 4. ACQUISITION AND EXTINCTION OF CONDITIONED EYELID RESPONSES OF ONE *S*

A partial wink is recorded as one-half a full wink. During acquisition there were 4 *CS-US* pairings and one *CS* alone per block of 5 trials. After the pre-test with *CS* alone there were 5 trials with *US* alone. The figure shows only trials in which *CS* alone was presented.

Other phenomena of respondent conditioning may be studied with this apparatus; for example, spontaneous recovery, disinhibition, the relative speed of first and second acquisition and of first and second extinction, and the effect of partial reinforcement on resistance to extinction.

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[1] Our thanks are due our colleagues Nissim Levy, Lewis Lipsitt, and Frances Clayton who helped to design the apparatus and to collect data in their laboratory sections.

APPARATUS FOR RECORDING ORIENTING RESPONSES

By Robert B. Zajonc and Eugene Burnstein, University of Michigan

In experiments on perception and discriminative learning, it is often as necessary to determine what *S* is looking at as what he is seeing. The former type of response which exposes the organism to new stimuli or new properties of stimuli has been called the *orienting* or the *observing response*.[1] Generally, these responses have been taken as indices of attention, curiosity, exploratory behavior, and the like.[2]

The method of measuring orienting responses most commonly used with human *S*s consists of recording of eye-movements. Berlyne, for instance, observed *S*'s eyes and recorded by means of Morse keys which of two figures he was attending to.[3] More exact measurements may be taken by eye-movement cameras, such as the ophthalmograph[4] or the eye-camera developed by Brandt, designed to assess the attentional value of newspaper advertising.[5] The ophthalmograph and Brandt's eye-camera, aside from their considerable cost, have some technical weaknesses. The major difficulty is that both are rather sensitive to *S*'s head-movements. This requires keeping *S*'s head stationary, which is not often feasible nor desirable. Even with such devices as head- and chin-rests, and biting-boards of dental wax, *S*'s head is not perfectly stationary. Consequently, what sometimes appears as eye-movement in the record may have been caused by the motions of *S*'s head. A further difficulty with eye-movement cameras is that some fixations are of such a short duration that they fail to leave a trace on the film. In addition, the left-to-right tendency of eye-movements stemming from reading-habits cannot be always controlled in a satisfactory manner.

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* This project was a part of a research program on information-processing supported by a grant, NSF G2802, from the National Science Foundation.

[1] See, for instance, K. W. Spence, Continuous versus non-continuous interpretations of discrimination learning, *Psychol. Rev.*, 47, 1940, 271-288; D. E. Berlyne, The influence of complexity and novelty in visual figures, *J. exp. Psychol.*, 55, 1958, 289-296; L. B. Wyckoff, The role of observing responses in discrimination learning, *Psychol. Rev.*, 59, 1952, 431-442.

[2] Berlyne, Attention, perception and behavior theory, *Psychol. Rev.*, 58, 1951, 137-146.

[3] Berlyne, Novelty and curiosity as determinants of exploratory behavior, *Brit. J. Psychol.*, 41, 1950, 68-80; A theory of human curiosity, *ibid.*, 45, 1954, 180-191; An experimental study of human curiosity, *ibid.*, 45, 1954, 256-265; Conflict and information theory variables as determinants of human perceptual curiosity, *J. exp. Psychol.*, 53, 1957, 399-404; Berlyne, *ibid.*, 55, 1958, 289-296.

[4] The ophthalmograph records horizontal eye-movements by photographing the reflection of a beam of light from the *S's cornea*. It is mainly used in optometric diagnosis and in the analysis of reading habits. Allen has recently developed an apparatus which obtains horizontal as well as vertical eye-movements. (See M. J. Allen, A simple photographic method for continuously recording vertical and horizontal eye-movements, *Amer. J. Optom.*, 32, 1955, 88-92.)

[5] H. F. Brandt, A bidimensional eye-movement camera, this Journal, 49, 1937, 666-669; *The Psychology of Seeing*, 1945.

Considerably more precise measurements of eye-movements may be obtained by electronic means. This method is based on the relationship between eye-movements and the potential of the periorbital skin surface.[6] Two pairs of electrodes are affixed around the eye defining the vertical and horizontal coördinates intersecting the pupil. By recording differences in periorbital potentials Law and DeValois were able to record eye-movements as small as ½°.[7] Despite its high accuracy this technique is not too practical for most experiments in the area of perception and discriminative learning. First, the method requires expensive apparatus. Secondly, the exact location of the vertical and horizontal coördinates about the eye cannot be easily determined. Thirdly, the use of electrodes about the eye makes for some discomfort of the *S*. Lastly, the technique, like the ones mentioned above, is also subject to errors of head-movement.

All the methods described thus far may have one additional difficulty. *S* knows that the experiment has something to do with perception, and he may thus develop a set whose effects confound the experimental results. Many of these deficiencies may be avoided by the use of the following simple and inexpensive apparatus. The objectives in designing this apparatus were mainly to provide conditions where orienting responses would occur at slower rates, thus facilitating their recording, and to conceal from *S* the perceptual nature of his task. These objectives were attained by transferring the control over *S*'s orienting responses from his eyes to his hands.

Apparatus. The apparatus consists of two components, the exhibitor and an Esterline-Angus 20-pen recorder. Only the first component will be described here. The exhibitor (see Fig. 1) is constructed from a 20 × 20 × 4-in. plywood box with a hinged, translucent, ¼-in. thick, plexiglass cover (A). In the center of the plexiglass cover there is a 2 × ¼-in. opening (B). Directly under the opening on the inside of the box a 1 × ¼-in. copper plate (G) is affixed flush with the bottom of the box. The ground wire from the recorder (C) is soldered to the copper plate from the outside of the box.

The box contains an 8 × 8 × 3-in. stimulus-chassis (D). Twenty ¼-in. round holes are drilled at random in the bottom of the stimulus-chassis. In each hole is inserted a ½-in. ball-type brass cupboard doorlatch (F) with the ball of the latch facing the outside of the chassis. The terminals from the recorder-pens (E) are soldered to the inside portion of the latches. The top of the chassis serves to hold the stimulus-material which is presented on a stiff 8 × 8-in. cardboard held by four mounting corners. The positions of the stimulus-objects (nonsense-syllables, words, etc.) correspond exactly to the positions of the latches.

Two 12-in. handles are attached to two opposite sides of the stimulus-chassis and protrude through 2 × 12-in. slits cut in the corresponding sides of the box. The chassis rests on the 20 doorlatches and can be freely moved from the outside of the box by manipulating the handles. Whenever a latch rides over the copper plate a contact is made and a corresponding pen in the recorder is activated. The contact is maintained until the stimulus-object is removed by the *S* from his field of vision.

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[6] W. R. Miles, The steady polarity potential of the human eye, *Proc. Nat. Acad. Sci.*, 25, 1939, 25-36.

[7] Thomas Law and R. L. DeValois, Periorbital potentials recorded during small eye movements, *Mich. Acad. Sci., Arts, Let.*, 13, 1958, 171-180.

The exhibitor is placed on a low table with *S*'s head directly over the opening in the plexiglass cover (B). Because the cover is translucent, the locations of the stimulus-objects are apparent to *S*, but they appear as blurs and cannot be identified by him. *S* manipulates the handles, thus exposing the stimulus-materials one at a time to his vision. The 20-pen recorder gives a continuous record of what object, syllable, or word was exposed, when it was exposed, and how long it was exposed.

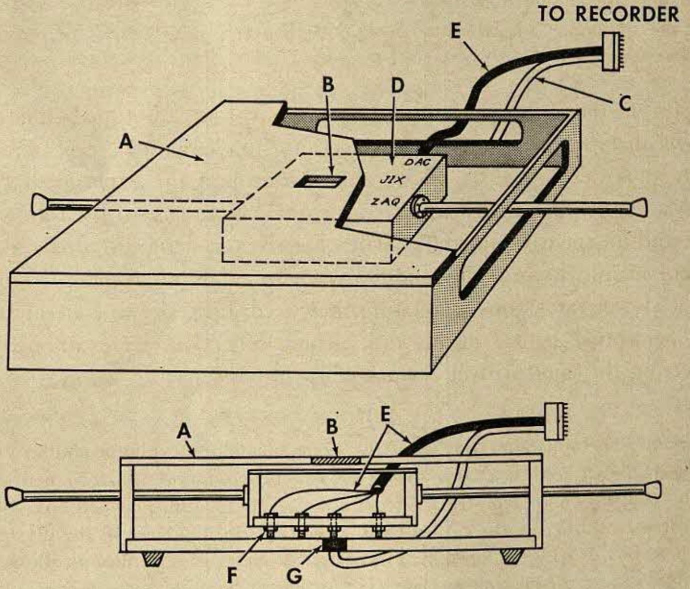

FIG. 1. EXHIBITOR OF ORIENTING RESPONSES
A = Plexiglass cover; B = Opening exposing stimulas-material; C = Ground wire to the recorder; D = Stimulus-chassis; E = Terminals at doorlatches; F = Doorlatches; G = Copper plate

An interchangeable cover and chassis can easily be constructed to accommodate stimulus-materials of a different size. If it is desirable to randomize positions, fewer stimulus-objects may be used and on each trial presented in different positions.

The apparatus may also be employed in human maze-learning where it is desirable to expose only a small part of the maze to *S*'s view. For this purpose, an opaque cover is substituted for the standard one.

Most *S*s learn quickly to operate the apparatus. Rarely is more than one practice trial necessary.

The sensitivity of the apparatus may be demonstrated by the following illustrative example. In an experiment now being conducted, the effects of associative value of nonsense-syllables on orienting responses in a rote-learning situation are examined. *S* is instructed to learn a set of 18 nonsense-syllables presented in the apparatus. Six of the nonsense-syllables are of 0%, six of 43%, and six of 100% associative value.[8] A set of 3-min. trials is given until *S* attains a specific criterion. The differences in the duration of observing responses as a function of differences in associative value are rather pronounced. Fig. 2 shows the results of an *S* selected at random. The curves show the average cumulative time in 10-sec. intervals spent on nonsense-syllables of different associative values. Already from the first 10-sec. interval *S* shows a considerably longer concentration on syllables of low associative value (0%) and maintains such differential attention systematically until the learning session is concluded.

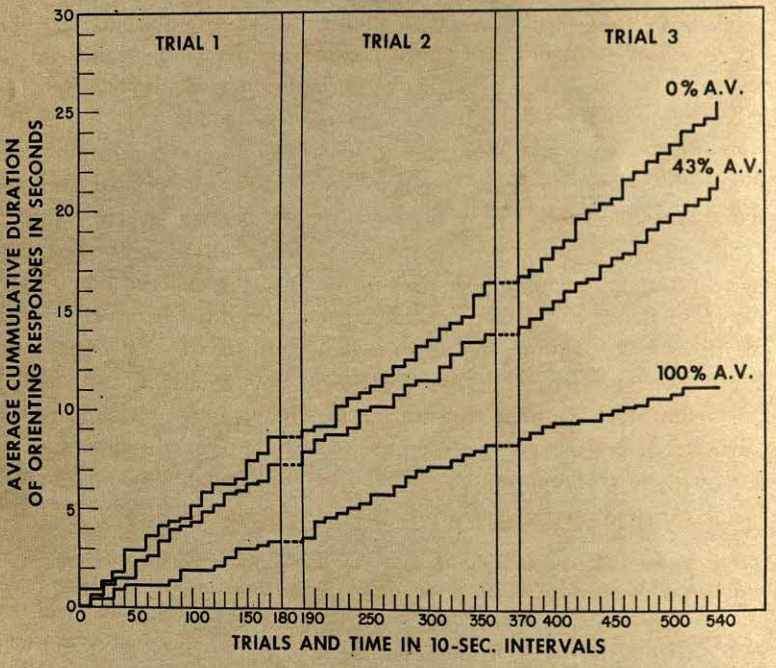

FIG. 2. AVERAGE CUMULALTIVE DURATION OF ORIENTING RESPONSES TO NONSENSE SYLLABLES OF 0%, 43%, 100% ASSOCIATIVE VALUE

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[8] The nonsense-syllables were selected from lists prepared by Glaze. J. A. Glaze, The association value of nonsense syllables, *J. genet. Psychol.*, 35, 1928, 255-267.

A DEVICE FOR DETECTING AND CONTROLLING AUTOMATIC PROGRAMMING OF AVOIDANCE-CONDITIONING IN A SHUTTLE-BOX

By F. ROBERT BRUSH and P. ROBERT KNAFF, University of Maryland

Devices for automatically programming and recording events in a Skinner box have been developed to a peak of high efficiency—and cost. Investigators interested in avoidance-conditioning in a shuttle-box, however, have been faced with the tedious and time-consuming task of manually presenting stimuli and recording responses. In studying an arbitrary response like lever-pressing, response-events are easily defined and converted into electrical energy. The rat simply closes a switch. With the equally arbitrary response of running, as studied in the shuttle-box, automatic programming has been lacking because the experimenter was required to define the response. The apparatus reported here performs the essential task of defining the response in a shuttle-box for rats. The 'fence-sitting' behavior frequently observed in the shuttle-box, is easily and appropriately dealt with by this apparatus. The circuitry is simple and involves relatively inexpensive components. While originally developed for work with rats, the apparatus can be used, with suitable modifications, in a wide variety of two-state systems.

Description and operation. Although the shuttle-box contains only one bit of information, *i.e.* it is essentially a two-state system, redundancy is introduced for the sake of reliability. Two photocell units, one for each compartment of the shuttle-box, are employed to detect the animal's presence in one side or the other. As illustrated in Fig. 1, two mirrors (M) are mounted parallel to each other just above the grid floor (B), along the front and back walls of each compartment. The mirrors are about 3 in. in height and 1 in. shorter than the length of the compartment. This permits a source of light (L) to be introduced at the rear end of the compartment. The light is then reflected back and forth across the floor of the compartment between the mirrors. By the use of plate glass mirrors and an ordinary 6-v. flashlight bulb, sufficient light is reflected along the compartment to be picked up by the CL-3 photocell (P) mounted on the wall at the center end of the compartment.

Fig. 2 provides the schematic circuit-diagram of the photocell units.[1] When the reflected light strikes the photocell, its resistance drops, changing the grid bias on

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[1] Photocell circuit modified from A. M. Ferres, Crystal photocell circuits, *Radio & Television News,* 57, 1957, 58-59 and 102-103. Our modifications contain additional elements which make the circuit more flexible and reliable than the original.

VI, the 5823 gas discharge-tube.[2] The tube conducts when a critical bias is reached, closing the relay.[3] With SW-2 closed, the relay will lock in the closed position until it or the main power switch SW-1 is opened. Because of individual differences in sensitivity of the relay and values of resistors, R-4 is provided to permit an adjustment which guarantees locking the relay closed when SW-2 is closed. R-3 permits adjustment of the sensitivity of the system to a wide range of light-intensities. SW-3 provides the option of delivering voltage to the output receptacle through either the normally open or the normally closed contacts of the relay. The additional double throw pole on the relay is available for switching voltages other than 117-v. AC, and may be wired similarly to another output receptacle.

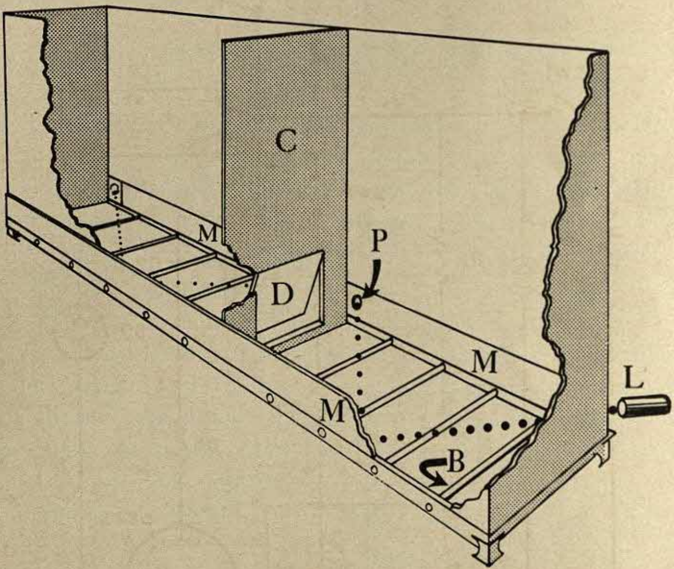

FIG. 1. SCHEMATIC DIAGRAM OF AUTOMATIC SHUTTLE-BOX
B = grid bars; C = center position; D = swinging door; L = light source;
M = mirrors; P = location of photocell

Fig. 3 schematically presents the remaining part of the apparatus. It consists of two horizontally mounted solenoids (S_1, S_2), a steel rod serving as the armature (A) for each solenoid in push-pull fashion, and two microswitches (MS-1, MS-2) driven by actuators mounted on the armature. The solenoids are wired to the outputs of the photocell units, one for each unit. As the rat moves back and forth from one compartment to the other, the solenoids pull the armature back and forth. MS-1 closes briefly during each change of state and, when suitably wired, provides an impulse for counting the total number of crossings, or for terminating some stimulus-event at the time of the response. MS-2 turns the system into a two-state one, the switch being either open or closed depending upon the location of the rat. This switch is used for selectively presenting stimuli to the compartment occupied by the rat.

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[2] Both the Clairex CL-2 and the CL-3 crystal photocells have been satisfactorily employed, although the CL-3 is more sensitive to the red end of the spectrum.

[3] Both 10,000- and 5,000-ohm plate relays have been employed. This variation in sensitivity is not critical.

Potentially, the system can be in four states: (1) rat in Side A; (2) rat in Side

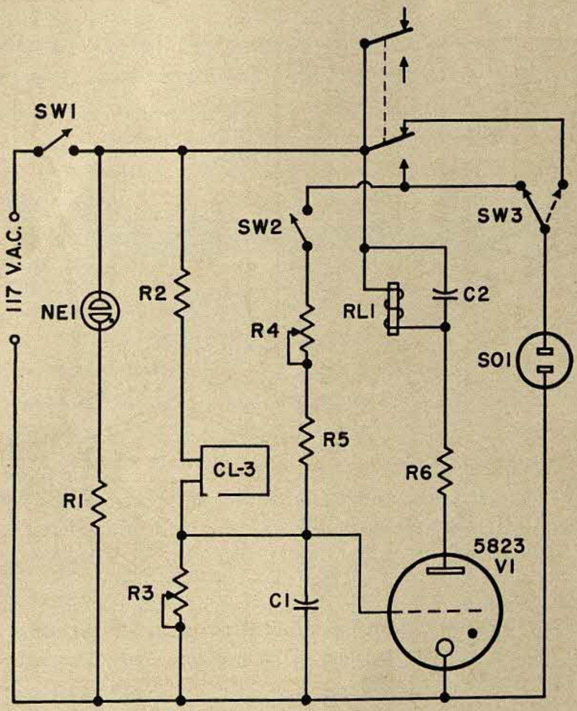

FIG. 2. SCHEMATIC CIRCUIT DIAGRAM

R 1 = 200,000 Ω, ½-w. res.
R 2 = 100,000 Ω, ½-w. res.
R 3 = 10 megohm pot.
R 4 = 1 megohm pot.
R 5 = 500,000 Ω, 1-w. res.
R 6 = 1,000 Ω, 1-w. res.
C 1 = 400 μμfd. ceramic capacitor
C 2 = 5 μfd., 150-v. elec. capacitor
SW 1, SW 2 = S.p.s.t. toggle switch
SW 3 = D.p.d.t. toggle switch
RL 1 = D.p.d.t., 5,000 Ω plate-circuit relay (Potter & Brumfield LM 11)
SO 1 = AC receptacle (Amphenol 61-F)
NE 1 = Neon pilot light (NE-51)
CL 3 = Crystal Photocell (Clairex CL-3)
V 1 = 5823 Tube

B; (3) rat in both A and B *e.g.* while crossing or during 'fence-sitting'; or (4) rat in neither A or B *e.g.* while jumping or otherwise avoiding the light-beam. Clearly, the first two states (States 1 and 2) offer no problems for automatically presenting conditioned and unconditioned stimuli. In this system MS-2 does the selecting and presents the stimuli to the appropriate compartment. This function could be accomplished by the photocell units directly. Under such direct control, however, States 3 and 4 present difficulties. If the rat were in both A and B simultaneously (State 3), the conditioned and unconditioned stimuli would be presented to both sides of the shuttle-box at once, and the shock would be unavoidable. If the rat had momentarily 'vanished' (State 4) the stimuli could not be presented at all. Both of these difficulties are easily dealt with by the properties of the solenoid switching-system. When the rat, in the act of crossing, is momentarily present in both compartments, both solenoids are activated. Since, however, the armature is more deeply embedded in the originally activated one, the second solenoid cannot move the armature and the system does not change until the rat enters one compartment *and* leaves the other. The fourth potential state of the system, the rat being in neither of the compartments, is similarly dealt with. Activation of the opposite solenoid is necessary to move the armature and this can happen only if, after 'vanishing' from one compartment, the rat appears in the opposite one. This is equivalent to a crossing and the system appropriately changes state. If the rat momentarily disappears by avoiding the light beam in one compartment, the solenoid system simply waits for his return either to the original or the opposite compartment. After a year of operation, we have yet to observe a rat successfully leaving one compartment and entering the other without immediate detection.

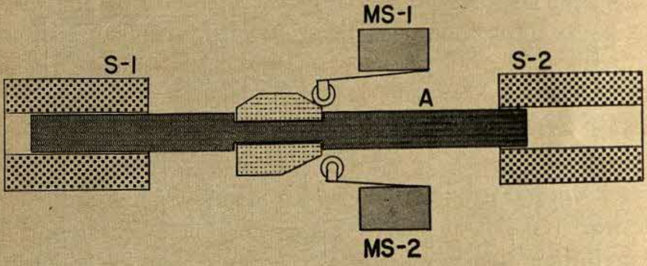

FIG. 3. SCHEMATIC DIAGRAM OF SOLENOID REVERSAL SYSTEM.
S-1, S-2 = solenoids; A = armature; MS-1, MS-2 = microswitches

No modifications to our pilot model have been necessary except for occasional adjustment of the sensitivity of the photocell units as the mirrors became progressively dirtier. After about 2 hr. of continuous operation, the solenoid system generates a good deal of heat. To ease the strain on the solenoids they are wired through the normally closed poles of the relays by appropriately positioning SW-3. Thus, when there is no rat in the shuttle-box, neither solenoid is activated. In addition, solenoids for this application should be of the 'continuous duty' type.

AN ELECTRONIC TAMBOUR: THE PIEZOELECTRIC CRYSTAL

By W. W. Dawson, Florida State University

The pneumatic tambour invented by Marey in 1878, and used by countless investigators since then, has fulfilled its purpose as a means for transforming motion into a form which can be used to drive the writing arm of some recording instrument.[1] The utility of this tambour is primarily restricted by the pneumatic volume, length of the recording arm, and flexibility of the recording system. These restrictions and the physical size of the device severely limit the use of the pneumatic tambour in many experimental situations.

In cases where high sensitivity or frequency-response are required the piezoelectric crystal is an ideal replacement. These crystals may be used for the recording of movement of a cyclic nature whose periodicity ranges between 0.05–5 × 10^{-6} sec. Their sensitivity is limited only by the amplification available from the associated electronic system. The commonest application of these crystals is found in the transducing mechanism of phonograph pick-ups. The manufactured units are mounted and fitted with various mechanical couplings which may be easily modified to fit the requirements of the individual investigator. Such units are inexpensive and may be obtained from any electronic distributor.

The distortion of the crystal element through its mechanical coupling produces an electrical signal proportional to its displacement. When amplified, the signal is useful in many ways. It may be displayed by an oscilloscope and recorded photographically; it will drive a loudspeaker for aural monitoring; or it can operate a recorder of a galvanometer.

Nominal voltage-outputs for various models range from 0.5 to 5.0 v. In situations where it is more desirable to record through a device sensing velocity than one dependent upon displacement, the crystal, which is inherently a displacement-transducer, may be converted by use of a simple network (Fig. 1). Further information on theoretical aspects of piezoelectricity may be obtained from Cady or Mason.[2]

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[1] E. J. Marey, *Méthode graphique,* 1878, 133 ff., 322 f., 456 ff.; cf. also Alfred
Binet, La circulation dans ses rapports avec la respiration et les phénomènes psy-
chiques, *Année Psychol.,* 2, 1896, 87-167; J. R. Angell and H. B. Thompson, A
study of the relations between certain organic processes and consciousness, *Psychol.*
Rev., 6, 1899, 51-69; E. B. Titchener, *Experimental Psychology: I. Qualitative;*
Instructors Manual, 176 ff.; C. A. Ruckmich, A demountable tambour, this JOURNAL,
27, 1916, 542 f.

The author has used the crystal as a transducer for the recording of minute responses from the fetal mammal *in utero*. The crystal, one of the models mentioned, was coupled to the uterus through the mechanical extension pictured in Fig. 2. Oscillographic recording was used and produced data similar to the sample in Fig. 3. The recording procedure proved entirely satisfactory, and with increased amplification the fetal pulse could be recorded as well as its gross activity. Meier also has found this method convenient where high responsiveness was required.[3] In as yet unpublished work, he reported the recording of the activity of embryonic chicks which had been subjected to various levels of anoxia. In this case, the source of

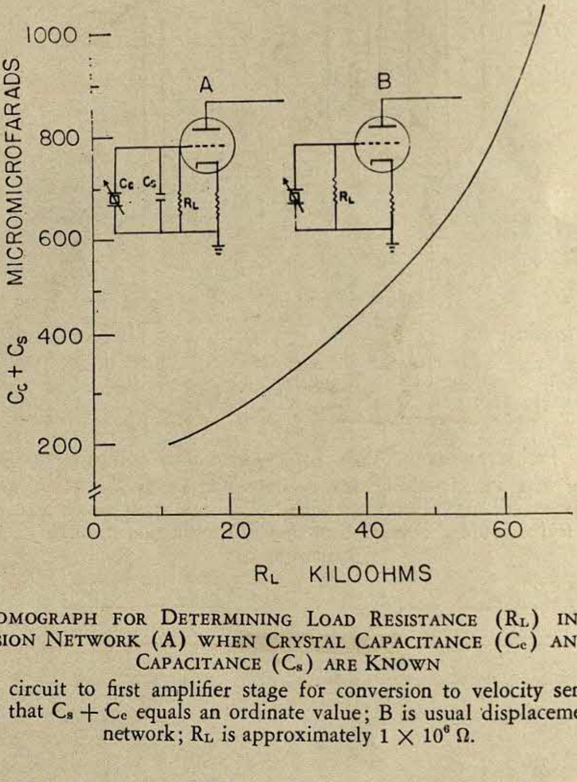

Fig. 1. Nomograph for Determining Load Resistance (R_L) in Velocity Conversion Network (A) when Crystal Capacitance (C_c) and Shunt Capacitance (C_s) are Known

A is input circuit to first amplifier stage for conversion to velocity sensing; C_c is so adjusted that $C_s + C_c$ equals an ordinate value; B is usual displacement sensitive network; R_L is approximately 1×10^6 Ω.

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[2] W. G. Cady, *Piezoelectricity,* 1946, 510-548; W. P. Mason, *Piezoelectric Crystals and Their Application to Ultrasonics,* 1950, 2-9, 114-136.

[3] G. W. Meier, Personal communication, 1957.

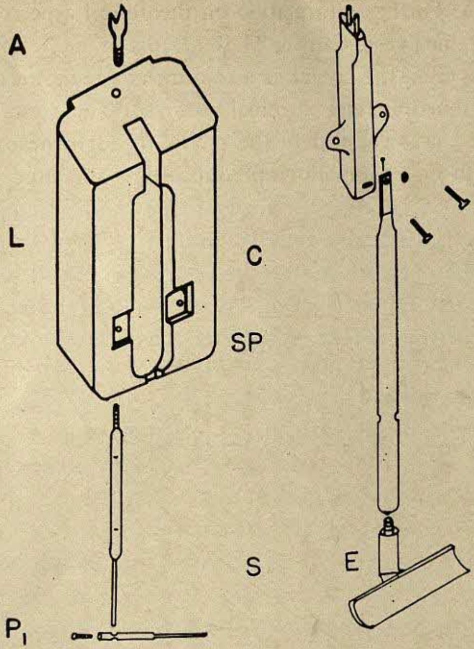

FIG. 2. ENLARGED VIEW OF TRANSDUCING ASSEMBLY
A = mounting rod; L = housing for crystal; P = pivot and pivot arms gives mechanical advantage; C = crystal cartridge; SP = stylus-post which actuates crystal element; S = stylus couples to movement under investigation through E, the stylus-extension.

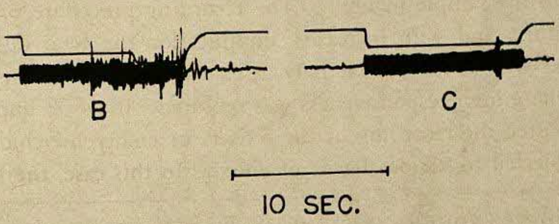

FIG. 3. RECORDINGS OF TRANSDUCED FETAL MOVEMENTS
Stimulus = sequence is marked by the upper oscilligraphic trace, activity by the lower; A = response occurs at onset of shock; B = response occurs at onset of tone and continues through shock; C = response is elicited by tone alone.

movement, the egg, was supported by the coupling to the crystal while the egg and sensing device were housed in an incubator.

In choosing a crystal for a given problem there are several points which should be considered for optimal results. The standard Rochelle salt crystal may have an output of as much as 5 v. *RMS;* however, the frequency-response of this type is limited to the range of 75–7000 cycles per sec. It is also easily damaged by higher than normal temperature and humidity. This type, then, is applicable only for certain types of data and environments where high output levels are necessary. Other cartridges are available which utilize a ceramic element, insensitive to both humidity and temperature. These have a very wide response-range, 30–15000 cycles per sec. but a somewhat lower output, 0.5–1.0 v. *RMS*. By selecting one of these discrete types or some variation of them, the experimenter can usually find a model closely suited to his particular application.

A DEVICE FOR MARKING THE PASSAGE OF ANIMALS IN RUNWAYS

By MAURICE SISKEL, JR., University of Nebraska

Investigators using straight alley runways and similar apparatus in animal research often note that their *S*s are skillful in eluding various recording devices. Photoelectric and weight-actuated switches have been extensively used to obtain data on latency and running speed, and recently electronic contact-switches have been used with moderate success. Most of these devices, however, are not entirely reliable and their installation in a runway often stimulates undesirable behavior by the *S*s.

The capacity-operated device described here is not only reliable, but it is superior to those previously used in that it requires the installation of only one object within the runway, *i.e.* a moving rat. All inside surfaces of the runway remain intact and a major source of potential stimulation for behavior which competes with the running response is eliminated.

The circuit shown in Fig. 1 is the author's modification of a known two-stage electronic circuit.[1] The first stage is a simple radio frequency oscillator (V 1) whose action depends heavily upon the capacitance between the antenna and ground. The output of the screen grid of the oscillator is rectified, filtered, and fed into the control grid of a gas-filled thyratron (V 2) which acts as an amplifier or control tube for the relay (RY). When V 1 is oscillating, a negative DC-bias is applied to the grid of V 2. This prevents current from flowing between the plate and the cathode of V 2 and no current is available to energize RY. When the output of V 1 diminishes sufficiently, there is, however, no longer sufficient grid bias on V 2 to prevent it from igniting and hence energizing RY. The relay will then remain energized until sufficient grid bias is re-applied to V 2.

The sensitive elements of this circuit which the experimenter (*E*) ordinarily desires to manipulate are the position of the antenna and the setting of C2. The antenna is simply a 5-in. square metal plate (dimensions and shape may be grossly altered if desired) with a wire lead attached as shown in Fig. 1. For initial testing, the unit should be placed on a clear bench space with the antenna plate placed two or three feet away from the unit and from E. Then C2 is tuned grossly until RY is heard to operate.

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[1] Frank Shepard, Jr., Two practical capacity-operated relays, *Electronics*, 10, 1937, (April) 35.

Hereafter, only very small changes should be made in the setting of C2 and after each setting, *E*'s hand should be drawn away from the unit. This procedure is repeated until it is found that RY closes as soon as *E*'s hand approaches or touches C2 and that RY opens when the hand is withdrawn. This kind of relay operation will now similarly occur when the antenna plate is approached. The sensitivity of the antenna, *i.e.* how close to the antenna a hand or body must be to energize RY, may be varied continuously (by changes in the setting of C2) between requiring actual finger contact with the antenna plate to 'sensing' a fist extended about 4 ft. from the plate.

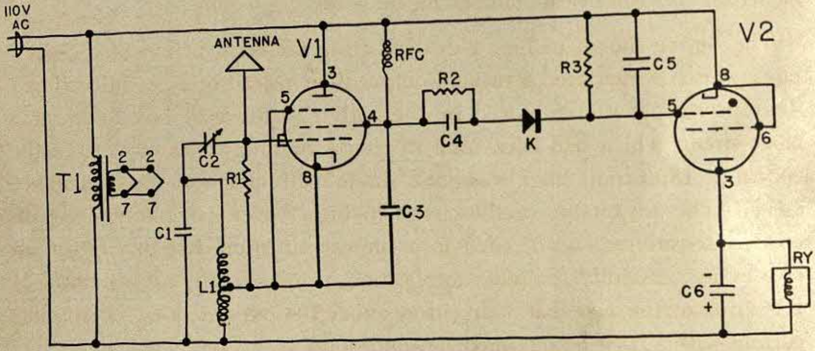

FIG. 1. CIRCUIT DIAGRAM OF THE CAPACITY-OPERATED SWITCH
(All resistors are 0.5 w. and all capacitors are 600 v.)

C 1 = 50 mmf.
C 2 = 4.5 — 100 mmf. (Hammarlund MAPC — 100)
C 3 = 0.001 mmf
C 4 = 0.1 mmf
C 5 = 0.01 mmf.
C 6 = 0.5 to 8 mfd. as needed to stop relay chatter.
L 1 = 100 turns, CT, #18 wire on 1.5 in. diam. form.
K = Crystal Diode, 1N38

R 1 = 20 megohm
R 2 = 1 megohm
R 3 = 2.2 megohm
RFC = 8 Mh., 20 Ma.
RY = Relay, 110 v. (Potter & Brumfield KA11AY)
T-1 = pri. 110 v.; sec. 6.3 v., 1.2 amp. (Stancor P — 6134)
V 1 = 6J7
V 2 = 2050

E usually desires two performance measures from *S*s in a runway—latency and running time. It is suggested that an ordinary mechanical switch and two capacitance switches be employed to obtain such records. The mechanical switch may be operated by the door of the starting-box to start a clock which records latencies. The first capacitance switch may be placed wherever desired to stop the latentcy-clock and simultaneously to start a clock which records running time. The second capacitance switch may be placed near the goal box and is used to stop the clock that times running. More specifically, the antenna plates of the capacitance switches are used at the desired recording points by simply placing them underneath the runway floor, leaning them against the outside of the runway, or laying them on top of the runway. After placing the antenna plates, crude tuning of the switches may be accomplished by *E* using his fist as a substitute for the body of a rat. Final adjustments should then be made by using a test-animal under the same conditions that are anticipated during experimentation. Final adjustments will depend upon such factors as the cross-section size of the runway, the proximity of the antenna plates to other wiring, metal, and other conducting objects, and how closely *E* must pass near the antenna plates in order to remove *S* from the apparatus. Regarding the last consideration, much difficulty may be eliminated by so arranging the clock relay-circuits that the second capacitance switch turns off the entire recording circuit.

The device shown in Fig. 1 evolved from the construction of a capacitance switch which used a vacuum output tube instead of a gas-filled tube.[2] This prototype was employed by the author at the goal box to break a clock-circuit which had been used to record running times of rats. In the course of 1800 trials there was not a single malfunction of the capacitance switch. The advantages of the circuit in Fig. 1 over its prototype are increased sensitivity (not needed in a runway situation, but this factor increases its versatility for other applications) and all-or-none operation of RY (due to the fact that a thyratron either conducts or does not conduct current with almost no intermediate condition).

Many variations of the circuit in Fig. 1 are possible and, in specific applications, may even be desirable. For example, if the switch is to be located more than a few feet from the antenna plate, it would be best to increase the number of turns of wire on L1 in order to lower the operating frequency of V1. This change will reduce losses by radiation from the antenna lead and tend to maintain the effectiveness of the switch.

The present circuit should prove useful in many researches besides the one indicated above. It may be useful to mark an animal licking a fluid reward in a runway or Skinner box. By placing a small antenna in contact with the fluid and reducing the sensitivity of the switch to a degree requiring body contact to close RY, tongue contacts with the fluid will then operate the switch. This arrangement has the advantage over some other types of electronic recorders of precluding any polarization of the fluid being ingested.

Exploratory behavior may be recorded by connecting a few antenna plates together and placing these plates in different locations under the space which *S* is to be allowed to run. This arrangement would allow relative measures of total movement, but would give no indication of specific locations of *S* at any given moment.

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[2] Shepard, *op. cit.*, 35.

Another variation now being developed by the author is the application of the present circuit to the problem of obtaining a continuous measure of the volume of fluid consumed by animals in a free ingestion situation. This would eliminate the need for the relatively more complicated float and lever systems now used in drinkometers.

In human research, the present switch might be useful in situations where it is important to minimize the effort involved in *S*'s response. This may be accomplished by reducing the sensitivity of the unit so that *S* need only touch a small metal plate to operate the switch.

The above applications of the capacitance switch are only a few that may be made. It is believed that the introduction of this device may prove quite helpful in other psychological researches. Electronic instrumentation is widely recognized as being more reliable than equivalent mechanical systems and the capacity-operated switch shown here is both reliable and relatively simple to construct.

A CHEST-RESPIRATOR FOR SMALL ANIMALS

By Allan H. Frey, University of Illinois

Devices to maintain respiration artificially in small animals have been used rather extensively in psychology. These devices have, in general, been of two kinds. The first kind involves cannulation and is generally used with acute preparations.[1] The second, respirators of the iron-lung type, require the animal to be tightly bound.[2]

The writer has developed a third kind, a chest-respirator, which he believes has the following advantages over the earlier types: first, an operation is not required, as with the first type; secondly, it can be applied to the animal without an anesthetic; thirdly, it is very light and can be worn by control as well as experimental animals with little restriction of their movement; and fourthly, it is simple to construct.

This apparatus was devised for rats, but the writer believes that it may easily be modified for use with other small animals. Its principal components are an air-source, mercury manometer, and cellulose acetate chamber. The air-source (A) is a flexible bellows. This is made of spring reinforced, neoprene coated, fiberglass fabric, and is 6 in. in diameter and 3½ in. high. Both ends are sealed by endplates, the bottom one with an exit air-tube. A bar is connected by a pivot joint to the top of the bellows and is driven by a Bodine 1/150 hp., 60 r.p.m. motor (B). This induced respiration rate of 60 breaths per min. does not differ significantly, as judged by the rat's health, from the normal resting rate of the common laboratory rat of 85.5 breaths per min.[3] The tube leading from the bellows is tygon, a clear plastic with negligible yield to air pressure. This leads to a glass Y-tube (E), one branch of which has a short piece of tygon attached. On this tygon is a hosecock clamp which regulates the amount of air released. The air pressure can be adjusted more accurately here than at the bellows. The other branch of E leads to a second Y-tube (F), one arm of which is attached to a mercury manometer (C) by tygon with a hosecock clamp between it and the manometer. Thus, the manometer can be cut in and out of the system as desired. The second arm of this Y-tube leads to the chest-chamber (D).

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[1] Matthew Wayner, An inexpensive respirator for use with small animals, this Journal, 70, 1957, 457 f; J. Q. Griffith and Edward Farris, *The Rat in Laboratory Investigation,* 1942, 384 f.

[2] R. A. McCleary, Personal communication.

[3] D. S. Dittmer and R. M. Grebe, *Handbook of Respiration,* 1958, 41.

The chamber, in principle and construction, is more like an iron-lung than a sphygmomanometer cuff. The writer has found that a cuff-like device applying pressure to the abdomen and lower chest of an animal is not as efficient as the chamber described below.

The chamber (D) is a very light (1¼ oz.), rigid cylinder with the open ends tapered to make nearly airtight seals with the animal's body. It encloses the body between the fore and hind legs. The air-source tube enters the chamber where the animal's abdomen is located. The chamber is constructed by first cutting a sheet of 3/100-in. thick cellulose acetate into a multiple W-shape (see Fig. 2). Edges R and R′ are brought together to form a cylinder, and the joint is sealed with cellophane tape. Each pair of S–S′ edges is then brought together and taped. This operation forms a cylinder with tapered open ends. The amount of taper is a function of the animal's weight.

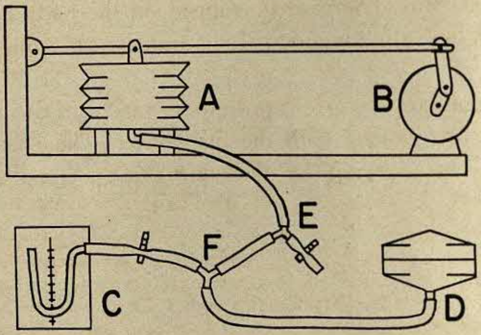

FIG. 1. CHEST-RESPIRATOR FOR SMALL ANIMALS
A = bellows; B = motor; C = manometer; D = chest-chamber; E and F = Y-tubes.

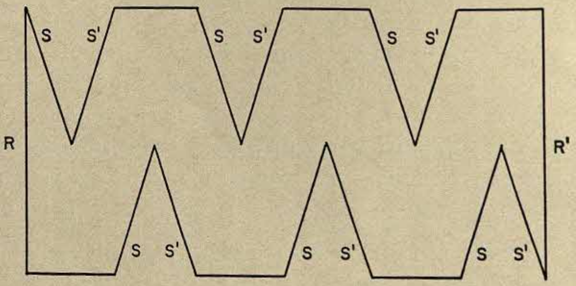

FIG. 2. TEMPLATE OF CHEST-CHAMBER

The chamber is permanently formed by placing it in an oven at 90°C. for 15 min. Since the cylinder may collapse when heated, it is necessary to support it while in the oven. The cylinder is inserted vertically into a beaker of such size that the widest part of the cylinder will not pass through the rim of the beaker. In this way, the cylinder is supported by the rim of the beaker and does not collapse when heated. A hole in the chamber for the air-source tube is made by the heat of a match. The tygon tube is then placed in the hole and cemented in place.

When put in use, the chamber is split longitudinally by cutting along the R–R′ joint, spread somewhat, slipped on the body between the fore and hind legs, and the longitudinal slit sealed with a piece of cellophane tape.

Pressure-values are not given, since these vary with the animal's weight, but are easily determined with the manometer. The effectiveness of the respirator may be evaluated by observing *EEG* potentials or blood pressure.[4]

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[4] F. E. D'Amour and F. R. Blood, *Manual for Laboratory Work in Mammalian Physiology,* 1948, Experiment 10. A commercial device for measuring blood pressure can be obtained from Medical-Electronics Development Co., 400 Northern Blvd., Great Neck, N.Y.

APPARATUS NOTES

A NEW DEVICE FOR THE STUDY OF KINESTHETIC AFTER-EFFECTS

Previous workers have used a tapered comparison-scale to measure kinesthetic after-effects. These instruments were tapered either continuously[1] or in a series of discrete steps.[2] Measurements with the former type of instrument may be inaccurate, since one cannot be sure that thumb and index finger are placed opposite one another on the scale, and *E* has some discretion about which part of the finger-widths he selects for readings. The latter type of instrument restricts the size of interval

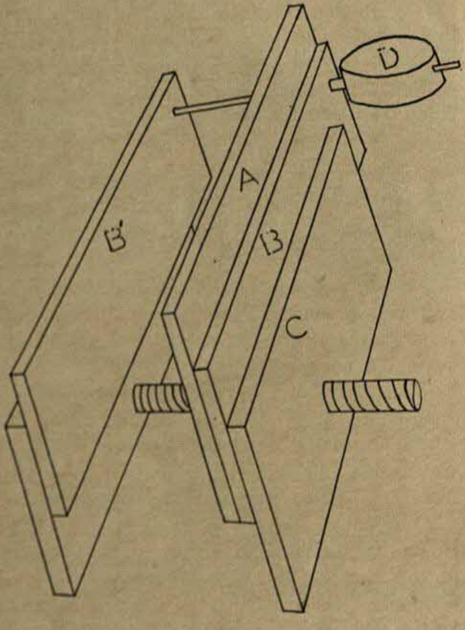

FIG. 1.—DIAGRAM OF THE APPARATUS
A, rider-mounting; *B*, *B'*, variable test-strips; *C*,*C'*, jaws of vise; *D*, dial-gauge.

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[1] G. S. Klein and David Krech, Cortical conductivity in the brain-injured, *J. Personal.*, 21, 1952, 118-48; H. J. Eysenck, Cortical inhibition, figural after-effect and theory of personality, *J. abnorm. soc. Psychol.*, 51, 1955, 94-106.

[2] Wolfgang Köhler and Hans Wallach, Figural after-effects; an investigation of visual processes, *Proc. Amer. phil. Soc.*, 88, 1944, 269-357; Michael Wertheimer, Constant errors in the measurement of figural after-effects, this JOURNAL, 67, 1954, 543-66.

which *S* is allowed to discriminate. For a recent experiment,[3] the writers designed a new piece of equipment free from these limitations.

The device is constructed from a woodworker's vise, a few pieces of smoothed hardwood, and a dial-gauge. The instrument is illustrated diagrammatically in Fig. 1.

A rider (omitted from the diagram) is attached to the section marked *A*, to guide the fingers. *B* and *B′* are two strips of hardwood which are bolted to opposite jaws of the vise. *S* places his thumb at *B* and his index finger on the outside of the strip marked *B′*. The sections *B′* and *C′* can be moved by means of the worm-screw of the vise. As they move, the spring-loaded lever of the dial-gauge, *D*, is displaced by amounts which are registered on its face. A dial-gauge, the lever of which can travel 2 in., was found to be adequate to cover the wide range of individual differences encountered. Gauges with shorter travel can be obtained, and wider gaps between the jaws of the vise can be dealt with by extending the length of the anvil at the end of the lever. The units in which these gauges measure are very fine. Our instrument measured in thousandths of an inch, and we found that our *S*s easily could discriminate 0.04 in. after only a little practice.

Queen's University of Belfast

ROBERT S. RODGER
PETER MCEWEN

A PORTABLE MODIFICATION OF THE WISCONSIN GENERAL-TEST APPARATUS

The apparatus described here was devised originally for use in a comparative study of visual perception in monkeys.[1] Because we wished to duplicate with children the procedures employed with animals, the Wisconsin General-Test Apparatus for monkeys[2] was modified as reported here.

Experience with the device has shown that it is adaptable to other experimental situations and demonstrations in which it is desirable to restrict *S*'s view of *E*. Inasmuch as it can be used in any room which can be darkened, the apparatus affords a simple, inexpensive means of employing a one-way visual screen. Portability proved to be a particular asset when young children were tested because they could be studied in familiar surroundings.

Description. Plywood of ½-in. thickness was used in construction of the apparatus. It is H-shaped with the cross bar containing a hinged door in which a transparent, one-way mirror is installed.[3] Four parallel plywood panels are attached to the center section and form enclosures in front of and behind the one-way screen. As shown in Fig. 1, one enclosure accommodates the *S*, the other, the *E*.

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[3] Peter McEwen and R. S. Rodger, Some individual differences in figural after-effects, *Brit. J. Psychol.*, in press.

[1] This study is being conducted under reasearch grant M-1215 (C2) from the National Institute of Mental Health, U.S. Public Health Service.

[2] H. F. Harlow and J. A. Bromer, A test-apparatus for monkeys, *Psychol. Record*, 2, 1938, 434-436.

[3] Cost and weight of the equipment can be reduced by substituting for the mirror a piece of dark gray theatrical gelatin mounted between two pieces of 1/16-in. Plexiglass.

The center panel is slotted at the bottom to allow passage of a sliding tray which holds the stimulus-objects. Side panels are attached to the center section with piano hinges and can be folded easily. They are secured with leather straps when the equipment is transported.

A shaded light fixture containing two 40-w. showcase bulbs is mounted above the center panel on *S*'s side. Extension of this fixture above the panel by about 3 in. safeguards against breakage in movement. Fifteen feet of light duty extension cord with a line switch attached allows use of a conventional electrical outlet.

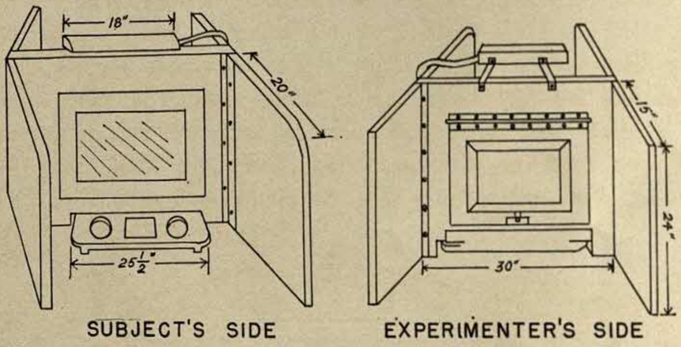

FIG. 1. FRONT AND REAR VIEWS OF THE APPARATUS

Howard University

VERA D. HUNTON
LESLIE H. HICKS

NOTES AND DISCUSSIONS

A WORKING MODEL OF THE COCHLEA

Simple anatomical dissection reveals that the cochlea is a hydraulic (*i.e.* liquid-filled) engine and that its phragma is leather-like.[1] This determines the specifications for a working model. Strangely, during the last 60 yr., many authors have used an elastic rubber membrane for the "häutige Scheidewand," of Helmholtz, not an animal tissue. The choice of a substance so much more elastic than animal tissue for this purpose is dubious. I have used, therefore, for the models I have constructed, from 1927 to the present, a tanned chamois skin. As pictures of my model, previously published,[2] do not give details of the design, I give them here to aid anyone wishing to build a working model of the cochlea.

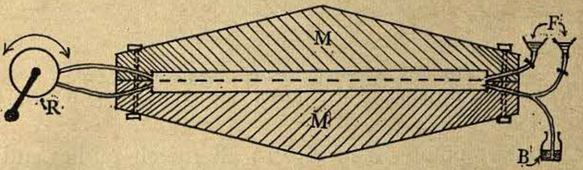

FIG. 1. LONGITUDINAL SKETCH OF THE MODEL
B = siphon-bottle; F = funnels; M = metal casting; R = rotary pump

Fig. 1 is a longitudinal sketch of the model; Fig. 2 a cross-section. Its transparent windows are 24 in. long and 1 in. high, being illuminated by a fluorescent tube placed on the side opposite to the one from which *E* observes the movements and positions of the various parts of the phragma.

The model is built as follows. Two identical metal pieces (28 in. long) are cast, as shown in Figs. 1–2. Into two grooves or channels cast into each part are cemented transparent, plastic strips, each 24 in. long, 1 in. wide, and ¼ in. thick. After having each of the two composing pieces (plastic plus metal) smoothed in a planer, a long, faultless strip of chamois skin was placed between them and the halves of the model were firmly bolted together (see Figs. 1 and 2). The leather forms a sort of gasket, dividing the chambers of the apparatus. When the two parts are firmly bolted together, there is little or no leakage when the chambers are filled with a liquid.[3] (As I have found, the best liquid to use is castor oil.) A small rotary pump (R in Fig. 1) alternately pushes and pulls the liquid from the two tube-like chambers formed by the leather 'gasket' (Fig. 1, R).

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[1] Helmholtz (*Tonempfindungen,* 5 ed., 1895, 223, 250) described it as follows: "Häutige Scheidewand ist nicht ein einfache Membran [sondern] teils knöchern, teils häutig," that is, "in part bony, in part leathery." He nowhere described it as rubber-like. In constructing my model I followed Helmholtz's prescription as closely as possible, and I made it conveniently large that movement within it might be observed without the aid of a microscope.

[2] M. F. Meyer, The hydraulic principles governing the function of the cochlea. *J. gen. Psychol.,* 1, 1928, 261 f.

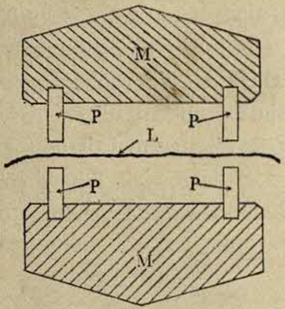

FIG. 2. CROSS-SECTION OF THE MODEL
L = leather membrane; M = metal casting; P = plastic strip

Let us now compare this engine with the anatomically existing machinery. Fig. 3 is a schematic sketch of the anatomical labyrinth. The figure explains itself. The dotted line indicates a thin membrane (irrelevant for the hydraulic function) that divides what in the figure appears as a single sack into two cavities (of which the anatomists call only the right half 'sacculus' and the left half 'utriculus'). Since the division into two half-cavities does not interest us hydraulically, the word 'utriculus' is omitted from the figure. It should be carefully noted by the physicist, who knows mechanics, that the 'round window' (R) is a part and parcel of the cochlea, whereas the 'oval window' (O) lies at a distance from the cochlea; the sacculus intervenes between the two windows. See below that the sacculus thus can act as 'a sort of hydraulic ram.'

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[3] Although there is not true leakage, one must remember that leather, even when compressed, is porous. The builder of this model will notice that after a few weeks the capillary suction of the leather brings out drops of the liquid which will then have to be wiped off to keep the outside of the instrument dry. This nuisance, however, can be entirely overcome by holding the liquid within the model under a slight negative pressure. A small rubber tube inserted in a hole drilled to the inside chamber and leading downwards to a bottle will serve as a siphon. The lower the bottle, the greater the suction. If the bottle is properly placed, the suction of the siphon will balance that of the capillaries and the outside of the instrument will remain perfectly dry.

An air-wave or air-puff put into the middle ear reaches the oval window sooner than the round window, hence within the incompressible, internal liquid there is an instantaneous hydraulic[4] phase-difference of pressure between the windows.[5] We owe much to the otologist Bárány for having clarified this matter. In a certain disease condition, the oval window hardens, so that there can no longer be a hydraulic phase-difference. The surgeon then cuts (operation called 'fenestration') an artificial window in one of the semicircular canals. He covers it with a flap of soft tissue. The hydraulic pressure-difference is then again possible. (The utriculus and the semicircular canals have sensitive cells that serve 'the sense of position' and not hearing.) On the other hand, from the sacculus has grown out in evolution the lagena of the birds and the cochlea of the mammals.[6] There can be little doubt that the proper sacculus and the cochlea are both serving the sense of hearing, with some division of labor as to the very high frequencies and the lower and musical frequencies.[7]

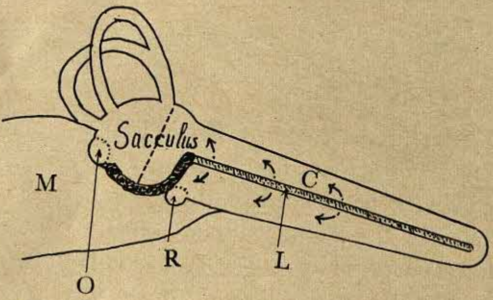

FIG. 3. SCHEMATIC DIAGRAM OF THE INNER EAR
C = cochlea, straightened out; L = leather-like phragma; M = middle ear; O = oval window; R = round window

The hydraulic phase-difference between the windows may be brought about also in another way than by introducing air-puffs into the middle ear. The other way is by shaking the whole skull. Then the liquid in the sacculus, *because of inertia,* does not strictly follow the bony cavity walls of the skull but swings between the two windows.[8] It is thus a sort of hydraulic ram. The traditional phrase referring to this way of audition, brought about by 'shaking the skull,' is "hearing through bone conduction." This phrase is not very happily invented. It is illusory. Hearing through shaking the skull is more realistic.

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[4] The correct etymology of the term "hydr-aulic" is often forgotten. It is confused with the far more general term "hydrodynamic." Hydraulic means "liquid in tubing" and is properly used only when *pressure* in liquid brings about motion.

[5] Meyer, Phase difference of pressure between the windows, the essence of sound stimulation, *Ann. Otol. Rhin. Lar.*, 41, 1932, 323-331.

[6] Meyer, The hydraulic theory of the cochlea and comparative anatomy, this JOURNAL, 65, 1952, 288-293.

[7] Meyer, Neurological theory of beat tones, *J. acoust, Soc. Amer.*, 28, 1956, 877. Footnote 3.

I never speak of "the basilar membrane," although many physiologists are in love with that term. When the phragma of the cochlea, hydraulically pressed, bulges in one way or the opposite way, it bulges totally with all the layers composing it, that is, from the delicate membrane of Reissner on one side to the vascular layer covering the opposite side of the tough membrane. I do not understand how anyone has ever conceived of the basilar membrane bulging alone while all the other layers resting on it could be standing still.

To every reversal of the *curve* of physical stimulation corresponds a momentary resting position of (a piece of) the cochlear phragma. Its extension lengthwise is determined by the *size* of a positive or negative stroke as indicated by the ordinate distance from maximum to minimum or minimum to maximum of that curve. Whether there are inflections between a maximum and minimum is of minor importance and may usually be neglected.[9]

Of physiological importance is the fact that the *sensitive cells,* bringing about pitch-sensations through mediation of the cochlea (*i.e.* cells not in the sacculus), are imbedded within the phragma. What happens to these cells *electro-chemically* is outside the scope of demonstrating the moving liquid by the aid of a hydraulic model of the cochlea and its phragma. Hydraulics is 'mechanics.'

Miami, Florida MAX F. MEYER

THE EFFECT OF ORIENTATION ON THE REVERSAL OF ONE CUBE INSCRIBED IN ANOTHER

In a previous experiment, it was found that contiguous or spatially separated cube-figures of the same size tend to reverse simultaneously.[1] The experimental variations introduced in this experiment focused on the role of various organizational relationships between the cubes. In the present experiment, we studied the role of spatial orientation on the reversals of one cube inscribed in a larger cube.

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[8] Meyer, *How We Hear,* 1950, Chap. I and V.

[9] *Idem,* p. 50.

[1] P. A. Adams and Mason Haire, Structural and conceptual factors in the perception of double-cube figures, this JOURNAL, 71, 1958, 548-556.

Procedure. Two ambiguous cubes were drawn in such a way that the smaller inscribed cube lay entirely within the contours of the larger figure. Typical drawings are shown in Fig. 1. All figures were of the same size, one side of the larger cube being 3 in., of the smaller cube, 1 in. Six figures were studied. Half of the figures had the two cubes with similar orientation, as shown in Fig. 1, to the right, and half had cubes with the opposite orientation. Of the three figures with each orientation, the 'A-version' had the inscribed cube equidistant from the immediately surrounding edges of the circumscribed cube. In the 'B-version' the inscribed cube was equidistant from two parallel sides but moved up ¼ in. closer to one of the other sides. In the 'C-version' the outer edges of the inscribed cube were ¼ in. from one side and 5/32 in. from the nearest perpendicular line of the outer cube. In the A-version of the figure with similar orientation the diagonal lines of the major axes of the two cubes are thus continuous, and the two cubes are symmetrical with respect to all four sides of the circumscribed figure. Symmetry with respect to two sides of the surrounding figure occurs with the 'B-version.' In the C-figure the major axes are not colinear nor are the cubes in a symmetrical relation. The importance of symmetry in the reversal-behavior of the two cubes is tested by the comparison of the results with the different versions.

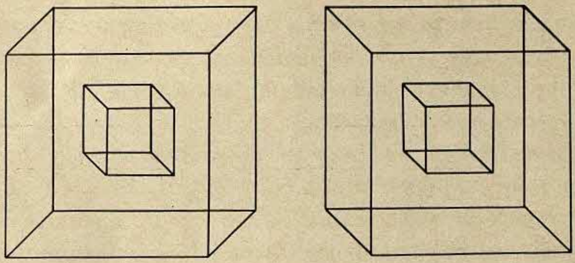

FIG. 1. SAMPLE OF THE CUBE-FIGURES IN OPPOSITE AND SIMILAR ORIENTATIONS. 'A-version'; inscribed cube equidistant from the immediately surrounding edges of the circumscribed cube.

The *O*s were instructed to observe both cubes at the same time and, when the rate of reversal had become stable, to describe the relative rates and any correspondence in the reversal of one with respect to the other. Each *O* observed from one to three figures. When an *O* observed more than one figure, figures in both orientations were used. The stimulus-cards were presented, to different *O*s, rotated on each of their four sides. A total of 20 figures was observed. Thirty-six men and women students at the University of California served as the *O*s.

Results. The *O*s' observations on the relative rates of reversal are tabulated in Table I. In no case was the rate of reversal of the inscribed cube slower. The cubes with similar orientation generally reversed at the same rate, and in all 40 cases when the rate was the same, the *O*s also reported that the cubes appeared to be in the same position at any given time. In contrast, significantly more than half the *O*s observing the figures with opposite orientation reported that the smaller cube reversed at a *faster* rate than the larger cube.

TABLE I

RELATIVE RATES OF REVERSAL OF TWO CUBES, ONE WITHIN THE OTHER. NUMBERS ARE *O*S REPORTING EACH RATE.

Version	Similar orientation		Opposite orientation	
	rate same	inscribed faster	rate same	inscribed faster
A	22	0	2	20
B	10	0	3	7
C	8	2	2	8

The differences among the three versions differing in symmetry were not significant. The faster rate of reversal for the smaller of the two cubes can be attributed to the difference in the sizes of the two figures or to the figure current distribution associated with the two types of figures. Earlier studies on the effect of size have given divergent results.[2] According to the theory of figural after-effects the reversal of an ambiguous figure is due to the satiation of the cortical processes associated with each appearance of the figure. Such satiation might be greater for a smaller figure, or a faster rate might be attributed to greater satiation in the interior of the larger figure. In the experiment reported, however, size is not the only factor. Differences in rate occur only when the cubes have opposite orientations. When the two cubes have parallel axes and are perceived in the same way, the figure-currents associated with each cube would be similar and would reinforce each other. Similar modes of appearance would be simultaneously satiated. When the two cubes have opposite orientations the interaction of the figure currents associated with each cube would be less.

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[2] I. E. Ash, Fatigue and its effects upon control, *Arch. Psychol.*, 4, 1914, (No. 31) 1-61; M. F. Washburn, H. Mallay, and A. Naylor, The influence of size of an outline on the fluctuations of its perspective, this JOURNAL, 43, 1931, 484-489; E. D. Sisson, Eye-movements and the Schröder stair-figure, this JOURNAL, 47, 1935, 309-311.

Menlo Park, California PAULINE AUSTIN ADAMS
University of California MASON HAIRE

CONTRAST BY CONTOUR-ENHANCEMENT

Mach originally proposed a process of contour-enhancement as an explanation of the anomalous appearance of Mach rings.[1] He realized the relation of such a process to subjective contrast. While more recent writers have acknowledged the importance of the edge or contour-region in contrast,[2] little work has been done on evaluating the role of contour-enhancement. Under simple assumptions, a recent attempt was made to estimate the contour-enhancement on certain simple distributions of intensity.[3] When the subjective perceptions of these distributions were compared with the enhanced distributions, a strong drop in contrast was noted for situations with little enhancement.

The same procedure of assuming a Gaussian filter-function to calculate the retinal distribution of intensity,[4] and then graphically determining a contour-enhancement proportional to second differences of this distribution,[5] is applied to the Wertheimer figure (see A, Fig. 1). Since the subjective contrast observed in the two identical small triangles is denied by most theories of contrast, it offers a likely target for testing the hypothesis. Although the calculated enhancement is identical along most of the

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* This work, done in the Applied Physics Laboratory of The Johns Hopkins University, was supported by the Bureau of Ordnance, Department of the Navy, under Contract NORD 7386.

[1] Ernst Mach, Ueber die Wirkung der raumlichen Vertheilung des Lichtreizes auf die Netzhaut, *Math, u. naturw. Kl.*, 52, 1865, 303; Ueber den physiologischen Effect raumlich vertheilter Lichtreize, *ibid.*, 54, 1866, 131.

[2] S. L. Lamar, Selig Hecht, Simon Shlaer, and C. D. Hendley, Size, shape, and contrast in detection of targets by daylight vision, *J. opt. Soc. Amer.*, 37, 1947, 531-545.

[3] Vivian O'Brien, Contour perception, illusion, and reality, *J. opt. Soc. Amer.*, 48, 1958, 112-119.

[4] Charles Lapicque, L'optique de l'oeil et la vision des contours, Thesis, Paris, 1938; G. M. Byram, The physical and photochemical basis of visual resolving power: I. The distribution of illumination in retinal images, *J. opt. Soc. Amer.*, 34, 1944, 571-591.

[5] R. N. Bracewell, Simple graphical method of correcting for instrumental broadening, *J. opt. Soc. Amer.*, 45, 1955, 873-876.

perimeter of the two small triangles, there are significant differences in the region of the acute angles. In fact the sign and intensity of the enhancements are just about opposite for the two triangles in those regions, as one has a net positive enhancement, the other a net negative enhancement. The sign of the net enhancement in these regions corresponds to the subjective contrast perceived when observing these triangles, the positive enhancement to the brighter triangle and the negative enhancement to the one less bright. The importance of these corner-regions is also supported by the change in contrast observed when the small triangles are so displaced that their edges are no longer aligned with the background figure.

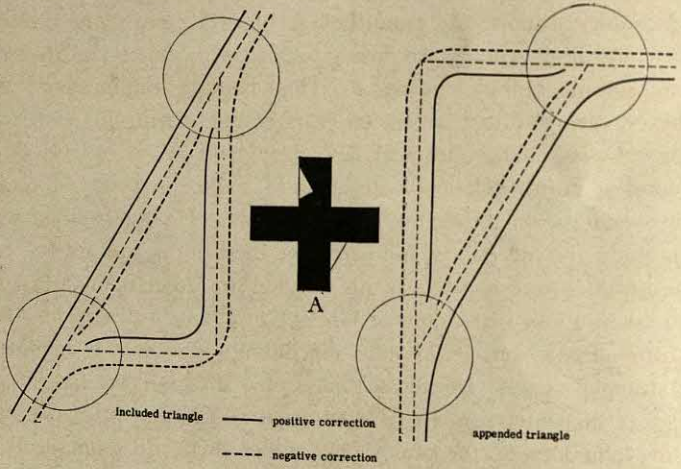

FIG. 1. CONTOURS OF MAXIMAL ENHANCEMENT FOR WERTHEIMER'S TRIANGLES

Benary made a number of experiments with various versions of the Wertheimer figure,[6] and his results correlate quite well with estimated contour-enhancements. For example, the inability to adjust the background-figure to negate the contrast in some cases is tied to the sign of the enhancement. Further, the fact that there is a critical size of the background-figure indicates that, under the assumption of a second difference-correction, an indirect measurement of retinal receptor-spacing may be possible.

The Johns Hopkins University VIVIAN O'BRIEN

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[6] Wilhelm Benary, Beobachtungen zu einem Experiment über Helligkeitskontrast, *Psychol. Forsch*, 5, 1924, 131-142.

John Broadus Watson: 1878-1958

The founder of Behaviorism, who died in Woodbury, Connecticut, on September 25, 1958, was born on January 9, 1878, the second of five children of a well-to-do farmer living near Greenville, South Carolina. He learned in boyhood to take part in farm work, including carpentry which remained for him a life-long hobby. From the age of six years, he attended country schools, and from the age of twelve years, the Greenville grammar and high schools. According to his own account (in his very interesting autobiography),[1] he was far from being a studious boy; yet we find him entering college at sixteen years of age, paying his own way in part by assisting in the chemical laboratory; and, when he had finished his studies at Furman College with an A.M. degree in 1900, we find him eagerly seeking a university for graduate study. (His mother had wished him to study for the Baptist ministry. She died during his college course and he became quite negative in his attitude toward religion.) In college he had taken a full curriculum of Greek, Latin, and mathematics, though with little satisfaction. Philosophy (including psychology) had made a much stronger appeal. He chose philosophy, therefore, as his field and Chicago as his university. He was attracted to Chicago primarily by the fame of John Dewey, the philosopher. Once there, he soon made close contact with other members of the department, including James R. Angell, the psychologist, whose "erudition, quickness of thought, and facility with words" won him over to a major in experimental psychology, with minors in philosophy and neurology. In H. H. Donaldson's neurological laboratory he made the acquaintance of the white rat and discovered that he liked rats better than human 'observers' as experimental subjects. His doctoral dissertation combined neurological and behavioral studies of the development of the white rat. After three years of strenuous work, and a near-breakdown from overwork, he obtained the Ph.D. degree in 1903. Watson remained at Chicago five more years as an assistant and instructor in experimental psychology. He taught Titchenerian experiments in the main laboratory, meanwhile constructing an animal laboratory in the basement where he carried on his own very active research. He greatly enjoyed working with rats and monkeys, but he became increasingly impatient with the current human psychology—with its introspective reports, the "stuffy, artificial instructions given to subjects," and the "intangible, imponderable" concepts of William James. He married Mary Ickes in 1904. In 1908 he was offered an assistant professorship at Chicago, but a full professorship at the Johns Hopkins University, where, from 1908 to 1920, he was Professor of Experimental and Comparative Psychology and Director of the Psychological Laboratory. At Johns Hopkins he threw himself with great energy and success into the work of developing and equipping an animal laboratory, later extending his behavioral researches to the study of young children. His university work was suspended for a year in 1917-18, while he served as Major in the Signal Corps, Aviation Section of the U.S. Army.

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[1] J. B. Watson, Autobiography, in *A History of Psychology in Autobiography*, Clark University Press, III, 1936, 271-281.

In 1920, misfortune overtook Watson in the shape of a sensationally publicized divorce suit. Being asked to resign from Johns Hopkins, he sought business opportunities in New York. He "knew nothing of life outside the walls of a university," but, with characteristic enterprise and willingness to work, he soon made contacts with business men and in 1921 became a staff member of the J. Walter Thompson Company, specialists in advertising and selling. He was advanced to a Vice President of that company in 1924 and remained there until 1936, when he became Vice President of William Esty and Company, where he remained until his retirement in 1946. He found "that it can be just as thrilling to watch the growth of a sales curve of a new product as to watch the learning curve of animals or men." Meanwhile, between 1920 and 1930, he gave some very popular lecture courses on Behaviorism at the New School for Social Research and at the Cooper Union, both in New York. These lectures were published in his book, *Behaviorism,* in 1925, revised in 1930.

During his business career in New York, having married Rosalie Rayner in 1920, he bought 80 acres of farm land near Westport, Connecticut, and developed there a well-equipped farm and residence. In the course of time he built a fine barn with his own hands and the help of his three sons. After his wife's death about 1934 and his retirement from business in 1946, he gave up his farm and moved to a much smaller estate farther out in the country. He is survived by his four children—a married daughter living in Connecticut; one son, an oil geologist in Texas; another son, a psychiatrist in New York; and the third son, a personnel psychologist in California—and ten grandchildren.[2]

Watson was president of the American Psychological Association in 1915. He was also a member of the American Physiological Society, and, in 1917, he became a Fellow of the American Academy of Arts and Sciences.

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[2] Thanks are due to William R. Watson, M.D., for interesting family data.

He was editor of the *Journal of Animal Behavior* and of *Behavior Monographs* in 1911–1917, of the *Psychological Review* in 1911–1915, and of the *Journal of Experimental Psychology* in 1916–1926.

Watson's fame as a theorist should not make us forget Watson the experimentalist—the man who equipped two animal laboratories, designed complicated apparatus when necessary, and carried out pioneering experiments on rats, birds, monkeys, and human babies. He attacked such problems as these: behavioral development in relation to neural development in the white rat; sensory cues utilized by the rat in learning and running a maze; the role of kinesthesis in the control of behavior; color vision in different animals; the homing instincts and performances of noddy and sooty terns; and the early emotional development of the human child. Watson as an experimenter was keen, hard-working, persistent, always on the trail of some challenging theoretical conclusion. The homing study of terns was a large-scale enterprise, involving several years of field study during the nesting season of these birds. The study of infantile emotional development was another large-scale enterprise which, however, was broken off by the abrupt ending of his academic career.

WATSON'S BEHAVIORISM

Watson's behaviorism began to germinate during his first year of teaching psychology. He began to wonder whether the introspections of human subjects were really as essential as officially supposed, and whether the fundamentals of psychology might not be even better learned from behavioral experiments on animals. He got little encouragement for such radical heresies from his Chicago colleagues, but at Johns Hopkins he encountered Knight Dunlap whose views were somewhat similar though less extreme. By 1912-13 he was ready to come forth with a visiting-lecture course at Columbia and an article in the *Psychological Review* on "Psychology as a behaviorist views it." In opposition to the traditional, or at least official definition, of psychology as the "science of consciousness," Watson's paper had the boldness of a declaration of independence, and even more. Behavior study was not only to be free from domination by the psychologists of consciousness, but was to take entire possession of the field.

> The time seems to have come when psychology must discard all reference to consciousness; when it need no longer delude itself into thinking that it is making mental states the object of observation. . . . It is possible . . . to define it . . . as the 'science of behavior,' and never . . . to use the terms consciousness, mental states,

> mind, content, will, imagery, and the like. . . . It can be done in terms of stimulus and response, in terms of habit formation, habit integration, and the like.[3]

Of his reasons for discarding the introspective method, the most obvious was its unavailability for the study of animals. Another was the lack of agreement among psychologists who were attempting to use the method in difficult fields like the thought-processes—disagreement as to the supposedly observed facts. A third reason was that introspection was supposed to provide data for a scientific description of the 'content' of consciousness; a perception, for example, was presumed to consist of sensory and imaginal elements, and the problem was one of identifying the elements. Watson wished to work with 'functional' concepts and laws which would enable him to predict and control an individual's behavior.

How does behavior-psychology differ from physiology, since both have to do with objectively observable activities of the organism? Watson met this challenge by pointing out that physiology is concerned with the several bodily functions, such as circulation, digestion, muscular action, and nerve-and-brain action, whereas behavior is the activity of the body as a unit in relation to the environment. This, then, is the true field of psychology—from the behaviorist's standpoint.

Behavior, from Watson's point of view, consists of motor and glandular responses to sensory stimuli; it is always sensorimotor. A stimulus arouses activity in one or more receptors; so nerve impulses are excited in sensory nerves conducting to the brain and spinal cord, where outgoing nerve currents are excited in the motor and glandular nerves, with resulting excitation of muscles and glands. Whatever 'organization' takes place in the brain and cord is mostly beyond our ken at present, and it is not worth while for the psychologist to devise speculative schemes of the brain processes. We can be reasonably sure of two things: that every sensory impulse arouses in the brain a prompt motor impulse, and that every muscular contraction feeds back a kinesthetic sensory impulse to the brain. By such feedback an often-repeated series of movements can be so directly chained together as to run off smoothly without any dependence on external stimuli—an important factor in Watson's theories of motor skill and even of skill in thinking.

Implicit behavior. Much sensorimotor activity can go on inside the body without being observable from outside, unless perhaps by a more sensitive recording apparatus than is yet available. Very slight muscular contractions may excite weak kinesthetic feedback and this may suffice to keep a behavior-chain going forward until finally an overt movement is produced. For instance—the most important instance—a person confronted by a problem may stand apparently motionless for several minutes and finally come forth with a solution expressed in spoken words or executed by overt hand movements. This invisible process of thinking, according to Watson's theory, is just as genuine sensorimotor behavior as if it consisted of walking, manipulating, or talking aloud. It consists of subvocal talking, silently talking to oneself. Fluent speech makes use of kinesthetic feedback, and such skill is carried over to silent speech.

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[3] Watson, *Behavior: An Introduction to Comparative Psychology,* 1914, 7, 9.

> You have been trained both at your mother's knee and in psychological laboratories to say that thinking is something peculiarly incorporeal, something very intangible, very evanescent, something peculiarly mental. . . . The behaviorist advances a natural science theory about thinking which makes it just as simple, and just as much a part of biological processes, as tennis playing. . . . The behaviorist advances the view that what the psychologists have hitherto called thought is in short nothing but talking to ourselves . . . the muscular habits formed in overt speech are responsible for implicit or internal speech (thought) . . . after our overt speech habits are formed, we are constantly talking to ourselves (thought). . . . Soon any, and every bodily response may become a word substitute . . . whenever the individual is thinking, the whole of his bodily organization is at work (implicitly).[4]

Instinct. Watson's views on instinct changed markedly between 1914 and 1925. In his 1914 book, *Behavior,* he made much of the instincts of various animals, defining an instinct as an inherited combination or serial pattern of simple reflexes, with the pattern as well as the component reflexes being native and unlearned in a given species, though usually perfected and extended by learning. In his great 1919 book, *Psychology from the Standpoint of a Behaviorist,* he took up the question of human instincts and insisted that purely unlearned behavior can be observed only in little babies, because of the quick overlay by habit. Starting with the numerous little unlearned movements of the infant, we can trace the development of the complex learned performances which have been carelessly called human instincts. The inborn tendency to manipulate objects, for example, provides the basis of manual and constructive skills.

In his book of 1925, basing his conclusions again on his studies of infants, he came out almost savagely against the notion of human instincts.

> There are then for us no instincts—we no longer need the term in psychology. Everything we have been in the habit of calling an 'instinct' today is a result largely of training—belongs to man's learned behavior. As a corollary from this I wish to draw the conclusion that there is no such thing as an inheritance of capacity, talent, temperament, mental constitution and characteristics. These things depend on training that goes on mainly in the cradle.[5]

He had evidently come to regard the notion of instinct as contaminated with the 'mental,' the "mystical inner life of bents and instincts." Thus he was led to the extreme environmentalist position of asserting that any normal individual, if given the appropriate environment and behavioristic training from birth on, could be bent toward any occupation (by conditioning) and given all the requisite ability.

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[4] Watson, *Behaviorism,* 1925, 191, 192, 213.

[5] Watson, *Behaviorism,* 1925, 74f.

Precursors. There were precursors of behaviorism in Germany, in Russia, in England, and in America. The early German 'objectivists,' students of animal behavior and physiology, had proposed to eliminate all terms such as *seeing* and *desiring* which seemed to imply consciousness in animals. The Russian physiologists hoped to explain all mental operations in physiological terms. Watson quite early got to know something of Pavlov and of Bekhterev and made some use of the conditioned-reflex method for the study of the senses of animals. Later it seemed to him that Pavlov had found the key, in conditioning, for all processes of learning; but by no means did he base his original behaviorism on these leads. The English psychologist, William McDougall, had also refused to define psychology as the science of consciousness, insisting that it must become a science of human conduct or behavior. Several American psychologists had adopted the same definition, or had chosen objective methods for their own experiments, though without seeking to eliminate introspection from all psychology. Many objective experiments had been made elsewhere, as by Helmholtz and by Ebbinghaus.

As Watson observes in the Preface of his 1919 *Psychology,* this preceding work and discussion made no great contribution to behaviorism as such. Watson did not, however, "claim behavior psychology as a creation of his own. It has had a rapid development and is a direct outgrowth of the work on animal behavior. It is purely an American production."

Behaviorism seemed to go into a decline when Watson's vigorous impetus was ended. About 1930, however, several productive younger behaviorists appeared on the scene, none of them agreeing with all of Watson's views but all of them determined to push forward by the use of objective methods and functional concepts. It has sometimes been difficult to say whether behaviorism has absorbed psychology in the United States, or whether psychology has absorbed behaviorism.

Is it still too early in psychological history to appraise the permanent effect of the behaviorism which created such a stir in the early decades of this century? Certainly very little has been heard of *consciousness* in these later decades; Watson's taboo against the word is still potent. Other 'mentalistic' terms which he banished, like *perception,* are coming back as names for important functions of the human and even the animal organism. As to the introspective method, it tends to come back in its simpler form, the *verbal report* of psychophysics and of the study of personality, though little use is being made of its complex form as used in the old studies of imagery and thinking.

Watson's peripheralism—his tendency to locate 'implicit behavior' in the muscles rather than in the brain—has been the guiding principle of the Hullians down to the present time. It is by no means the guiding principle of the modern physiological psychologists. His muscular theory of thinking is still accepted by many psychologists, though regarded as wholly inadequate by many others. His extreme environmentalism still has its stanch adherents but on the whole has given way to a more balanced estimate of the relative importance of heredity and environment in human development and the production of individual differences.

Watson's preference for animal experiments and his reliance on them for investigation of the fundamental laws of learning find plenty of support in recent work. Such laws, it is often felt, should be established in the animal laboratory; otherwise they may only reveal human ingenuity and use of language. Conditioning still plays a big role in learning theory, as it did for Watson, except that the law of effect or reinforcement can no longer be discarded as it was by him. The perceptual factor in learning, also overlooked by Watson, has been coming to the fore in numerous studies of human skills, industrial and military. Instead of speaking of *motor skills,* we now call them *perceptual-motor.*

WATSON'S STATUS IN CONTEMPORARY PSYCHOLOGY

To illustrate Watson's standing in contemporary psychology we could cite a large number of comments dating from the decade of the fifties.

The most laudatory of all is from one who looks at Watson's work from the standpoint of logic and the history of science. Bergmann writes:

> Second only to Freud, though at a rather great distance, John B. Watson is, in my judgment, the most important figure in the history of psychological thought during the first half of the century . . . although the attention he receives now, in the fifties, is perhaps not as great as it was in the twenties and thirties. . . . Among psychologists the sound core of Watson's contribution has been widely accepted; his errors and mistakes have been forgotten. . . . Watson is not only an experimental *psychologist* . . . he is also . . . a systematic thinker, . . . a *methodologist.* In this latter area he made his major contribution.[6]

Quite a different estimate, though from the same general standpoint, is contained in an analytical review of Clark Hull's system:

> The 'classical' behaviorism of Watson . . . was itself little more than a set of orientative attitudes . . . not a single behavioristic author put forward a concrete theory. . . . It was Hull's firm intention to assume intellectual responsibility for developing the orienting commitments of earlier behaviorism into detailed explanatory theory. In this, he was setting himself an impossible goal.[7]

Hull's own reactions to Watson are to be found in his autobiography. At first, while inclined to agree on the futility of introspection, Hull was repelled by Watson's dogmatic claims. Later, after hearing Koffka criticize Watson at great length, he writes:

> I came to the conclusion not that the Gestalt view was sound but rather that Wat-

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[6] Gustav Bergmann, The contribution of John B. Watson, *Psychol. Rev.,* 63, 1956, 265-276.

[7] Sigmund Koch, Clark L. Hull, in W. K. Estes *et al., Modern Learning Theory,* 1954, 5, 163.

> son had not made out as clear a case for behaviorism as the facts warranted. Instead of converting me to *Gestalttheorie,* the result was a belated conversion to a kind of neo-behaviorism—a behaviorism mainly concerned with the determination of the quantitative laws of behavior and their deductive systematization.[8]

Let us see what some other behaviorists, whose autobiographies are included in the same (1952) volume, have to say. Walter S. Hunter writes:

> By 1922 I had come to the belief that behaviorism represented essentially the only adequate scientific point of view in psychology and that some of Watson's pronouncements represented less the essential details of behaviorism than his own prodigious effort to fill in the experimental gaps with hypotheses pending further work. . . . The fundamental issue in behaviorism is not, and never was, the particular speculations of any one behaviorist—of Watson for example.[9]

Edward C. Tolman, known as a 'purposive behaviorist,' relates how in his graduate-student days, he became worried about introspection, then asserted to be the chief method in psychology, though it seemed to be of little use in the laboratory. In his further development, he continues:

> On the one hand I sided with Watson in not liking the Law of Effect. But, on the other hand, I did not like Watson's over-simplified notions of stimulus and response . . . a really useful behaviorism would not be a mere 'muscle-twitchism' such as Watson's. . . . The further notion that purpose and cognition are essential descriptive ingredients of . . . behavior. . . . Today, however, the operational battle has so largely been won that to the average psychologist it no longer seems worth arguing about. In other words, today we are practically all behaviorists.[10]

When Tolman said, "Today we are practically all behaviorists," he was thinking of American psychologists, for of the seven Europeans included in the 1952 *Autobiographies,* only one mentioned Watson or American behaviorism. Albert Michotte van den Berck, early in his career, was a member of Külpe's introspective school and employed his methods. Later, his confidence in these methods was shaken both by Titchener's criticisms and by the contentions of the behaviorists.

> I became convinced that it was a vain task to try to describe internal events and analyze them into elements, and that the only value of introspection must be an *informative* one. . . . General information . . . about the state of mind of the observer, about the way in which he understands the situation . . . and about his own reactions . . . seemed to me indispensable in all psychological experiments. . . . It is to a large extent . . . the meanings that certain objects or actions may have for a man or an animal . . . that have prevented me from subscribing to a radical behaviorism.[11]

All through the years, European psychology has been comparatively little influenced by Watson. If we consult all four volumes of the *Autobiographies* (1930, 1932, 1936, 1952), we find 29 European psychologists narrating their careers. Only 9 of these make any mention of Watson or American behaviorism, and these 9 comments are about equally divided between the favorable and the unfavorable. (The corresponding American count is: no comment, 6; favorable, 11; unfavorable, 11.) A characteristic European comment came from Claparède (1930): "Psychologists have lost much time in these controversies . . . I do not oppose the study of behavior, but simply behaviorism as a dogmatism."

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[8] C. L. Hull, Autobiography, in *A History of Psychology in Autobiography,* Clark Univ. Press, IV, 1952, 153f.

[9] W. S. Hunter, Autobiography, *ibid.,* 172, 186.

[10] *Ibid.,* 326, 329-331.

[11] E. C. Tolman, Autobiography, *ibid.,* 220, 222.

Since an autobiographer usually ranks as an 'elder statesman,' we may do well to quote also from some younger psychologists of the 1950s. So from Kenneth W. Spence:

> Watson . . . proposed to extend the same objective behavioral methods of observation to the study of human behavior. . . . I should like to turn now to an analysis of the general conception of their science that the majority of psychologists in this country hold today. While behavioristic in outlook, this conception is probably better described as objective psychology. As such it does not require adherence to the orthodox doctrines of Watson.[12]

This from a recent book of Skinner's:

> J. B. Watson argued that 'words function in the matter of calling out responses exactly as did the objects for which the words serve as substitutes.' . . . This is a superficial analysis which is much too close to the traditional notion of words 'standing for' things.[13]

Skinner's views are also well represented in this quotation from Keller and Schoenfeld's text:

> John B. Watson, an American pioneer in the objective, natural-science approach to psychology . . . proposed a cataloguing of reflexes as basic to the control of human and infrahuman behavior. . . . Watson's proposal is now deemed impracticable. . . . We need a dynamic, rather than a static, picture of the behavior of organisms. . . . Watson's views were received with hostility by most of his contemporaries because they were radical for that time and because they were not developed to a stage where their fruitfulness could silence criticism. . . . Watson was thus correct in his general approach, though he erred in detail.[14]

Watson's purely 'peripheral' theory of emotion is regarded by Jersild as unfortunate. He writes:

> So many psychologists for so many years were so easily seduced into following the simplified version of emotional development proposed . . . by John B. Watson. . . . A vast amount of research on emotional behavior has suffered from this tendency to pay attention primarily to objective and overt features without proper regard for subjective factors which, from the individual's point of view, are the most crucial feature.[15]

And here are two recent judgments, not entirely contradictory, regarding Watson's contribution to the psychology of thinking:

> We must conclude that the influence of behaviorism on the psychology of thought has been a negative one. Watson showed how the phenomena of thought could be

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[12] K. W. Spence, *Behavior Theory and Conditioning,* 1956, 11, 15.

[13] B. F. Skinner, *Verbal Behavior.* 1957, 86-87.

[14] F. S. Keller, and W. N. Schoenfeld, *Principles of Psychology,* 1950, 7, 379.

[15] A. T. Jersild, Emotional development, in Leonard Carmichael's, (ed.), *Manual of Child Psychology,* 1954, 79, 841.

> rephrased to fit his system of behavior, but he did not show how to do anything about it. His criticism of subjective methodology scared many psychologists away from [research on] thinking. . . . Modern behaviorists, more sophisticated than Watson in some respects, have avoided [research on] thinking.[16]
>
> The motor theory of thought . . . embodies a concept of the greatest importance for understanding behavior. . . . The theory is no longer entertained as a complete explanation . . . but the concepts which it developed concerning sensory feedback remain valid and important.[17]

What shall we say then? Watson's mistakes have evidently not been forgotten; every recent comment takes note of one or more of them. It would not be safe to overlook them unless we were prepared to overlook Watson altogether; and that we are not prepared to do—not in America anyway. It seems that our psychologists have been gravitating together, the behaviorists broadening their coverage and the nonbehaviorists more and more employing functional concepts—which were after all the positive core of Watson's system.

Columbia University ROBERT S. WOODWORTH

AN ACKNOWLEDGMENT

The JOURNAL is indebted to Dr. William R. Watson for the photograph and signature of his father that are reproduced in the frontispiece of this number. The photograph was taken in 1943, when he was 65 years of age; the signature was written shortly before his death. K. M. D.

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[16] D. M. Johnson, *The Psychology of Thought and Judgment*, 1955, 11.
[17] D. O. Hebb, *A Textbook of Psychology*, 1958, 58.

John Gilbert Beebe-Center: 1897-1958

John Gilbert Beebe-Center was born on March 19, 1897, in Boston. He was the son of Cyrus Gilbert Beebe and Janet Ingliss Hogg. Cyrus Beebe was a Boston cotton broker and also President of the National Bank of South Reading (now the Wakefield Trust Company), a bank founded in 1854 and one in which various Beebes held positions of importance. Cyrus died in 1901, when his son, Jack, was only four. Not long after that Jack's mother married Edward Chester Center and Jack added his step-father's name to his own without the hyphen. He was known as a boy, in school in both France and America and in service in World War I, as John Gilbert Beebe Center (thus as "Mr. Center"), but at Harvard he registered in 1915 as "Beebe-Center," so that after the war he began to be indexed with the Bs.

When Jack was still a little boy his mother and step-father moved to France near Tours. They had means but money went farther in France than in America. In France they spent the remainder of their lives as American citizens. At the time of World War I they purchased the Chateau Monrepos near Blois, but Jack reacted against luxurious living and presently sought the more monastic simplicity of academic life in America. He visited his parents; they would have liked him before his marriage in 1925 to live with them, but he withstood their pressure. Mr. Center died in 1938, and Jack's mother in 1939, just as France was again being invaded.

Jack's education was trilingual, providing him with skills that were to give him a life-long advantage over his less fluent American collaegues and to make him during nearly thirty years the invariant standard for the French and German examinations in Harvard's Department of Psychology. He was sent first to school at the Collège de Marmontiers near home at Tours, then to the Bischöffliches Gymnasium in Strasbourg. After that in 1911-12 he returned to America and St. George's School at Newport, Rhode Island, where his academic record was splendid in every subject but English. In less than a year, though, he was back in France, where he completed his secondary education at the École de l'Ile de France in Liancourt, Oise.

Beebe-Center, influenced by his cousin, Junius Oliver Beebe of the Harvard class of 1916, came to Harvard College in 1915, entering as a member of the class of 1919. The war in Europe, however, soon interrupted his academic progress. In August 1916 he joined the American Ambulance Service, Section 5, the "Harjes Formation," as a driver. (H. H. Harjes was an American banker in Paris of the firm of Morgan and Harjes, an American Francophile like Beebe-Center's stepfather.) In this capacity Beebe-Center served with the French Army on the Verdun and Aisne fronts until September 1917. His unit received several citations and Beebe-Center, along with other members of the Harjes Formation, was awarded the Croix de Guerre.

By 1917 the United States was already in the war and Beebe-Center than transferred as a civilian to the Intelligence Section of the Chief of Staff of the American Expeditionary Forces. There is a letter extant to Captain George C. Marshall, agreeing to the loan of "Mr. Center, one of our best French interpreters, to help you out on French translation. He is with you for temporary duty only, so send him back as soon as you can spare his services."

In April 1918 this "Mr. Center" was commissioned Second Lieutenant in the Corps of Interpreters in the U. S. National Army, and assigned as interpreter to the Intelligence Section of the General Staff of the First Division of the American Expeditionary Forces in France, where he worked under General C. P. Summerall. He was made First Lieutenant the following October and then, after the Armistice, Captain in the regular Army in April 1919, to be discharged the following June.

After his discharge from the Army in June 1919, Beebe-Center returned to Harvard to finish his work for his degree. He was granted the A.B. *cum laude* in 1921 as of the class of 1919 after only two and a half years of actual study in residence. He was also at this time awarded by Harvard a "Detur book for general excellence in his studies," a beautifully bound copy of Jules Lemaître's *Théories et Impressions.*

After the A.B. he stayed on for graduate work in psychology and it was during this period that his loyalties to psychology, research, and Harvard became fixed. He never sought personal success or prestige, and his financial situation was such that he could devote himself to the success of scholarship and the advancement of knowledge, even when his contribution was so private that it was appreciated only by his intimates. What he wanted to do with his life was to promote research, his own and others', in psychology and at Harvard.

With this idealistic pattern of endeavor fixed in his graduate years, he went on to a brilliant Ph.D. in 1926 with a thesis on the law of affective equilibrium (this JOURNAL, 1929, 41, 54-69), one of the very first demonstrations of what later came to be called adaptation-level, if we omit from consideration visual, thermal, and olfactory adaptation. Actually Beebe-Center had to be urged to complete his experimental work and to present himself for the doctorate at this time, for the degree itself seemed unimportant to him. The psychologists who recommended the degree would have attached a *summa* to it had that been possible; instead, they instituted a special little ceremony, calling Beebe-Center back into the examination room and filing past him to congratulate him formally.

Beebe-Center was appointed Assistant in psychology at Harvard in 1923, and Tutor in Harvard's Division of Philosophy and Psychology in 1924. He married Roxanna Smiley Murphy on June 24, 1925, and they went abroad in 1926, right after his Ph.D., to fulfill his post-doctoral appointment as Sheldon Fellow from Harvard University. They attended the VIIIth International Congress of Psychology at Gröningen, went to Köln for a short period of study, and then settled down at the University of Berlin, where the exciting new movement of Gestalt psychology was now firmly entrenched under the leadership of Max Wertheimer and Wolfgang Köhler.

Beebe-Center returned to Harvard as Instructor in the fall of 1927.[1] He was appointed Lecturer on Psychology in 1935, and a few years later this appointment was continued "without limit of time" (as the Harvard phrase is), because it had become plain that the morale of this selfless devotee of research would not suffer if, with the quota of permanent appointees full, normal advance on the ladder of promotions was not to be forthcoming. He was elected to the American Academy of Arts and Sciences and to the Society of Experimental Psychologists, both in 1933. During the emergencies of World War II he gave instruction at Harvard on personnel psychology and participated in research on military projects in the Psychological Laboratories of Tufts College. He died suddenly on December 6, 1958, of a coronary thrombosis, while in conversation with his wife and son and without even a few seconds' warning.

Jack Beebe-Center lived a consistent life. To decisions that he made in youth he remained true for forty years, or, at any rate, we may say that he had the gift of perceiving correctly—as so few men have—the nature of his own basic motivations. He wrote in 1926 of how "the war of 1914-16, with its futile squandering of life and energy, greatly strengthened" his determination "to avoid the turmoil of social and financial competition by withdrawing into the quiet security of academic life." This he did and faithfully. Generous of time, energy, and personal concern for the problems of others, no colleague or student ever asked him for help to find it withheld or given stintingly, and no chairman but knew that in any emergency Beebe-Center stood ready to take on new responsibility with seeming gladness. Appointed for part time but throughout the years contributing full time to the work of the Harvard Psychological Laboratory, he became the friend of everyone, the advisor of graduate students whose only claim upon him lay in his own humanity, the wise mentor of both his senior and junior colleagues. Always the Laboratory felt his influence. At its staff luncheons it was most often he who provided the novel topic for discussion that made the conversation more important than the food. With his knowledge of French and German, throughout the years he remained, as we have noted, the invariant assessor of the ability of graduate students to use these languages. No one really knew how many holes he filled until at the end he left them so suddenly empty.

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[1] His only child, John G. Beebe-Center, Jr., was born the following winter on January 1, 1928.

One of the consequences of his philosophy of living was his individualism. In 1954, at the twenty-fifth reunion of his Harvard class, he wrote: "My non-academic life centers on family, home, and home-town. My political views parallel my interest in family and home: I favor state's rights over Federal ones, regional over states', individual ones over regional. I rationalize this political attitude by contending that centralization of power has, in the past, proved bad." In spite of his life-long dedication to research, he refused in the early 1940s to send professional information about himself to the National Scientific Roster or even to the Directory of the American Psychological Association. He would be neither catalogued nor controlled, yet would give more freely to honest need than ever did any regimented contributor.

At home Beebe-Center lived simply and plainly, adequately but in striking contrast with the splendor of his parents' chateau near Blois. There were always cats in the house on which he could exercise his humanity—cats in lieu of graduate students perhaps. The great domestic luxury that he and his wife enjoyed was the direct view of the Atlantic Ocean, right across the street from the house. He and his wife became sailors, too; first a canoe, then a dory, then a bigger dory, then a "210" sloop, which they sailed in the races of the Marblehead 210 fleet. Of this fleet Beebe-Center was made the Commodore with a special flag designed for him, a flag placed on the casket at his funeral.

It was his philosophy of avoiding strife and competition, plus his haunting memory of having learned as a boy some lines from Lucretius' *De rerum natura* about the Epicurean values (the greatest good is the repose of the mind that virtue brings), that led Beebe-Center into research on feeling, emotion, and eventually motivation—that and a remark of L. T. Troland's that, "considering the importance of feeling to human beings, the psychologists' neglect of this subject is a disgrace to the science." A full half of Beebe-Center's research and writing was on feeling and emotion—half of that half experimental and the rest evaluative. His doctoral dissertation on affective equilibrium must just have missed permanent recognition, partly because it was couched in older systematic terms at the time when psychology was turning to the new behavioral vocabulary, and partly because Beebe-Center never moved in his own behalf to insist on the significance of his thesis when all the talk about adaptation-level became rife. His compendium, *The Psychology of Pleasantness and Unpleasantness* (1932) was the clearest exposition of this body of research available at that time. The facts were sound, but gradually the terminology became a little strange to the new generation. The many papers found in his desk show how hard he worked, using his reading and his seminar and lecture notes, to update this account when in 1951 he wrote the chapter on *Feeling and Emotion* in Harry Helson's *Theoretical Foundations of Psychology.*

Beebe-Center's war research in the early 1940s was on visual acuity and the night-flying of pilots. At this time he also came to be deeply concerned with promoting friendly relations between the United States and Latin America, especially with the psychologists of our sister continent. For long years he remained in the Harvard Psychological Laboratory the greeter for foreign visitors. That may have been in part because of his linguistic competence, but it was also because of the generosity and friendliness that were basic to his nature.

In the last decade of his life his research interests turned to the psychology of taste. He had been led earlier in this direction by his study of the feeling-values of odors. Now he was concerned with thresholds and gustatory scales. He undertook to specify the intensitive unit of taste, the *gust.* In many of these studies he worked with younger collaborators, but the enthusiasm and the informing drive had always originated with him. He was indeed the perfect teacher for an experimental laboratory, a man who lived research and knew how to bring scholarship to its assessment, an individualist who never worked alone because he never withdrew from others and because his verve was infectious. Experimental psychology at Harvard and in America would profit greatly by more of his kind.

Harvard University EDWIN G. BORING

BOOK REVIEWS

Edited by GEORGE L. KREEZER, Washington University

Neuropharmacology: Transactions of the Third Conference. HAROLD A. ABRAMSON, Editor. New York, Josiah Macy, Jr. Foundation Publications, 1957. Pp. 381. $4.50.

This volume constitutes the transactions of the third annual multiprofessional conference on neuropharmacology sponsored by the Josiah Macy, Jr. Foundation. A surprising amount of material relevant to psychology is included within the 10 papers published in this book.

Either by design or accident there is considerable overlap between papers. Rather than being a simple reiteration, however, the overlapping is presented from widely differing points of view, thus affording the reader a greater appreciation of the topics' ramifications. The presentation concerning the effects of alcohol exemplify this trend. Richter's ambitious study of the alcoholic cravings in rats by dietary self-regulation produced some notable results with respect to the functions of alcohol in nutrition. One aspect of his study which may prove valuable to clinical psychologists is the relationship found between thyroid-function and alcoholic consumption. Rats made hyperthyroid showed a greatly decreased appetite for alcohol, while thyroidectomized rats greatly increased their consumption of alcohol. Quastel reviews his investigation of the action of alcohol on the central nervous system by the incorporation of glycine C^{14} into brain proteins. He has found that the stimulated respiratory activity of the brain seems definitely to be affected by alcohol, and he hypothesizes that the alcohol is interfering with electron-transport at the neuronal membranes. Finally, Masserman has studied the effects of alcohol on animals with experimentally induced neuroses. The results indicate that alcohol acts as an anti-neurotic agent and appears to be superior to both the barbiturates and most tranquilizers in this respect.

One of the newest and certainly the most publicized neuropharmacological agent presently being investigated is LSD-25 (lysergic acid diethylamide). The use of this compound is found in at least half of the studies reported in this volume. Abramson reviews his study concerned with the inhibition of the LSD-25 reaction by brain extract in Siamese fighting fish. Jarvik presents data regarding the effect of LSD-25 on snails. The results indicate that the snail exhibits a very marked individual response to small amounts of the chemical, indicating that it may be an ideal subject for bio-assay. Elkes's paper is concerned with the effects of psychomimetic drugs, among them LSD-25, but unfortunately limited space prevents even an inadequate review of his very excellent paper. Only the particularly illuminating discussion of the effects and his hypothesis of the mechanism of action of LSD-25 can be noted here. Other papers record the effects of LSD-25 on the depression of dendritic activity with the hypothesis that the chemical acts by interfering with the normal depolarizing action of neural tissue and interacting with neurohumoral mediators.

The evidence for the possible existence of noncholinergic neurohumoral transmitters is presented by Brodie, Purpura, and Elkes. Brodie speculates that serotonin and norepinephrine are antagonistic chemical mediators which regulate the central autonomic nervous system. Cross-perfusion experiments by Purpura indicate that neurohumoral agents play a role in the activity of isolated cortex through reticular stimulation.

Beecher's study of pain as a prototype for the study of other subjective responses has implications for psychology which certainly ought not be overlooked. His results indicate that a patient's subjective reaction to pain in large part determines pain-threshold elevation or depression. He then speculates on the relationship between these findings and the influence of efferent transmission-gradation and inhibiting effects within the central nervous system.

Olds presents the final paper on the brain-response to drugs mapped through self-stimulation. The effect of drugs such as reserpine and chlorpromazine on parasympathetic areas of the brain are studied by the use of his self-stimulation technique, a technique which is familiar to most psychologists.

The field of neuropharmacology is so new, yet expanding so rapidly, that any publication relevant to the area is valuable. Even though this volume is far from a good introduction to the uninitiated, it might well prove extremely valuable to subsequent investigators for reference-material as well as a stimulant to further research.

The University of Texas RANDOLPH H. WHITWORTH

Human Potentialities. By GARDNER MURPHY. New York, Basic Books, Inc., 1958. Pp. x, 340. $6.00.

This volume, it appears to me, is Gardner Murphy's 'testament' of faith and hope in the future of mankind. The jacket of the book carries the statement that its author "dramatically shows us how we can, by our free choices, control not only our own destinies, but those of countless generations to come." (Murphy should not be held responsible for the euphoria of his publisher's advertisement writer.)

It is true, the book contains many scholarly references to books, concepts, and principles in philosophy, graphic arts, music, literature, physical, biological, and social sciences. Murphy attempts to organize and utilize findings, concepts, and methods of these disciplines, together with those from psychology in an "exploration of the future directions of human development" (to quote again from the jacket).

In reading the book, I started out with the expectation of learning how human potentialities might be discerned, beyond what psychologists are already capable of discerning, and how these potentialities are to be or might be utilized for the greater good of mankind.

By promises of things to come, my expectations were built up in the first few chapters. In these early chapters, Murphy discusses some of the well-known concepts and problems in the areas of learning, drives, and cultural forces. He does so, it appears, as part of his purpose; namely, that "This book is an effort to describe the sources available within human nature for the outgrowing of human nature, the constitution of new varieties of human thought, value, and aspiration." (p. 7). Murphy states these new varieties of though, value, and aspiration present a "central problem"; that is, the fulfillment of the three human natures [sic] in their interrelations; [namely] the fulfillment, within the individual personality, of the raw stuff of his being, the acquired cravings that he experiences as a human being, and the deep protest [sic] against both as he seeks new meanings." (p. 21). How is this to be achieved? "New components and new interactions will produce a kind of human nature and human experience which will be as far from the present as our own is from that of our earliest Old Stone Age Ancestors." (p. 22).

To me, the reading of this book was a frustrating experience; because the remainder of Dr. Murphy's presentation only elaborates the *need* to develop new varieties of thought and behavior, as well as his belief that mankind has not by any means come close to realizing his potentialities for an improved and more humane kind of life; at least, for a different kind of world to live in. He does so by discussing some current psychological findings and principles, historical eras of creative achievement, esthetic and scientific developments, and biological changes in man. Presumably, mankind's achievements thus far are indicative that he has a future more impressive still, as well as a nobler one in terms of creativity and interpersonal relationships. This would be an inspiring goal which nearly all readers would consider most desirable.

Reading further in this book, however, was a frustrating experience because its author does not actually come to grips with basic questions of "How?" and "What?" Which psychological principles, methods, and tools shall be used, and how, and to which ends? Are some goals, values, and ends more consistent with mankind's potentialities ("natures") than others? Can we, in view of history, say with any assurance that mankind's biological (perhaps atavistic) nature will produce or yield to improved cultural patterns and to the search for "new meanings"? From the humanistic point of view, has mankind actually progressed, not merely changed his mode of living, in the last five or ten thousand years? (See Nelson Glueck's *Rivers in the Desert,* (1959). It he has not, what are the prospects that he will or can in the not-too-remote future? Are the great creative achievements in the graphic arts, architecture, music, literature, science, and technology typical of "mankind" or of only a minute fraction who are the extreme deviants, perhaps the mutants? In view of what we have learned about human abilities in the last fifty years, what may we expect of the generality of mankind? How might psychologists, biologists, and social scientists approach and effectively deal with the forces of established and entrenched social and economic practices, religious hostilities and tensions, national and racial dogma? All of these are serious obstacles in the way of attaining the great goal which the book continually holds before our eyes.

Murphy does not, in fact, deal with these and similar questions which to me seem fundamental in an extended analytical discussion of human potentialities and the uses to which they are put. On the last page of the book, he concludes that "The realization of human potentialities lies in studying the directions in which human needs may be guided, with equal attention to the learning powers of the individual and the feasible directions of cultural evolution. . . . The future extension of scientific and esthetic interest, together with the evolution of greater capacity for satisfaction in relations between people, will not constitute a goal or a Utopia, but will define a widening theatre for the development of new potentialities."

It is my judgment that this book in condensed form would make a good beginning for a book dealing with an exploration of the problems mentioned above and searching for possible solutions. Perhaps Dr. Murphy will undertake this task in another volume. It is to be hoped that in another volume he will not yield to a predilection for cumbersome figures of speech and an elaborate literary style which interferes with his exposition.

It is my belief that Gardner Murphy's major contributions, in book form, are to be found in his two earlier and excellent volumes: *Historical Introduction to Modern Psychology and Personality: A Biosocial Approach to Origins and Structure.*

Cornell University FRANK S. FREEMAN

The Psychology of Interpersonal Relations. By FRITZ HEIDER. New York, John Wiley and Sons, 1958. Pp. ix, 322. $6.25.

Person Perception and Interpersonal Behavior. RENATO TAGIURI and LUIGI PETRULLO, Editors. Stanford, Stanford University Press, 1958. Pp. xx, 390. $7.50.

There is an evident complementary relationship between these two books which prompts their joint review. Within the span of approximately a decade, psychologists concerned with social behavior have moved increasingly toward a probing of the effects individuals produce on one another as they interact. These two volumes serve well to expose the *locus operandi* that social psychology has now reached in the sphere of interpersonal affairs—the diadic relationship.

Heider has been in the vanguard of this movement, and now offers this book—the result of his long quest for an integrative approach. It merits serious attention. He explores "how one person thinks and feels about another person, how he perceives him and what he does to him, what he expects him to do or think, how he reacts to the actions of the other." (p. 1).

His work will strike some as an odd mixture of commonsense, imagination, and science. Aware of this prospect, he says, "This book does not claim to present a complete program for describing interpersonal relations in terms of underlying concepts. But it should serve as the beginning of such a task and will include an attempt to clarify some of the basic concepts that are most frequently encountered in an analysis of naïve descriptions of behavior" (p. 14). He employs fables, short stories, and a wide array of experiences to analyze and elucidate his points. His familiarity with the scientific literature is, however, evident throughout his treatment.

Perception holds the key position in his work. It is sometimes difficult, however, to discern the limits of perception, on the one hand, and behavior, on the other. His treatment of perception and cognition is, moreover, not consistent; sometimes he writes of them separately, at other times as if they were of a piece.

What makes the work eminently worthwhile, withal, is Heider's otherwise systematic application of the gestalt point of view, his rigor in defining variables of relevance in the interpersonal sphere, and his introduction of a standard symbolic form to treat them. This latter element constitutes a stride forward. It is a fulfillment of Heider's intent "to offer suggestions for the construction of a language that will allow us to represent, if not all, at least a great number of interpersonal relations." (p. 9).

The second book, edited by Tagiuri and Petrullo, contains twenty-three papers that were presented at a symposium sponsored jointly by the Office of Naval Research and Harvard University. With the exception of two papers, reprinted from psychological journals, all were especially prepared for the occasion. Among the contributors one finds the most notable figures in this area of activity. The nature of their reports varies from strict research papers to more discursive, theoretical articles. It is difficult, therefore, to do justice to the range of papers and the substance they offer. One misses, however, an over-all, integrative paper, though Tagiuri attempts in the introduction to set the stage. Read with discrimination, many fruitful directions of research can be discerned. The reader will find this collection an adequate review of the current developments in this sphere—particularly those with an experimental cast.

Washington University E. P. HOLLANDER

Schedules of Reinforcement. By CHARLES B. FERSTER and B. F. SKINNER. New York, Appleton-Century-Crofts, 1957. Pp. vii, 741. $6.50.

This book presents a microscopic analysis of the behavior of a single species (pigeon), in a single situation, as a function of the variation of the sequential dependencies of reinforcement in the domain of time and number and rate of responses.

After introducing the subject of reinforcement schedules and giving a general orientation to Skinner's system of analysis, the authors follow with an encyclopedic presentation of the varieties of complex schedules obtainable from a logical manipulation of various simpler schedules and their combinations, together with certain collateral events. The third chapter presents a clear description of certain techniques relating to care of laboratory animals (the White Carneaux pigeon), instrumentation, and recording and representation of behavioral data. While this chapter does provide the reader with a good understanding of many of the techniques employed later in the text, it is not a laboratory 'cookbook.' The remainder of the book is devoted to the description of behavior following the establishment of schedules of reinforcement such as fixed ratio, fixed interval, variable ratio, variable interval, tandem schedules, chained schedules, concurrent schedules, and differential reinforcement of rate of response. Each of these topics is treated in a separate chapter. Sub-topics peculiar to a given schedule of reinforcement are dealt with in the appropriate chapter, *e.g.* percentage reinforcement in fixed-ratio schedules. More general sub-topics appearing in several different chapters include: transition from continuous reinforcement to a given schedule, extinction after a given schedule, effects of drugs, brain ablation, novel stimuli, added stimuli correlated with some aspect of the schedule, etc. A serviceable glossary of new and older terms completes the book. A cursory index is also provided.

This book cannot be recommended as a textbook for any but the most highly specialized course; it is not a handbook; it is a book of experimental results and careful conclusions resulting from a study of one-quarter of a billion responses of the experimental animals. As such a book, it is definitely worth reading, provided that the reader is prepared to make allowances for certain limitations: the organization of the materials is utterly foreign to any system of analysis previously published, data are represented almost exclusively by graphs transcribed directly from record charts, small numbers of animals are used for a given experiment (generally two, sometimes four), and summaries of results of complex manipulations are not always presented. Some of these criticisms are answered by the authors. For example, since behavior appears reversible (*i.e.* once on a schedule, an animal may be exposed to certain effects and return to the earlier schedule with a corresponding return to the appropriate behavior), large numbers of animals are not needed, the animal serving as its own control. It is highly probable that research in this area will be stimulated by the demonstration of the possibilities inherent in the instrumental techniques employed. The friction in this system will be mostly in representation and interpretation of the graphic data usually obtained. The total of written text in the book amounts to 287 pages, and the rest of the book is taken up with 434 pages of graphs. Some readers may infer, therefore, that the book is not as long as it appears; if so, let them be reminded of the ancient Chinese proverb—"One picture is worth a thousand words."

Washington University C. E. SHERRICK

Multivariate Correlational Analysis. By PHILIP H. DUBOIS. New York, Harper & Brothers, 1957. Pp. XV, 202. $4.50.

In the preface of this refreshing change from run-of-the-mill non-mathematical introductions to statistics, the author states a major intention to present "a systematic formulation of multiple and partial correlation that relates the formula methods of Yule to the matrix methods in common use." This statement of intention is misleading, however, since nowhere has any attempt been made to introduce or even use any of the concepts or algebra of modern matrix-methods. Once it becomes clear that this is not a book concerned with the transformation of Yule's (and others') formulations to matrix-forms, but rather a description in non-mathematical terms of many of the relationships among variances, covariances, multiple and partial correlations of all orders, and partial variances and covariances of all orders, it is a delightful and informative book for the non-mathematically trained user of such statistics. In addition to the excellent descriptive material, DuBois has provided an efficient and flexible worksheet (rather than matrix) method for computation of any of the descriptive statistics involving correlations that might be of interest to the research worker who must rely on desk calculator methods and who does not have mathematical training beyond highschool algebra. In a few places, especially in Chapter 10 which is entitled "Some mathematical considerations," the author has presented derivations of some of his formulae but nowhere is there an attempt to develop rigorous mathematical proofs. This is a fortunate choice, since the readers to whom the book is directed would be unlikely to find such proofs of interest.

The author points out clearly and correctly that any correlation-coefficient is a statement of the linear relationship between two and only two variables, and interpreted as such there are few important restrictions on its use. He does not discuss (except for very brief mention on p. 155) any of the standard error equations for regression-weights and for various forms of correlation-coefficients, all of which are readily available from the worksheet. Nor does he discuss any of the common pitfalls in interpretation of significances of correlation-coefficients and differences between correlation-coefficients. This may be considered a serious omission, but it is this reviewer's opinion that only cursory treatment can be given in non-mathematical terms, hence the author's choice of emphasizing description rather than significance may be the wiser course. The book is refreshing because it is consistent in that it assumes little knowledge of mathematics and does not implicitly require such knowledge.

With the exception of two chapters (35 pp.) on some relations of factor analysis to multiple correlation analysis and a short chapter entitled "Multivariate analysis in social science research" (11 pp.), the material presented is restricted to an exposition of correlation-analysis and a description of the worksheet methods. There are appendices of 14 pp. and a useful glossary of 9 pp.

System Development Corporation **JOHN M. LEIMAN**

Decision Making: An Experimental Approach. By DONALD DAVIDSON and PATRICK SUPPES, in collaboration with SIDNEY SIEGEL. Stanford, California: Stanford University Press, 1957. Pp. 121. $3.25.

The research reported in this monograph has not been published before except as technical reports of the Stanford Value Theory Project. The collection can be read for its specific findings; however, its more general methodological contributions are sufficient to warrant study by psychologists interested in the application of mathematics to human behavior.

The book presents a theory of rational choice that is a broader variation of the Von Neumann and Morgenstern axiomatization than similar theories attempted to date. The authors stress the need for testable hypotheses, and develop a method for doing just that from their own theory. A behavioristic solution is proposed which measures psychological probability separately from utility by means of a simple one-person game which, in turn, provides the basis for two sets of experiments. The first set tested the basic, ordinal model against betting behavior elicited by gambling for small amounts of money. The second set tested a less-restricted, linear programming model by means of the same game but with non-monetary outcomes. In addition, a very interesting model for incomparable outcomes, *i.e.* outcomes that are not directly comparable, but which are indirectly comparable, is discussed in the final chapter.

The experiments are reminiscent of classical psychophysical studies primarily because of the repeated measurement of the same individuals, and the careful interpretation made of each set of data. In general, the models predict behavior better than a random guessing model; however, there are deviations that indicate more comprehensive analysis is required, as the authors point out. On the other hand, the manner in which the authors face and deal with the problem of going from abstract mathematics to practical experimental procedures is certainly worthwhile reading.

Dayton, Ohio **RICHARD L. DEININGER**

The Methods of Research in Psychology. SADAJI TAKAGI, Editor. Tokyo, Iwanami Shoten, 1958. Pp. vii, 488.

This book deals with general and specific methods of research in different fields of psychology rather than with the results of research, the latter having previously been reported by the editor in his *Handbook of Experimental Psychology* (Jikken Shinrigaku Teiyo), which was published in 1951.

The introductory chapter deals with the fundamental assumptions of science in general and of psychology in particular, with introspection, mathematical description, the working hypotheses of psychology, experimental design, and with the recent non-parametric tests.

The four chapters which follow deal with various topics in experimental psychology. Explanations are given of the procedures followed in typical experiments, together with detailed notes. In the field of perception, we find 11 topics in vision, ranging from dark-adaptation through social perception; 8 in audition; and 4 in the other fields of sensations. Many additional experiments from the fields of learning, thought, and motivation are described and illustrated. Recent experiments in Japan, such as Motokawa's physiological method of studying color-contrast, Yokose's method for measuring the "field-strength of the visual form," Morinaga's anomaly in relation to geometric illusions, and Toda's game-method for study of intuitive probability are described. The reader is assumed to be familiar with the psychophysical and the traditional memory-methods, consequently references to topics such as the constant-errors and perceptual leveling are not included.

In the last five chapters, the survey-techniques in the fields of personality, social, developmental, abnormal, and industrial psychology are considered. Interview, rating-method, attitude-scale, and sociometry are introduced and discussed in detail. The clinical methods are, however, sketched only in outline.

Though not an exhaustive treatment of the research methods of psychology, this book is a useful compendium and guide for young research workers.

Washington University — SHINSHU NAKAJIMA

The Tibetan Book of the Dead or The After-Death Experiences on the Bardo *Plane according to Lama Kazi Dawa-Samdup's English Rendering.* By W. Y. EVANS-WENTZ. With a psychological commentary by Dr. C. G. Jung. Forewords by Lama Anagarika Govinda, and Sir John Woodruffe. Third Edition. London, Oxford University Press, 1957. pp. lxxxiv; 249, 9 illus. $4.00.

Tibetan Yoga and Secret Doctrines or Seven Books of Wisdom of the Great Path, According to the Late Lama Kazi Dawa-Samdup's English Rendering. Edited by W. Y. EVANS-WENTZ, Foreword by Dr. R. R. Marett and Yogic Commentary by Prof. Chen-chi Chang. Second Edition, London, Oxford University Press, 1958. pp. xlii, 389, 9 illus. $6.75.

Both of the works noted above have been frequently reviewed in their earlier editions. The reissues have added nothing which enhances their value to psychologists beyond that which they might originally have had. The commentary by C. G. Jung, first printed in English in the *Book of the Dead,* has in fact now been made available in the context of similar writings under the Doubleday Anchor imprint (*Psyche and Symbol:* A selection from the writings of C. G. Jung, Ed. by Violet S. de Laszlo, Anchor Books, N.Y. 1958. $1.25).

Both studies offer insight into Buddhist theories of the interplay between *psyche* and *soma* and the possibilities of detaching the one from the other. One need not be acquainted with the intricacies of Buddhist doctrine to find interesting the discussion of causal sequences which are assumed to lead to final realization of the conscious-subconscious links. In fact, the key concept in both works is that 'mind' and 'body' *are* separable, but that in life the interplay of mind, body, and external environment form an inextricable whole, (one is tempted to translate: a system in equilibrium) and that one cannot examine either mind, body, or environment as a thing in itself. Thus *yogic* exercises and ways of thought are necessary to bring the "body" and "mind" to point of separation—to allow mind to remove itself from the physiological bonds which normally retain it. One is reminded of the recent article by Kendon Smith ("The Naturalistic Conception of Life," *American Scientist,* 46, 1958, particularly pp. 419-421) which suggests a view startlingly close to that of the Buddhist thinkers. I suggest that Smith's article and both books of Evans-Wentz be read and compared by psychologists not averse to speculating on what it is they are 'really' studying.

Washington University R. J. MILLER

Assessment of Human Motives. By GARDNER LINDZEY (ed.). New York, Rinehart, 1958. Pp. 273. $5.00.

This representation of the current status of motivational theory and research contains the contributions of nine psychologists whose own research and theoretical orientations are widely known and diverse. Lindzey's introductory essay details the major issues in the assessment of human motives. These are they: How important are conscious as opposed to unconscious motives? How important are ego processes or cognitive controls? How important is the situational context as compared with knowledge of the individual's past history? How important are multivariate analyses as compared with detailed studies of individual cases? How important is research on animals?

Two chapters are psychoanalytic, one by Irving Janis, who suggests how the raw data of the analytic interview might be treated to yield information of nomothetic value by revealing how situational events of adult life modify patients' attitudes; and the other by Roy Schafer, who illustrates how temporary, controlled lowering of the level of psychic functioning aids adaptation and how systematic study of this process will further research in personality.

The other approaches include the sophisticated psychometric of Raymond Cattell, the cognitive dissonance of Leon Festinger, the personal constructs of George Kelly, and the cognitive control of George Klein. Essays on the problem of how to describe motivation in the simplest language and with the most economical number of constructs by Gordon Allport and on the relationship between style of life and motivation by Henry Murray complete the volume. The conception of human nature which Murray sees typifying the American way, and reflected in American psychological research, emphasizes an "ascensionism"—an ambition originally religious and zealous and now secular but just as zealous. Ascensionism is defined as an integrate of a need for achievement and a stronger need for attention and prestige.

Most interesting in this up-to-date set of essays on motivational theory is the relative lack of emphasis on physiological levels of drives or psychological levels of simple reinforcement. The emphases presented here involve complex levels of cognitive functioning and of integration of personality structure. Unconscious motivation is now taken for granted—showing how far we have come since Knight Dunlap.

St. Louis University WALTER L. WILKINS

The Psychology of Affiliation: Experimental Studies of the Sources of Gregariousness. By STANLEY SCHACHTER. Stanford. Stanford University Press, 1959. Pp. 141, $3.75.

This little book makes an important contribution to a field characterized by a decided lack of information. Its importance lies in the variety of ways in which the author demonstrates that the affiliative tendency (gregariousness) can be studied experimentally as well as by correlational and observational techniques.

Schachter's basic paradigm can be illustrated by the following experiment which is discussed extensively in the first half of the book: one group of *S*s is given instructions producing anxiety and a second group is not. Following this, all *S*s are told to wait for the experiment to begin and then are polled to find if they wish to wait alone or with other *S*s. The results indicate that significantly more *S*s in the 'anxiety' group than in the control group prefer to wait with other people. The author also finds that birth order is significantly related to the affiliative tendency when tested in the above manner. The author goes on to develop a theory relating dependency and the affiliative tendency.

Schachter often fails to make proper use of common statistical techniques and employs techniques of questionable value. For example, the author assumes his *S*s are independent (for statistical treatment) whether or not they serve individually or in groups (*e.g.* pp. 16 and 17); he employs two *t*-tests to compare three groups when the assumption of normality of the population from which he is sampling is questionable (p. 116) and he computes seven Mann-Whitney *U*-tests to compare six groups when a double classificatory analysis of variance would seem to be more appropriate (p. 110).

The statistical problems should not detract from the point that the author is making: the affiliative tendency can be studied in an experimental situation. The book should serve as a stimulus for further research in this area.

University of Texas ROBERT K. YOUNG

The Ants. By WILHELM GOETSCH. Ann Arbor, University of Michigan Press, 1957. Pp. 169. $4.50.

Goetsch's little book on the ant, translated by Ralph Manheim from the second German edition of *Die Staaten der Ameisen,* is of interest to the layman as well as to the student of comparative psychology. After describing the anatomy and physiology of the different castes—queen, male, soldier, worker—Goetsch traces the development of a colony from the fertilization of the queen on her nuptial flight to its establishment within its particular environment. He explains the origin of the castes (differential care of the larvae and pupae), the division of labor (nursery, housekeeping, hunting, warfare), the effect of environment (woods, meadows, deserts, mountains, climate, and seasonal rhythms); and he describes how ants communicate with one another, how they recognize nest-mates, and how they find their way home from hunting expeditions. Separate chapters are given to aliens and enemies found within the nests, to ants with granaries and fungus gardens, to aphid tenders, and to honey-pot ants whose bodies serve as reservoirs for food. The book is well illustrated with 85 line cuts and halftones, but it is without an index. For the use of students, a subject matter index should have been provided. An index of illustrations would also be helpful. K. M. D.

The Age of Psychology. By ERNEST HAVMANN. New York, Simon & Schuster, 1957. Pp. ix, 115. $1.00.

Mr. Havmann is a feature writer on the staff of *Life* magazine and has a master's degree in psychology. He was assisted by C. T. Morgan and F. C. Redlick as consultants in the preparation of the manuscript. It is, therefore, a fairly accurate layman's guide to the science of behavior.

The book is simply written and is recommended for the layman who has a passing interest in the relationships of psychology, psychiatry, and psychoanalysis, and as collateral reading for undergraduate students taking general psychology.

The author gives numerous examples of the application of psychological principles in business and shows how these enter the fabric of our everyday lives. Two chapters are devoted to basic research on the topics of seeing, hearing, and learning. The author then shows how this information is applied to school and industrial problems, thus demonstrating the relationship of basic and applied science. This is followed by a discussion of the application of psychology, psychiatry, and psychoanalysis to mental health problems.

Florida State University D. R. KENSHALO

The Sense of Form in Art: A Comparative Psychological Study. By HEINRICH WOLFFLIN. Translated by Alice Muehsam and Norma A. Shatan. New York, Chelsea Publishing Company, 1958. Pp. 230, $6.50.

This is a translation of the 1931 German edition. The English title might be more accurately rendered as "Italy and the German form concepts" (Italien und das Deutche Formgeguel), and the somewhat misleading subtitle as "A Comparative Study of Their Characteristics." The book has little interest for psychologists excepting those few who might be well trained in the history of art, since familiarity with a number of lesser known artists and works is assumed. The investigation concerns itself primarily with the interrelationship between the Italian and German Rennaisance. The presentation, while often brilliant, is at times marred by overtones reflecting the prevailing atmosphere of its original publication date.

New York WARREN R. STONE

Anthropology and Human Nature. By M. F. ASHLEY-MONTAGU. Boston, Porter Sargent, 1957. Pp. 390, $6.00.

This is a collection of Ashley-Montagu's papers, covering a broad spectrum of problems: general characteristics of humans; race; anthropology and medicine; sexuality; evolution; and medicine. Some of the papers are written in a very 'popular' style, others are exceedingly technical. The book does not show the author at his best and some of the papers seem out of date and even out of place.

Washington University JULES HENRY

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RATE OF APPARENT CHANGE OF A NECKER CUBE AS A FUNCTION OF PRIOR STIMULATION

By LEON COHEN, New York University

Recent interest in apparent fluctuations of ambiguous figures derives largely from Köhler's suggestion that those neural processes which are assumed to produce figural after-effects also operate in determining reversal-rate.[1] At present, there is little question that both reversal-rate and figural after-effects share some common features.

For example, Hammer reports that the magnitude of figural after-effects is a function of the length of the inspection-period up to 90 sec., after which a stable maximal displacement is produced.[2] Similarly, Brown investigated the reversal-rate of moving patterns with ambiguous direction and depth-relations and found that rate of apparent change (*RAC*) increases during the first minute or two of observation and is maintained thereafter at a level significantly higher than the initial rate.[3]

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* Received for publication October 30, 1958. This article is based upon a portion of a dissertation submitted to the Department of Psychology of New York University in partial fulfillment of the requirements for the Ph.D. degree.

[1] Wolfgang Köhler, *Dynamics in Psychology,* 1940, 73 f.; The nature of these neural processes is at present the subject of considerable debate. See Wolfgang Köhler and Hans Wallach, Figural after-effects: An investigation of visual processes, *Proc. Amer. phil. Soc.,* 88, 1944, 270; C. E. Osgood and A. W. Heyer, Jr., A new interpretatioon of figural after-effects, *Psychol Rev.,* 59, 1952, 98-118; M. B. Bender and H.-L. Teuber, Phenomena of fluctuation, extinction and completion in visual perception. *Arch. Neurol. Psychiat.,* 55, 1946, 627-658; Robert Jaffe, Kinesthetic after-effects following cerebral lesions, this JOURNAL, 67, 1954, 668-676; G. S. Klein and David Krech, Cortical conductivity in the brain-injured, *J. Pers.,* 21, 1952, 118-148; K. S. Lashley, K. L. Chow, and Josephine Semmes, An examination of the electrical field theory of cerebral integration, *Psychol Rev.,* 58, 1951, 123-136; R. W. Sperry, N. Minor, and R. E. Myers, Visual pattern perception following sub-pial slicing and tantalum wire implantations in the visual cortex, *J. comp. physiol. Psychol.,* 48, 1955, 50-58.

[2] E. R. Hammer, Temporal factors in figural after-effects, this JOURNAL, 62, 1949, 337-354.

[3] K. T. Brown, Rate of apparent change in a dynamic ambiguous figure as a function of observation-time, this JOURNAL, 68, 1955, 358-371.

Both phenomena also depend on the presence of contours in the visual field during the inspection-period. Köhler and Wallach report that no figural after-effects can be produced by inspection of a homogeneous field.[4] Brown states that merely fixating a homogeneous field will not increase the *RAC* of a moving ambiguous pattern presented immediately afterwards.[5]

The effects of varying figure-ground contrast are not quite as clear. Köhler and Wallach found that a figural after-effect is stronger if the brightness-difference between the inspection-figure and its ground is increased.[6] Brown tested this relationship indirectly by altering his *S*s' contrast-sensitivity (*i.e.* by varying the preadaptation-luminance prior to testing).[7] In accordance with satiation-theory, exposure to high preadapting luminance (producing low contrast-sensitivity) resulted in significantly fewer reversals than exposure to low preadaptation-luminance. Mull, Ord and Locke tested the hypothesis by directly varying the figure-ground brightness-contrast of a Necker cube, and found, however, that this factor did not alter reversal-rate.[8] Whether this discrepancy results from the different means of altering effective brightness-contrast, or from the use of different types of ambiguous figures, or both, is unclear.

Köhler and Emory investigated the effects of inspecting three-dimensional figures on the subsequent perception of plane figures, and found that such inspection induced apparent depth in the plane figure which was opposite in direction to the orientation of the inspection-figure.[9] Carlson, employing the paradigm of figural after-effects, required his *S*s to inspect a reversible perspective figure which had been so slightly modified that only one of the two possible alternatives was seen.[10] When the ambiguous version of the figure was then presented, the alternative which was *not* seen during the inspection-period tended to appear first and occupied a relatively longer time during each reversal-cycle.

That figure-reversal is markedly altered following brain injury is well documented.[11] A similar susceptibility to brain lesion of kinesthetic after-effects has been claimed[12] and disclaimed.[13] Further investigation of alterations of figural after-effects following brain injury is necessary to settle the issue.

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[4] Köhler and Wallach, *op. cit.*, 270.

[5] Brown, Studies on rate of apparent change as a function of observation-time in a new type of dynamic ambiguous figure, *Wright Air Development Center Technical Report,* 1954, 1-31.

[6] Köhler and Wallach, *op. cit.*, 270.

[7] Brown, *op. cit., Wright Air Development Center Technical Report,* 1954, 1-31.

[8] H. K. Mull, Nancy Ord, and Nan Locke, The effect of two brightness factors upon the rate of fluctuation of reversible perspective, this JOURNAL, 67, 1954, 341-342.

[9] Wolfgang Köhler and D. A. Emory, Figural after-effects in the third dimension of visual space, this JOURNAL, 60, 1947, 159-201.

[10] V. R. Carlson, Satiation in a reversible perspective figure, *J. exp. Psychol.,* 45, 1953, 442-448.

[11] M. R. Harrower, Changes in figure-ground perception in patients with cortical lesions, *Brit. J. Psychol.,* 30, 1939, 48-51; G. K. Yacorzynski and L. Davis, An experimental study of the frontal lobes in man, *Psychosom. Med.,* 7, 1945, 97-107; G. K. Yacorzynski, B. Boshes, and L. Davis, Psychological changes produced by frontal lobotomy, *Res. Publ. Ass. nerv. ment. Dis.,* 27, 1948, 642-657; Henry Feinberg, Necker cube reversals in normals and men with frontal lobe injury, Unpublished Master's thesis, New York University, 1953; Leon Cohen, Perception of reversible figures by normal and brain-injured subjects, *Arch. Neurol. Psychiat.,* 81, 1959, 765-775.

In view of these observations, it would seem worthwhile to explore systematically, whether various additional factors known to influence figural after-effects will also influence reversal-rates; or whether there are some factors which play a role in one set of phenomena but not in the other. Specifically, we shall investigate the following problems.

(1) Brightness-reversal. Köhler and Wallach have stated that if an inspection figure (*I*) is given black on white and the corresponding test-figure (*T*), white on black, "we must obtain the same figural after-effect as though the *T*-figure were also black on white."[14] One question, then, is whether inspection of a black-on-white reversible figure will induce an increased rate of apparent change (*RAC*) in an otherwise identical white-on-black *T*-figure.

(2) Spatial proximity. The magnitude of figural after-effects is a function of the spatial proximity of inspection- and test-contours, so that such effects are greatest in the immediate vicinity of inspection-contours. We may ask, therefore, whether the *RAC* of a reversible test-figure can be increased by prior inspection of patterns whose contours coincide with, or are in close proximity to, the contours of the test-figure.

(3) Perceived similarity. Figural after-effects are not dependent on perceived similarity of inspection- and test-patterns. Any visual object can induce figural after-effects in an appropriately placed test-figure. This raises the possibility that inspection-patterns which differ in perceived form from reversible test-figures can produce an increased *RAC* in the test-figures.

Apparatus: Experiments I–II. A black cardboard rectangle (64 x 24 cm.) with a centered aperture (10 cm. square) was clamped to a table in *S*'s frontal plane at a distance of 88 cm. between *S*'s chinrest and the center of the aperture. The stimulus-figures were mounted on a panel so placed behind the cardboard rectangle that *E* could expose figures at the aperture, one at a time, by manually sliding the panel. A typewriter was so placed on a stand to *S*'s right that he could conveniently keep his hand on one of the keys during the testing procedure. The experimental room was darkened and the stimulus-display was illuminated from over *S*'s head by clamping a substage-lamp of a microscope to a ringstand. The same stand supported the chinrest.

Experiments III–VI. A more substantial wooden instrument was built with the same over-all dimensions but with an aperture 15.7 cm. square. The sliding panel on which the figures were mounted was set on a track and was spring-loaded. *E* could remotely shift a desired figure into view by actuating a solenoid. The physical arrangement for lighting and response-recording was the same as in Experiments I and II, except that the illumination was now provided by a 60-w. lamp housed in a reflector. A wooden chinrest clamped to the table was substituted for the arrangement used in the previous experiments.

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[12] Klein and Krech, *op. cit.,* 118.
[13] Jaffe, *op. cit.,* 668.
[14] Köhler and Wallach, *op. cit.,* 331.

Materials. All figures were non-gloss, black-and-white photographic prints. Contrast between figure and ground for all figures was equal. The vertical and horizontal edges of the *T*-figure, which was used throughout, measured 17 mm. In addition, an unexposed square of photographic print paper was developed and was used for light-adaptation and control purposes; the reflectance of this figure was equal to that of the backgrounds of all black-on-white figures and was equal to the reflectance of the white contours in the case of a white-on-black figure. Small disks of red cellophane tape served as fixation-points in Experiments I and II; all other figures had black fixation-points.

EXPERIMENT I. RATE OF APPARENT CHANGE (*RAC*) VS. OBSERVATIONAL TIME

The aim of Experiment I was to determine the manner in which the rate of apparent change (*RAC*) of a conventional Necker cube increased with prolonged observation.

Method. Each *S* was seated, chin in rest, with a finger on a typewriter key. *S* fixated the homogeneous white field (Fig. W) for 1 min., after which *E* immediately shifted the T-figure (the Necker cube) into view. *S* was instructed to strike the typewriter key every time a change in the apparent perspective of the cube occurred, and also to respond if the cube appeared to become 'flat' (*i.e.* lost its three-dimensional appearance) or resumed a cube-like appearance after having seemed 'flat.'

Every 15 sec., *E* struck the solidus-key, permitting a count of the number of fluctuations by 15-sec. periods. Every *S* observed the cube in this manner for 3 min., *i.e.* for 12 consecutive 15-sec. periods.

Subjects. Twenty-six clerical and professional employees of various research laboratories at New York University served as the *Ss*.

Results. Analysis of variance yielded a significant *F* for the observational periods (*OP*), and a *t* was computed between the mean number of reversals obtained in each *OP* (see Table I). Rate of apparent change (*RAC*) increased during the first minute of observation and it was maintained at this level for the remaining 2 min. of fixation (see Fig. 1). The increase in reversal-rate which we obtained is steeper than that reported by Brown, but the maximal level was reached in approximately the same time.

Although these results show that a substantial increase in *RAC* occurs with continuous fixation, the variables which produced this rise cannot yet be specified. Such variables may be explored by employing the figural after-

TABLE I

Summary of Analyses of Variance

Experiment	Source	Mean square	*df.*	*F*	P
	Observation periods	14.95	11	5.66	.001
I	Ss	20.55	25	7.78	.001
	Residual	2.64	275		
	Observation periods	15.14	8	5.94	.001
II	Ss	23.27	27	9.12	.001
	Residual	2.55	216		
	Conditions	1.31	4	9.36	.001
III*	Ss	.78	23	5.57	.001
	Residual	.14	92		
	Conditions	10.86	4	3.60	.01
IV	Ss	41.92	23	13.88	.001
	Residual	3.01	92		
	Conditions	28.45	6	17.10	.001
V	Ss	7.26	23	4.37	.001
	Residual	1.67	138		
	Conditions	.57	7	4.38	.001
VI*	Ss	2.32	29	17.80	.001
	Residual	.13	203		

* Values for these analyses are based on a $\sqrt{+0.5}$ transformation of the data to correct for non-homogeneity of variance.

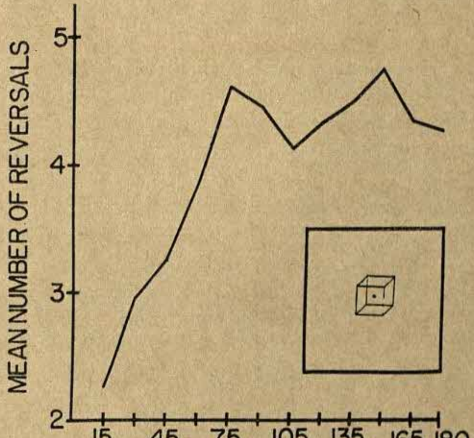

Fig. 1. Rate of Apparent Change of a Necker Cube (Fig. T) As a Function of Observation-Time in Experiment I

Inset shows figure as it appeared in the apparatus.

effects paradigm, *i.e.* presenting two different stimulus-objects sequentially and determining the effects of the former (inspection-figure) on the *RAC* of the latter (test-figure).

This procedure was employed to determine whether prior inspection of a black Necker cube on a white ground would increase the rate of fluctuation of a cube subsequently presented, that was identical with the first but white on black.

EXPERIMENT II. EFFECT OF THE *I*-FIGURE ON THE RATE OF APPARENT CHANGE (*RAC*) OF THE *T*-FIGURE

According to the Köhler-Wallach hypothesis, the satiation-effects of otherwise identical visual objects with reversed figure-ground brightness are equivalent. They wrote:

> It can make no essential difference whether the *I*-figure is shown dark on a bright ground or bright on a dark ground. The electromotive forces must be the same, although in the second case their direction must be reversed. This merely reverses the direction of the current; but apart from that the spatial distribution of the flow will remain about the same. Its electrotonic action will therefore also occur in the same distribution, and as a result similar after-effects must be observed in the two situations.[15]

If increased *RAC* results from increased satiation produced by continuously fixating a reversible figure, inspection of a black-on-white cube should lead to an increased *RAC* of a white-on-black test-cube.

Hochberg tested the satiation-hypothesis of figure-reversal by requiring inspection of an unambiguous Maltese Cross and then presenting ambiguous test-figures which could either be seen as black-on-white or white-on-black crosses. When the corresponding portions of the inspection- and test-patterns had the same black-white relationships, the unsatiated alternative dominated the test-period, but "satiation had *no* effects at all when the black-white relationships of satiating figure and the corresponding portion of the test-pattern were reversed."[16] Hochberg did not, however, determine whether the *RAC* of the test-pattern increased under this condition.

Method. Figs. W, T, and D were employed (see Fig. 2). Since Fig. D was the printed negative of Fig. T, both figures were identical except that the black-white relations were reversed. As before, Fig. W was a white homogeneous field.

Subjects. Twenty-eight *S*s were employed; 21 of these had participated in Experiment I. *S* was tested once under the experimental and the control conditions. The experimental trial began with fixation of Fig. W for 1 min., then *S* fixated Fig. T for three consecutive 15-sec. observational periods which were followed by Fig. D for three additional periods. *S* responded to apparent changes in both the inspection- and test-figures. Instructions were the same as in Experiment I.

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[15] Köhler and Wallach, *op. cit.,* 331.

[16] J. E. Hochberg, Figure-ground reversal as a function of visual satiation, *J. exp. Psychol.,* 40, 1950, 684.

S received the control trial at least 40 min. after the experimental. The control condition was identical with the experimental in all respects other than what was seen during the inspection-period. *S* continued to fixate Fig. W throughout the 45-sec. inspection-period and then Fig. D was immediately presented. If increased *RAC* of Fig. D during the experimental condition was simply due to either the lengthened period of observation, or to the change in the brightness of the background, *RAC* during the control test-period should equal the rate obtained during the experimental test-period. On the other hand, if increased *RAC* of Fig. D in the experimental condition was partially determined by the presence of the previously viewed figure, *RAC* in the experimental condition should significantly exceed that obtained in the control trial.

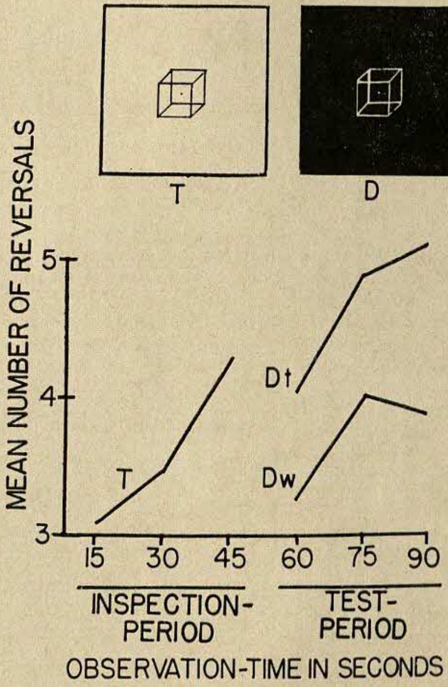

FIG. 2. RATE OF APPARENT CHANGE AS A FUNCTION OF OBSERVATION-TIME AND PRIOR STIMULATION IN EXPERIMENT II

Capital letters designate figure to which responses were made; subscripts identify the preceding inspection-figure. Insets show Figs. T and D as they appear in the apparatus.

Since each *S* received the control trial after the experimental condition, any residual effect due to order would be expected to operate against our hypothesis, since it would have resulted in a higher control *RAC*, *i.e.* more nearly equal to that obtained in the preceding experimental trial.

Results. Analysis of variance showed that observational periods (*OPs*) under the different conditions differed significantly from one another ($p < 0.001$). The average *RAC*-values for each of the three experimental *OPs* were significantly higher than the corresponding control values ($p < 0.05$). Despite the over-all brightness-difference between Figs. T and D, no significant differences were found between the *RAC* of Fig. T in the experimental condition and Fig. D in the control trial (see Figure 2). We may conclude, therefore, that inspection of a black-on-white cube increases the *RAC* of a subsequently fixated white-on-black cube.

EXPERIMENT III. EFFECT OF PERIPHERAL VISION ON RATE OF APPARENT CHANGE OF CENTRALLY FIXATED *T*-FIGURES

Köhler and Wallach state that "a figure which has become unstable in its original location must again appear more stable when it is shown in a new position."[17] It was therefore expected that inspection-figures which were displaced to the left or right of the fixation-point would not induce an increased *RAC* of a centrally fixated test-figure.

TABLE II

EXPERIMENTAL CONDITIONS OF EXPERIMENT III

Condition	Adaptation-period (60-sec.)	Inspection-period (45-sec.)	Test-period (45-sec.)
L	Fig. W	Fig. L	Fig. T
R	Fig. W	Fig. R	Fig. T
T	Fig. W	Fig. T	Fig. T
W	Fig. W	Fig. W	Fig. T

Method. Each *S* was tested once under each of four conditions which differed from one another only by what was presented during the 45-sec. inspection-period. The four conditions are shown in Table II.

Figs. R and L were identical in size and form with Fig. T, but Fig. L was displaced to the left, and Fig. R to the right of the fixation-point. The fixation-point of Fig. T was centered within the cube. When proper fixation was maintained, there was a gap of 6 mm. between the medial edges of Figs. L and R, and the lateral edges of Fig. T.

Each of a new group of 24 undergraduate students who served as *Ss* was randomly assigned to one of the 24 possible sequences of the four conditions. A rest-interval of 3 min. was interposed between conditions.

Since pretesting revealed that *Ss* tended to shift their gaze to Figs. R and L when instructed to respond to apparent changes in these figures (thus producing partial retinal coincidence of the contours of the inspection- and test-figures), *Ss* were instructed to respond only to fluctuations of Fig. T. At the conclusion of testing, *Ss* were asked to comment upon the appearance of Figs. R and L. A large majority reported seeing few, if any, fluctuations during inspection of these figures, and a number of these *S*s added that R and L did not appear to be three-dimensional.

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[17] Köhler and Wallach, *op. cit.*, 335.

Results. Since differential effects of the four inspection-conditions should be (and are) most apparent during the first observational test-period, only these test-values were employed in the analysis. In addition, these test-values were compared with the *RAC* of Fig. T during the first observational inspection-period of Condition T to determine whether the rate of apparent change in the test-periods exceeded the initial level of *RAC*.

The variances of these values were not homogenous; this condition was corrected by a $(+0.5)^{1/2}$ transformation. Analysis of variance yielded a significant F for conditions ($p < 0.001$), and ts between the mean *RAC*-values were computed (see Table I).

Only when Fig. T was its own inspection-figure (Condition T) was there a significant increase in the *RAC* during the test-period ($p < 0.001$). When Fig. T was preceded by Figs. L, R, or W, no increase in *RAC* above the initial level obtained during the inspection-period of Condition T was observed ($p < 0.05$). Comparisons among the test-values obtained under the four conditions show that the *RAC* in Condition T was significantly higher than that in Conditions L, R, and W ($p < 0.001$), and that the latter conditions did not differ from one another ($p > 0.05$). (see Fig. 3)

Two alternative explanations for these results are suggested. (1) A common retinal locus of the contours of the inspection- and test-figure may be essential for increased test-*RAC;* or (2) increased *RAC* may depend on perceptual similarity of inspection- and test-figures. In the latter case, common location of contours may normally operate only to enhance this similarity. The possibility that a high inspection-*RAC* or a high rate of response during the inspection-period, or both, are necessary for increased test-*RAC* is considered in a later section.

EXPERIMENT IV. COINCIDENCE OR PROXIMITY OF THE CONTOURS OF THE INSPECTION- AND TEST-FIGURES

In Experiment IV, we investigated the possibility that retinal coincidence of inspection- and test-patterns was a sufficient condition for evoking increased *RAC* of an ambiguous test-figure. Specifically, our hypothesis was that fixation of an inspection-figure which did not resemble the test-figure but which either incorporated or was in close proximity to the contours of the test-figure, would induce a significantly higher test-*RAC* than merely inspecting a homogenous field. This follows directly from Köhler and Wallach, who wrote:

Patterns in the visual cortex are, both geometrically and in terms of conductivity, strictly equivalent to the retinal patterns to which they are related. As a consequence figure-currents in the cortex will be distributed just as though the cortex coincided with the retina, and as though cortical objects were identical with retinal images.[18]

Method. A new group of 24 undergraduates served as *Ss*. Each *S* was tested once under each of four conditions which differed only by what was observed during the inspection-period. Each *S* was randomly assigned to one of the 24 possible sequences of conditions. The four conditions are shown in Table III. A rest-interval of 3 min. was interpolated between conditions.

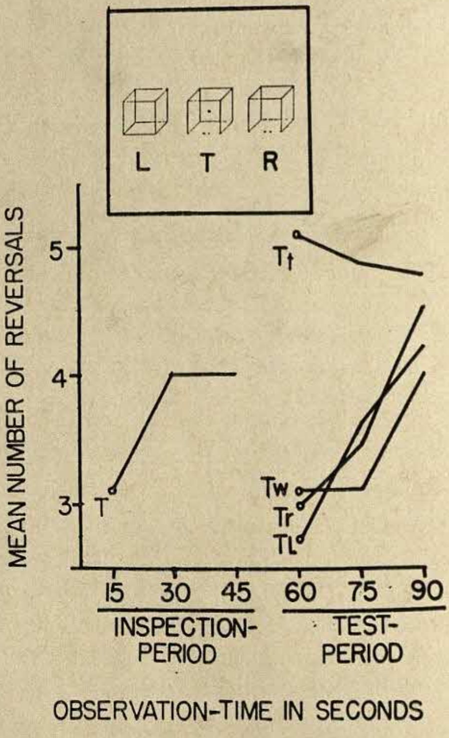

Fig. 3. Rate of Apparent Change As a Function of Prior Stimulation and Observation-Time in Experiment III.

Capital letters designate figure to which responses were made; subscripts identify the preceding inspection-figure. Inset shows a superimposition of Figs. L, R, and T to indicate their position relative to the fixation-point and to one another. Open circles identify the values employed in statistical analysis reported in text.

Fig. E was a modified Gottschaldt figure, in which the contours of the test-cube were 'embedded.'[19] Fig. S was drawn so that its contours closely bracketed, but did not coincide with, the contours of the test-cube. These relationships of inspection- and test-contours were, of course, dependent upon fixation of the dot centered in each of the figures.

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[18] Köhler and Wallach, *op. cit.*, 335.

Since Figs. E and S did not appear to fluctuate, *Ss* responded only to the apparent changes in Fig. T.

Results. As in the previous experiment, the mean *RAC*-values obtained for Fig. T during the first *OP* of the inspection-period of Condition T and the first *OP* test-periods of all conditions were used to test our hypotheses. Analysis of variance yielded a significant *F* for Conditions ($p < 0.01$) and *t*s between mean *RAC*-values were computed (see Table I).

Only when Fig. T was employed as its own inspection-figure (Condition T) was there a significant increase in the *RAC* during the test-period. When Fig. T was preceded by inspection of Figs. E, S, and W, reversal-rate was observed during the first *OP*-inspection of Condition T. Comparison among the values of the test-periods shows that *RAC* in Condition T was higher than that in Conditions E and S ($p < 0.01$), and higher than Condition W, although this difference failed to reach significance. The high *RAC* observed in Condition W may perhaps be due to the fact that the fixation-points of Figs. W and T were somewhat larger than those of Figs. E and S; this 'similarity' of W and T may have been sufficient to produce the observed high level (see Fig. 4). In any case, these results do not support the hypothesis that mere coincidence or close proximity of inspection- and test-contours is sufficient to produce increased *RAC* of the test-figure.

TABLE III

EXPERIMENTAL CONDITIONS OF EXPERIMENT IV

Condition	Adaptation-period (60-sec.)	Inspection-period (45-sec.)	Test-period (45-sec.)
E	Fig. W	Fig. E	Fig. T
S	Fig. W	Fig. S	Fig. T
T	Fig. W	Fig. T	Fig. T
W	Fig. W	Fig. W	Fig. T

EXPERIMENT V. FORM VS. SIZE

Experiment V tested the hypothesis that inspection-figures presented in central vision which are perceived as similar in form to the test-figure, but which differ in size from the test-pattern, can induce an increased *RAC* of the test-pattern.

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[19] Kurt Gottschaldt, Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren, *Psychol. Forsch.,* 8, 1926, 261-317; 12, 1929, 1-87.

In a study of figure reversal after frontal brain injury, Feinberg presented six Necker cubes of different sizes in ascending and descending orders under 'active' or 'passive' instructions.[20] Each cube was presented for 30 sec., and *Ss* were instructed to respond to apparent changes by striking a key. No significant difference between control and patient groups was observed under 'passive' instruction. Re-analysis of these data indicated that *RAC* increased as a function of observational time, despite the differences in stimulus-size throughout the series. Feinberg's *Ss* fixated, however, a corner of the central square of the cube in each *OP,* thus producing considerable coincidence of successive figural contours.

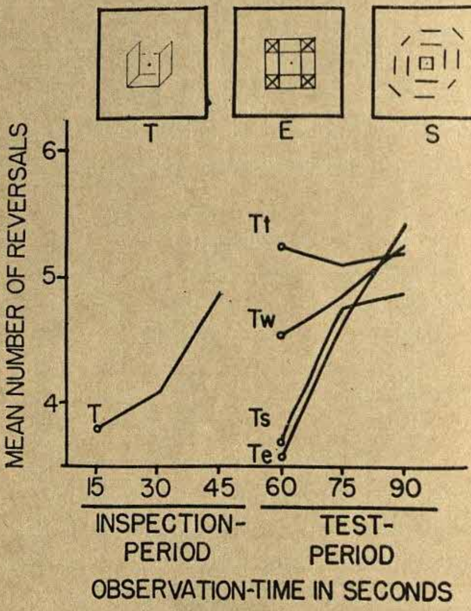

FIG. 4. RATE OF APPARENT CHANGE AS A FUNCTION OF PRIOR STIMULATION AND OBSERVATION-TIME IN EXPERIMENT IV

Capital letters designate figure to which responses were made; subscripts identify the preceding inspection-figure. Insets show Figs. E, S, and T as they appear in the apparatus. Open circles identify the values employed in statistical analysis reported in text.

The inspection- and test-figures in the present experiment were centrally fixated and were of such dimensions that no coincidence of inspection- and test-contours occurred with proper fixation. Since the large inspection-cube completely enclosed the retinal area, and thus the cortical, upon which the test-cube was subsequently presented, satiation of the test-cube obtained under this condition should be greater than that obtained with the smaller inspection-cube which was placed within the test-figure.

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[20] Feinberg, *op. cit.*

Method. A new group of 24 undergraduate students served as *S*s. Each *S* was tested once under each of the four conditions which are presented in Table IV. *S*s were randomly assigned to the 24 possible sequences of conditions. A rest-interval of 3 min. was interposed between successive conditions.

Fig. La was a cube large enough to permit Fig. T to fall entirely within its central square; Fig. Sm was a cube small enough to fall within the central square of Fig. T. The verticals of the large, middle and small cubes were 48 mm., 17 mm., and 6 mm., respectively. *S* was instructed to respond to apparent changes of both inspection- and test-figures.

Results. The *RAC*-values obtained for the first *OP* of the inspection- and test-periods under each condition were compared by analysis of variance.

TABLE IV

EXPERIMENTAL CONDITIONS OF EXPERIMENT V

Condition	Adaptation-period (60-sec.)	Inspection-period (45-sec.)	Test-period (45-sec.)
La	Fig. W	Fig. La	Fig. T
Sm	Fig. W	Fig. Sm	Fig. T
T	Fig. W	Fig. T	Fig. T
W	Fig. W	Fig. W	Fig. T

The *F* for conditions was significant ($p < 0.001$) and the relevant *t*-tests were performed (see Table I).

The *RAC* of the three inspection-figures did not differ significantly from one another, although Fig. La tended to fluctuate at a somewhat lower rate than Figs. Sm and T. Inspection of each of these figures induced a test-*RAC* which was significantly higher than that obtained during the inspection-period of Condition T. The test-*RAC* in Condition W did not differ significantly from the inspection-value of Condition T.

When the test-values of the different conditions are compared, the mean of Condition T is significantly higher than that of Condition Sm ($p < 0.01$), Condition La ($p < 0.001$), and Condition W ($p < 0.001$). Conditions La and Sm both yielded higher *RAC*-values than Condition W ($p < 0.01$ and $p < 0.001$, respectively), but did not differ significantly from one another ($p > 0.05$) (See Fig. 5).

We conclude that inspection of cubes differing only in size from the test-cube can induce an increase in the *RAC* of the test-figure even when inspection- and test-contours do not coincide. This increase is not as marked as that produced by fixation of the identical figure during the inspection- and test-periods. Note that the larger inspection-cube tended to induce a lower test-*RAC* than did the smaller cube. This result contrasts with the expectation that figural after-effects should be greatest within the portion of the visual field which was enclosed by an inspection-figure.

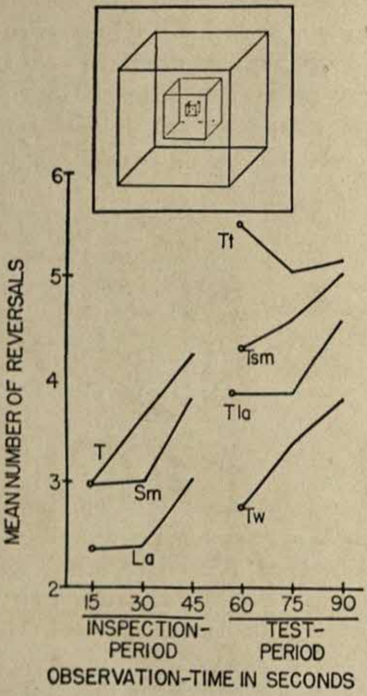

FIG. 5. RATE OF APPARENT CHANGE AS A FUNCTION OF PRIOR STIMULATION AND OBSERVATION-TIME IN EXPERIMENT V

Capital letters designate figures to which responses were made; subscripts identify the preceding inspection-figure. Inset shows a superimposition of Figs. La, T, and Sm to indicate their position relative to one another and to the fixation-point. Open circles identify the values employed in the statistical analysis reported in the text.

EXPERIMENT VI. INVESTIGATION OF ALTERNATIVE INTERPRETATIONS

Although the data of Experiments I-V are consistent with the hypothesis that inspection- and test-figures must be similar in perceived form to induce an increase in test-*RAC*, a review of the results of these experiments permits at least three other explanations.

(*a*) One possibility is that the *RAC* of the inspection-figure may be decisive in determining the *RAC* of the test-figure. Where inspection-figures did not fluctuate at all (Fig. W in all experiments, and Figs. E and S in Experiment IV) or appeared to change at a very low rate (Figs. L and R in Experiment III), no increase in test-*RAC* was noted. When inspection-figures appeared to reverse at a somewhat higher rate (Figs. La and Sm in Experiment V) the rise in test-*RAC* seemed to correspond to the level of change achieved during the inspection-period.

(*b*) A second possibility is that *response* during the inspection-period may somehow have determined the level of response during the test-period, since only those inspection-figures to which responses were made produced an increase in test-*RAC*.

TABLE V

Experimental Conditions of Experiment VI

Condition	Adaptation-period (60-sec.)	Inspection-period (45-sec.)	Test-period (45-sec.)
RV	Fig. W	Fig. RV	Fig. T
C	Fig. W	Fig. C	Fig. T
T	Fig. W	Fig. T	Fig. T
W	Fig. W	Fig. W	Fig. T
Tnr	Fig. W	Fig. T (no response)	Fig. T

(*c*) A third possibility is that only inspection-figures which appear to be clearly three-dimensional can induce an increased *RAC* in a three-dimensional test-figure.

Method. *Ss* were tested once under each of five conditions. Conditions T and W were identical with the conditions similarly designated in the previous experiment. Conditions RV and C employed a modification of the Rubin vase, and a cylinder with reversible perspective, respectively, as inspection-figures. Condition Tnr was identical with Condition T except that the *Ss* were instructed not to respond to fluctuations during the inspection-period. The five conditions are shown in Table V.

Six sequences of condition-order were randomly selected, and each of 30 new *Ss* was randomly assigned to one of the permutations of one of these sequences.

A rest-interval of 3 min. was interposed between successive conditions.

Results. The mean *RAC*-values obtained during the first *OP* of the inspection-and test-periods in each condition were compared by analysis of variance. A $(+ 0.5)^{1/2}$ transformation of the data corrected the non-homogeneity of variance. The *F* for Conditions was significant ($p <$

0.001) and *t*s between mean *RAC*-values were computed (see Table I).

Despite the fact that Condition RV has a significantly *higher* inspection-*RAC* than Conditions C or T ($p < 0.001$ and $p < 0.01$, respectively), it induced a significantly *lower* test *RAC* than that obtained in Condition T ($p < 0.001$). The test-value in Condition RV was even lower than that produced by fixating the homogeneous field (Fig. W) throughout the inspection-period ($p < 0.05$). Mere occurrence of a high *RAC* during the inspection-period thus cannot be responsible for increased *RAC* during the test-period. Similarly, although the inspection-*RAC*-values of Conditions C and T did not differ significantly from one another, Fig. C induced a test-*RAC* which was significantly lower than that of Condition T ($p <$

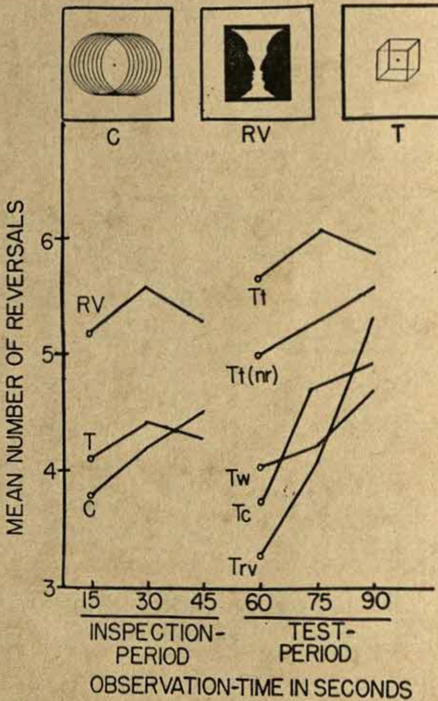

FIG. 6. RATE OF APPARENT CHANGE AS A FUNCTION OF OBSERVATION-TIME AND PRIOR STIMULATION IN EXPERIMENT VI

Capital letters designate figure to which responses were made; subscripts identify the preceding inspection-figure. Inset shows Figs. C, RV and T as they appear in the apparatus. Open circles identify the values employed in statistical analysis reported in the text.

.001) and did not differ significantly from the test-value of Condition W ($p > 0.05$). Since Fig. C appeared to be three-dimensional and considerably intersected the contours of the test-cube, this result also shows that neither of these factors is sufficient to produce increased *RAC*. It does not appear likely that response as such during the inspection-period is required to produce such increase, since the test-values of Conditions T and Tnr do not differ significantly from one another ($p > 0.05$), and both values are significantly higher than those obtained under all other conditions (see Fig. 6).

SUMMARY AND CONCLUSIONS

These experiments show that increased reversal rate exhibits at least five characteristics which have been reported for figural after-effects. We also find an additional factor which is not known to influence figural after-effects, but which is apparently crucial for producing increased *RAC*. This difference seems sufficiently important to justify a reëxamination of the hypothesis that both phenomena are mediated by the same central processes.

The common features of both effects can be described as follows: *(1)* Figural after-effects grow in magnitude as a function of inspection-time to a maximum which is obtained after 1–2 min. of fixation. This increase is paralleled by an increase in *RAC* when a reversible figure is continuously fixated.

(2) Just as inspection of a black-on-white figure can induce figural after-effects in a white-on-black test-figure, we find that a black-on-white Necker cube increases the *RAC* of an otherwise identical white-on-black test-cube.

(3) Both effects seem to require the presentation of inspection- and test-patterns to the same portion of the visual field.

(4) Fixation of a homogeneous field is insufficient to induce figural after-effects in a subsequently presented test-figure, and is similarly ineffective in producing increased *RAC* in a Necker cube.

(5) Neither figural after-effects nor increased *RAC* seem to be adequately accounted for on the basis of a generalized perceptual 'norm' or a response 'set' which is built up during the inspection-period.

The difference between the determinants of both phenomena becomes apparent when we consider the role of perceptual similarity of form. In the present experiments, all inspection-figures, which were perceived as similar in form to the test-figure (Necker cube), produced an increased *RAC* in the test-cube. Inspection-figures which did not appear cube-like, either because of their contour relationships or because they were presented in the periphery of the visual field, did not increase *RAC* during the test-period. Even when inspection-contours coincided with, intersected, or were in close proximity to the contours of the test-figure, inspection of figures which were not seen as cubes was as ineffective in elevating test-*RAC* as was inspection of a homogeneous field.

By contrast, figural after-effects are often obtained when inspection- and test-figures are markedly dissimilar in appearance.[21] Prentice, and Hochberg and Bitterman have shown, furthermore, that after-effects are influenced by the retinal, rather than by perceived size of inspection- and test- figures.[22] It seems fairly clear, therefore, that perceived 'similarity' may be unnecessary for figural after-effects, but crucial for increased *RAC*. This finding militates against the hypothesis that figural after-effects and reversal-rate are mediated by identical physiological processes.

It must be admitted that 'similarity' has not yet been adequately described in objective and measurable terms. Indeed the most useful definition of 'similarity' may well be in terms of *response* to stimuli rather than by objective comparison of their physical parameters. From this point of view, observing the effects of inspection-stimuli on reversal-rate of test-figures may offer a method for defining the conditions under which 'similarity' may exist or be altered.

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[21] For example, Figs. 11 and 23-30 in Köhler and Wallach, *op. cit.*, 278, 285-289.

[22] W. C. H. Prentice, On the size of the figural after-effect with varying distances, this JOURNAL, 63, 1950, 589-593; J. E. Hochberg and M. E. Bitterman, Figural after-effects as a function of the retinal size of the inspection-figure, this JOURNAL, 64, 1951, 99-102.

A RELATIONSHIP BETWEEN INCREMENTS OF DISTANCE AND ESTIMATES OF OBJECTIVE SIZE

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Numerous experiments have been concerned with the problem of apparent size and its relation to the size of the retinal image and physical distance. While theoretical disagreement exists in this area, there should be little dispute about some propositions of a descriptive nature. One of these is the statement that under normal viewing conditions, a near object which recedes a short distance from the subject (*S*), with proportional reduction in size of its retinal image, is perceived as remaining constant in size or as tending to regress toward the size projected by the visual angle. In an earlier report, the present writer has cast doubt upon the validity of this statement.[1] He showed that a group of *S*s, given free binocular regard, ample illumination, and normal cues of depth, made judgments, at points 2 ft. and 10 ft. distant from them, which indicated an *increase* in apparent size rather than constancy or decrease in apparent size. Though the increase was small, it was statistically highly significant.

This finding, if confirmed in subsequent work, would require revision of existing theories of size-distance perception.[2] A series of experiments was therefore planned to study more fully the direct relationship between small increments of distance and estimates of objective size. The first experiment of the series had two aims besides that of replicating the former results. First, it was intended to incorporate a third distance of comparison at a point intermediate between the two earlier studied. Using, as before, a standard reference-object at a remote distance, it was expected that the intermediate distance would yield size-matches smaller than those at the near point and larger than those at the farthermost point. Such a sequence of matches would confirm the hypothesis of a gradual increase in apparent size with increasing distance.

Secondly, it was intended to study the effect on size-matches of systematic change in illumination of the stimulus-objects. The earlier experi-

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* Received for publication October 1, 1958.

[1] Noël Jenkin, Effects of varied distance on short-range size judgments, *J. exp. Psychol.*, 54, 1957, 327-331.

[2] Kurt Koffka, *Principles of Gestalt Psychology*, 1935, 75-105, 237-240; W. H. Ittelson, The constancies in perceptual theory, *Psychol. Rev.*, 58, 1951, 285-294; R. S. Woodworth and Harold Schlosberg, *Experimental Psychology*, 1954, 480-491.

ment had been conducted under conditions of natural lighting, hence it is possible that one of the positions compared might have received slightly more illumination than the other. If objective judgments of size are appreciably influenced by degree of illumination, there should be a significant change in the mean size-match when the illumination of the standard is more than doubled.

EXPERIMENT I

Subjects. The *S*s were 7 men and 5 women, all but one of whom were staff members of The Training School. Some of them were experienced psychologists; all had had some training in psychology and had formerly served as *S*s in psychological experiments. None of them was aware of the hypotheses which the present experiment was designed to test.

Apparatus. The apparatus consisted of white cardboard squares. The standard card, 4 in. square, was attached by a steel wire to a ringstand, and was placed directly in front of *S* at a distance of 320 in. from his eyes. The comparison-series was a set of squares ranging from 2¾—6 in. by steps of ⅛ in. They were individually displayed at the same height as the standard upon a platform which was situated on *S*'s right. The angular separation between the standard and the comparison was 87°. The platform, which was adjustable in height, measured 25¾ × 23½ in., and was painted a dull green. A homogeneous field against which the square was viewed was provided by a wooden screen, measuring 20 × 25¾ in., attached to the rear of the platform. A similar screen, measuring 30 × 72 in. was placed 69 in. behind the standard. Both screens were painted a dull green to match the platform. Concealed pins and a slight depression, invisible to *S*, served to maintain the comparison-cards in vertical position in the middle of the platform. *S* was seated in a room facing a doorway which opened into a hall where the standard was situated. The comparison-object was placed in the same room as *S* and no objects were interposed between *S* and either of the stimulus-objects. Little daylight entered either the experimental room or the hall. The comparison-object was illuminated with a 75-w. floodlight, so placed as to give an illumination of 11 ft. candles (measured by a Macbeth Illuminometer) from a correctly positioned square. The illumination of the standard was varied. Under the less bright condition, a 75-w. floodlight was placed to give a reading of 11 ft. candles. Under the brighter condition, a 150-w. floodlight was placed to give a reading of 26.5 ft. candles. The large screen which provided a background for the standard was separately illuminated by a 75-w. floodlight placed 91 in. from the screen.

Design. Every *S* judged the size of the standard with the comparison-object at three different distances: 20, 40, and 160 in. There were two trials at each distance—a trial consisting of an ascending and a descending series. Every *S* also judged the size of the standard under each condition of brightness. Half of the *S*s (Group A) made their judgments under the 75-w. lamp (less bright condition) in the first session and then made their judgments under the 150-w. lamp (brighter condition) in a second session conducted on a subsequent day. For the other half of the *S*s (Group B), this order was reversed. In each group, half of the *S*s began with an ascending series and half began with a descending series. To distribute any possible effects of order of distances, each *S* in Group A was presented with a different permutation of the three distances. The six possible permutations were replicated in Group B. Each pair of ascending and descending measures was averaged. Twelve observations from each *S* were thus included in the analysis.

Procedure. The height of the stimulus-objects and *S*'s eye-level were properly matched. The platform was placed at the selected distance from the bridge of *S*'s nose. In the case of the 20- and 40-in. distances, a measuring tape was used each time the platform was moved. *S* was instructed to keep his eyes lowered except when making a judgment. He was requested to decide whether each comparison-card was smaller, equal to, or larger than the standard. 'Equality' meant equality of physical dimensions, a concept illustrated by presenting *S* with two cards of identical size (3-in. square). He was requested to make his judgments in each series independent of his judgments in any preceding series and was informed that each series would be started with a different-sized card. Prior to the test series proper, the procedure was illustrated by presenting two comparison-cards with sizes outside the range of the comparison-series, and obtaining judgments regarding each (one being smaller than the standard and the other larger). After four series of judgments in alternate ascending and descending order, the platform was moved to the second distance and a further set of judgments was made. Finally, another set of four series of judgments was obtained at a third distance. This completed the first session. A second session was identical, except for the changed illumination and for omission of the practice-demonstrations and practice-judgments. *S* was not encouraged to look back and forth between the standard and comparison, but was not prevented from doing so if he wished. *E* used two sets of comparison-cards identical with each other in sizes, randomly selecting one set or the other from series to series.

TABLE I

MEAN SIZE-MATCHES (IN IN.) OF A STANDARD UNDER TWO CONDITIONS OF ILLUMINATION
(Standard=4-in. square, situated 320 in. from *S*.)

	Distance of comparison-object from *S*		
Illumination (standard)	20 in.	40 in.	160 in.
150-w.	4.561	4.458	4.266
75-w.	4.549	4.477	4.297
Grand mean	4.555	4.467	4.281

Results. The mean size-matches for two conditions of illumination and for the three distances of the comparison-object are shown in Table I. From this it is clear that as the comparison-object recedes from *S*, the mean size-matches become smaller. To test the significance of this and the other sources, it was necessary to apply a logarithmic transformation to the data which showed heterogeneity of variance. Preliminary analysis showed that the Group-variable (order of experiencing the two conditions of illumination) was nonsignificant and had negligible interactions with the other variables. This left three main sources to be included in an analysis of variance, the results of which are shown in Table II. The Distance-variable (D) is significant at a very high level of confidence, while the Brightness- and Trial-variables (B and T) have F-values lower than unity. In addition to the over-all analysis, it was thought desirable to test the significance of the difference between adjacent pairs of distances. Analysis yielded, for the 20-in. vs. 40-in. pair, an F of 4.28 (1 and 77 *df*., $P < 0.05$), and, for the 40-in. vs. 160-in. pair, an F of 22.88 (1 and 77 *df*., $P < 0.001$).

The present data support the results obtained from the author's previous experiment and indicate that the former results are not likely to have been an artifact of uncontrolled lighting conditions.[3] The present data also extend the previous finding, in that use of an intermediate distance is found to result in size-matches intermediate between the values obtained respectively from very close and relatively remote placements of the comparison-object. The third comparison-distance now makes it possible to plot the results graphically. The grand means (which combine the two conditions of illumination) plotted against distance gave three points which were fitted by a perfect logarithmic curve. Fig. 1 shows the mean size-matches plotted against the logarithms of the distances. The straight-line relationship suggests the existence of some hitherto undiscovered law relating apparent size and short increments in distance. It is noteworthy that the slope of the line in Fig. 1 is in a direction opposite to that which would be predicted from the law of the visual angle. Matches selected on the latter basis would have been 0.25 in. at 20-in. distance, 0.5 in. at 40-in. distance, and 2.0 in. at 160-in. distance.

TABLE II

ANALYSIS OF VARIANCE OF SIZE-MATCHES
(Based on logarithms of the raw measures)

Source	*df.*	Mean square	*F*
Distances (D)	2	8313	20.13*
Brightnesses (B)	1	10	—
Trials (T)	1	284	—
Subjects	11	22130	53.58*
D×B	2	70	—
D×T	2	71	—
B×T	1	90	—
D×B×T	2	39	—
Residual	121	413	

* Significant beyond the 1/10% level of confidence.

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[3] Jenkin, *op. cit.*, 328.

EXPERIMENT II

The second experiment was a modified replication of Experiment I. The same physical situation was employed, with the sizes and distances of stimulus-objects identical with those of the former study. In Experiment II, however, an additional distance (80 in.) was inserted between the 40- and 160-in. positions. All judgments were secured under the 75-w. illumination. A further modification involved an inspection-period prior to each session of judging. This means that *S* examined the standard at a distance of 24 in. for 5-sec., after which it was placed at 320 in. The reason for this procedure lay in the finding that some *S*s of Experiment I had greatly overestimated the size of the standard, while others had overestimated it but little. Since the present series of experiments was not intended to study relationships between the size of the standard and the comparison-judgment, it seemed pointless to impede *S*'s task by making him judge the size of a totally unfamiliar object. The present study is concerned with relationships between the size of the match and the distance at which it is made. Any procedure, therefore, which would reduce the ambiguity of the standard object could be expected to increase the accuracy of the results and reduce the inter-individual variability, without affecting the gradient formerly discovered.

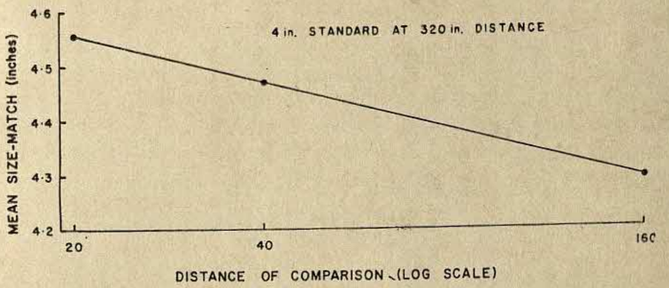

FIG. 1. MEAN SIZE-MATCHES AS A FUNCTION OF *S*'S DISTANCE FROM THE COMPARISON-OBJECT
(Experiment I)

It was predicted, then, that notwithstanding the provision of a pre-test inspection-period, the four mean size-judgments at different distances of the comparison would, when plotted graphically, yield points which all fall upon or near to a straight line relating apparent linear size to increments of distance on a logarithmic scale. The direction of the relationship was expected, as before, to be toward decreasing size-matches with increasing distance.

Subjects. The *Ss* were four men and two women, all but one of whom were employed as psychologists or research assistants at The Training School. The exception was a graduate student who had had some training in psychology. All *Ss* were well motivated to perform their task with care and accuracy. None of them was aware of the hypothesis under investigation. The six persons were selected on the basis of (a) their availability for daily tests over a period of time, (b) the adequacy of their natural or corrected vision, (c) the stability of their size-distance judgments as determined from a trial-session. One potential *S* was discarded for the reason of poor vision and two were discarded because of an atypical variability in their judgments.

Design. The standard remained, excepting for the brief inspection-period, at a constant distance of 320 in. from *S* throughout the experiment. There were four distances of the comparison-object: 20, 40, 80, and 160 in. Every *S* made judgments from an ascending and a descending series at each distance in each session. Since there are 24 permutations of the four distances, the order was varied in the 24 possible ways. Every *S* was thus presented with a different order of the comparison-distances in each of his four sessions, and no *S* at any time was presented with the same order given to any other *S*. Three *Ss* began each session with an ascending series and the other three began with a descending series.

Procedure. The procedure closely resembled that of Experiment I. Three categories of judgment were requested: smaller, equal, and larger. *S* was asked to try to make each judgment independent of each preceding judgment but to be consistent in use of his criteria for estimating physical equality. He was in particular requested to avoid being influenced in his judgments by the direction of the series, *e.g.* after his first 'equal' response to say 'smaller' on the next presentation *because* he knows it is a descending series. On the basis of previous experience, these instructions were designed to minimize constant errors and inter-individual variation.

After *S*'s position was established, the standard card was detached from the ringstand and placed 24 in. before *S*'s eyes. After he had scrutinized it for 5 sec., the standard was returned to its proper place and the judgments began.

Results. The mean size-matches for the four distances and the four sessions of judging are shown in Table III. As in Experiment I, decrements in the mean size-matches again follow increments of distance, and this trend is very consistent from session to session. Preliminary tests showed homogeneity of variance, and an analysis of variance was therefore performed on the raw measures. Data from ascending and descending trials were separately included in the analysis instead of being combined. The reason for this was that previous experiments have shown in this type of situation a constant error of anticipation. It is often assumed in psychophysical experiments that this error from ascending and descending series, respectively, is equal and opposite and hence may be cancelled out by averaging the results. In the course of the present experiment it was found that *S*s experienced more difficulty in discriminating the decrements in size of the comparison-series in the upper portion of its range than was the case with increments of size in the lower range. This opens the possibility that the error of anticipation in a descending series, expressed in the linear units of this experiment, may be greater than the same error in an ascending series. If the *total* error of anticipation decreases as a function of increasing distance, then the gradient found in previous work and in Experiment I could be interpreted as an artifact.[4] Hence it was thought necessary to test the significance of the interaction between 'Type of Trial' (ascending or descending) and 'Distances.'

TABLE III

MEAN SIZE-MATCHES (IN IN.) FROM FOUR SESSIONS OF JUDGING
(Standard = 4-in. square, situated 320 in. from *S*.)

Distances	Sessions				Grand mean
	1	2	3	4	
20 in.	4.292	4.073	4.167	4.151	4.171
40 in.	4.130	3.948	3.932	4.005	4.004
80 in.	4.010	3.859	3.870	3.859	3.900
160 in.	3.781	3.698	3.714	3.714	3.727

TABLE IV

ANALYSIS OF VARIANCE OF SIZE-MATCHES
(Based on raw measures; standard object 320 in. from S)

Source	*df.*	Mean square	*F*
Distances (D)	3	106.50	63.77‡
Sessions (S)	3	15.30	9.16‡
Types of Trial (T)*	1	14.36	8.60†
Subjects	5	85.19	51.01‡
D×S	9	0.82	—
D×T	3	1.43	—
S×T	3	1.42	—
D×S×T	9	0.34	—
Residual	155	1.67	

* Ascending *vs.* descending.
† Significant beyond the 1% level.
‡ Significant beyond the 1/10% level.

The results of an analysis of variance are shown in Table IV. 'Distance' is significant at an extremely high level of confidence, and 'Sessions' and 'Types of Trial' are also significant. None of the interactions approaches significance and it appears, therefore, that increasing size-matches with decreasing distance cannot be explained as an artifact of the type suggested above.

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[4] Jenkin, *op. cit.*, 328 f.

The variability due to 'Sessions' is accounted for principally by a decrease in the size of all matches from Session 1 to Session 2. Table III indicates, however, that the expected decrease in the size of matches with increasing distance was maintained in every session. Increased familiarity with the situation evidently decreased the over-all size of the judgments but did not affect the gradient which is under investigation. Tests of significance of the difference between size-matches for the pairs of adjacent distances found an *F* of 26.5 for 20-in. vs. 40-in. (1 and 75 *df.*, P < 0.001), an *F* of 14.01 for 40-in. vs. 80-in. (1 and 75 *df.*, P < 0.001) and an *F* of 30.71 for 80-in. vs. 160-in. (1 and 75 *df.*, P < 0.001). From these data and from a study of Table III it may be inferred that the mean size-matches at each distance were, at a high level of confidence, different from the mean size-matches at each other distance.

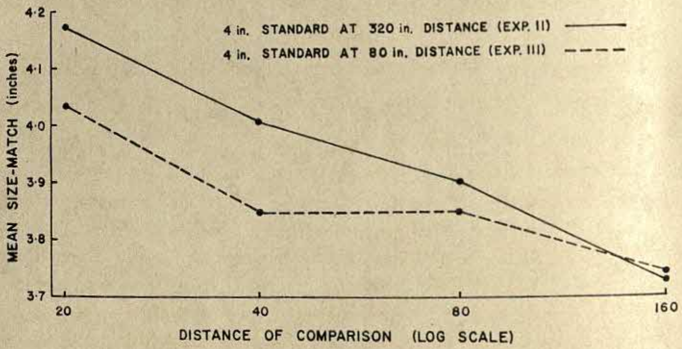

FIG. 2. MEAN SIZE-MATCHES AS A FUNCTION OF *S*'S DISTANCE FROM THE COMPARISON-OBJECT: STANDARD AT TWO DIFFERENT DISTANCES

A plot of the mean size-matches against the logarithms of the distances is shown by the solid line in Fig. 2. It will be seen that its direction of slope and its closeness to linearity confirm the prediction. The inclusion of a fourth judging-distance (80 in.), not investigated in Experiment I, yielded a mean size-match intermediate between that obtained at 40 in. and that obtained at 160 in. The two experiments therefore provide convincing evidence for the hypothesis that when a remote standard 4-in. square is employed, comparison size-matches over a short range of distances are linearly related to these distances on a logarithmic scale, in the direction of decreasing size-matches with increasing distance.

EXPERIMENT III

In the work earlier reported, and also in Experiments I and II of the present study, comparison-judgments at one distance have been compared with comparison-judgments at another distance rather than with the size of the standard. The latter has merely served as a reference-object, remaining constant throughout the experiment. The gradient found between estimates at the near and far positions of the comparison evidently means that changes in its physical distance from *S* are accompanied by systematic changes in apparent size or apparent distance or both, which do not correspond exactly to the physical properties of the situation. There is no reason to suppose that the apparent size or apparent distance of the standard itself are in any way changed in the course of the experiment. It may be reasoned that the gradient obtained is a function only of the distance of the comparison-object and that the reduction in size-matches with distance will occur regardless of the placement of the standard.

Experiment III was designed to test this hypothesis. The apparatus, design and procedure were exactly as for Experiment II, with only one simple change. The standard was placed 80 in. in front of *S*, instead of 320 in., as formerly. It should be noted that this modification altered the stimulus-situation in two possibly important ways. First, the decreased distance of the standard made it much less ambiguous as to size. Secondly, whereas the standard at its previous distance had always projected a much smaller visual angle than would a correct, objective match at any comparison distance, the new placement of the standard provided no such consistency. Its angle was smaller than that of a correct match at the closest (20-in.) position of the comparison and greater than that of a correct match at the most distant (160-in.) position of the comparison.

The selection of 80 in. as the new distance for the standard provided two advantages. (1) In the event of rejection of the principal hypothesis, it made possible the strengthening of an alternative hypothesis. This is the possibility that decreasing size-matches may be related to decreasing ratios of distance between standard- and comparison-objects. Though this would seem to be improbable, it is true that such a statement could describe the results formerly obtained. From the new (80-in.) placement of the standard one would expect continued decrement of size-matches from 20 to 160 in. if comparison-distance alone is the relevant factor. If, on the other hand, the relevant factor is the *ratio* of the distances, then one would expect a steady decrement from 20 to 80 in., and then a rise in the size-match at 160 in. (2) Since one of the distances of the comparison-object is 80 in., placement of the standard also at 80 in. gives an equidistant judging-position. This may provide data useful as a baseline for interpreting the other judgments which involve disparate degrees of depth.

Experiment III was conducted with the same *Ss* who had served in Experiment II, and involved four more daily sessions of judging, following immediately upon the conclusion of the former study. Since the same *Ss* were used in the two experiments and since it was not possible to counterbalance for order of presentation of the two distances of the standard, it was necessary to control for the possibility that the results of Experiment III were partly due to practice. This led to Experiment IV, which will be described subsequently.

Results. The mean size-matches for the four distances and four sessions are shown in Table V. Inspection of the grand means shows a decrement in the mean size-match from 20 to 40 in. and from 80 to 160 in. The results of an analysis of variance performed on the raw measures are shown in Table VI. The 'Distance' is again significant at a very high degree of confidence, and the present analysis differs from that of Experiment II in revealing no significant differences between 'Sessions' and between 'Types of Trial.' Tests of significance of the difference between size-matches for the pairs of adjacent distances found an *F* of 84.37 for 20-in. vs. 40-in. (1 and 75 *df.*, $P < 0.001$), an *F* of zero for 40-in. vs. 80-in. (1 and 75 *df.*), and an *F* of 18.77 for 80-in. vs. 160-in. (1 and 75 *df.*, $P < 0.001$). From these data and from a study of Table V it is clear that significant differences occur between the means of all pairs except 40 and 80-in.

TABLE V

MEAN SIZE-MATCHES (IN IN.) FROM FOUR SESSIONS OF JUDGING
(Standard = 4-in. square, situated 80 in. from S)

Distances	Sessions				Grand mean
	1	2	3	4	
20 in.	4.005	4.005	4.042	4.089	4.035
40 in.	3.854	3.849	3.807	3.880	3.848
80 in.	3.891	3.828	3.839	3.839	3.849
160 in.	3.734	3.740	3.781	3.719	3.743

TABLE VI

ANALYSIS OF VARIANCE OF SIZE-MATCHES
(Based on raw measures. Standard 80 in. from S.)

Source	*df.*	Mean square	*F*
Distances (D)	3	45.28	40.43†
Sessions (S)	3	0.36	—
Types of Trial (T)*	1	0.11	—
Subjects	5	82.77	73.90†
D×S	9	0.90	—
D×T	3	0.52	—
S×T	3	0.13	—
D×S×T	9	0.17	—
Residual	155	1.12	

* Ascending vs. descending; † Significant beyond the 1/10% level.

The means are plotted against the logarithms of the distances in the broken line of Fig. 2. It will be seen that the direction and degree of slope correspond almost exactly to the results of Experiment II between 20 and 40 in. The line is horizontal between 40 and 80 in., indicating no decrement in size between these distances, and then from 80 to 160 in. it slopes again in the expected direction of decreased size.

The data of Experiment III confirm the principal finding of previous work; namely, that size-matches tend to decrease as the comparison recedes from *S*. The data this time do not show, however, the regular and almost perfect linear relationship found from Experiments I and II. This is possibly due to the intrusion of an equidistant judging-position, which enabled *S* to use criteria different from those which he applied to the situation of disparate distances.

It is clear that we cannot accept the alternative hypothesis of a relationship between size-matches and decreasing distance-ratios. Such a relationship would reveal itself in an upward slope of the line from 80 to 160 in. instead of a downward slope as is the case. It is also clear that while we can predict a general tendency to decreased size-matches with distance under the conditions of this experiment, we cannot expect a linear relationship where an equidistant judging-position is included in the group of distances employed.

Finally, it is possible from the present data to make some deductions concerning 'size-constancy.' It is usual in work on this problem to take the physical size of the standard as a baseline. When the standard is placed closer to *S* than the comparison-object, a match which is physically larger than the standard represents an imperfect tendency toward size-constancy, and when the match is smaller, 'overconstancy' is indicated. If we adopt this classical convention, the mean size-match at 160 in. represnets a gross overestimation, and the match at 20 in. represents only the very slightest overestimation. This convention of using the physical size of the standard as a baseline would, however, be inappropriate to the data of the present experiment. This is indicated clearly by the fact that at the equidistant-position the mean size-match was 0.151 in. smaller than the standard. This match at 80 in. represents subjective equality of size, taking into account factors such as different surrounding fields and methods of supporting the stimulus-objects. At the same time, this measure excludes the effect of distance. The equidistant judgment of 3.849 in. should therefore represent 'perfect' size-constancy, and deviations should be considered from this value. From this point of view, then, the two extreme positions of the comparison both yield overestimates. Expressed another way, it could be said that the effect of distance (at both of these two extreme positions) was to enhance the estimated size of the more distant of the two objects (standard and comparison).

EXPERIMENT IV

In discussing the objectives of Experiment III it was pointed out that the sequence of distances at which the standard object was presented was not counterbalanced. This raises the question as to whether the earlier experience with the 320-in. condition may possibly have influenced the performance at the subsequent 80-in. position. Previous experience indicates that this is unlikely.[5] Inspection of Fig. 2 does, however, reveal a distinct difference in slope between the two gradients, even if the equidistant match in Experiment III is ignored. If the reduced slope of the gradient in Experiment III (Fig. 2) is due to practice or to increasing familiarity with the situation, then a replication of Experiment II should show a slope which deviates from that originally obtained. If, however, the slopes are not significantly different, there will be some grounds for believing that the two sets of results (Experiments II and III) are independent.

TABLE VII

MEAN SIZE-MATCHES BEFORE AND AFTER AN INTERPOLATED SERIES OF JUDGMENTS
(Standard=4-in. square, situated 320 in. from S.)

Distances	First session (Experiment I)		Last session (Experiment IV)	
	Asc.	Desc.	Asc.	Desc.
20 in.	4.208	4.375	4.104	4.198
40 in.	4.021	4.240	3.896	3.917
80 in.	3.958	4.062	3.802	3.844
160 in.	3.771	3.792	3.646	3.667

A further reason for planning Experiment IV was the finding that repeated experience appears to have the effect of reducing by an approximately constant amount the size of matches at all distances, while affecting the slope of the gradient but little. This conclusion was reached from a study of the four sessions of Experiment II. These four sessions are not, however, strictly comparable with each other, since the orders of presentation of the comparison-distances were different from session to session. To compare earlier and later judgments under identical conditions except for the intervening experience, it was planned to conduct, for all *S*s, a repetition of Session 1 of Experiment II (standard at 320 in.) on the day following the conclusion of Experiment III.

Results. Table VII gives the mean size-matches for the first session

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[5] Jenkin, *op. cit.*, 329.

(these data taken from Experiment II) compared with the corresponding matches in the last session (Experiment IV). The complete breakdown is given in order to show the differences that were found between ascending and descending trials. Analysis of variance revealed differences between 'Distances' and between 'Sessions,' both sources significant at a very high level of confidence. The hypothesis that repeated practice reduces the over-all size of the matches is unequivocally confirmed. The question as to whether the slope of the gradient in Experiment IV differs significantly from that of Experiment I is answered by the absence of any 'Distance' by 'Session' interaction. There is a suggestion of a 'Session' by 'Type of Trial' interaction, Table VII indicating that there was a smaller error of anticipation in the last session than was the case in the first session. Table VIII shows, however, that this interaction is statistically nonsignificant.

TABLE VIII

ANALYSIS OF VARIANCE OF SIZE-MATCHES UNDER DIFFERENT CONDITIONS OF PRACTICE

(Based on raw measures. Standard 320 in. from S.)

Source	*df.*	Mean square	*F*
Distances (D)	3	67.31	38.46§
Sessions (S)	1	44.01	25.15§
Types of Trial (T)*	1	11.34	6.48†
Subjects	5	15.19	8.68‡
D×S	3	0.78	—
D×T	3	0.96	—
S×T	1	2.67	1.52
D×S×T	3	0.66	—
Residual	75	1.75	

* Ascending vs. descending; † Significant beyond the 5% level; ‡ Significant beyond the 1% level; § Significant beyond the 0.1% level.

The mean size-matches from the two sessions are plotted against the logarithms of the distances in Fig. 3. From this it is clear that the gradient obtained in the final session closely resembles that obtained in the first session and does not appear to have been affected by the exposure of *S*s to the repeated sessions of Experiments II and III.

DISCUSSION

Size vs. distance. The general conclusion from the present series of experiments is that objective binocular size-matches of a standard at a distance tend in a very consistent way to decrease as the comparison-object is presented at successively increasing distances from the point of observation. This behavior shows a direction which is opposite to the relationships given by the visual angles. In introducing the problem and presenting the data, a statement of the relationship has been expressed in terms of an increase of apparent size with increasing distance. The reason for this will be seen when it is recalled that in Experiment I, for example, the mean match at 160 in. was 4.3 in., a value which was rejected as 'smaller' at the 20 in. distance.

It is possible to interpret the data in terms of progressive overestimation of distance rather than enhancement of size. Direct evidence to justify a decision between these alternatives is not available in the present data. Some relevant literature does, however, suggest that the correct interpretation involves size rather than distance. It is well known that with visual angle constant, there is an association between increased binocular convergence and decreased apparent size. For example, Hermans has found that *Ss* consistently report a shrinking of apparent size with increasing convergence but disagree as to the direction of the change in apparent distance.[6] Heinemann, Tulving and Nachmias, using a size-distance judging situation under various reduced-cue conditions, demonstrated: (1) that changes in convergence are a sufficient condition for the occurrence of changes in apparent size and that changes in accommodation and pupillary diameter are not necessary conditions for changes in apparent

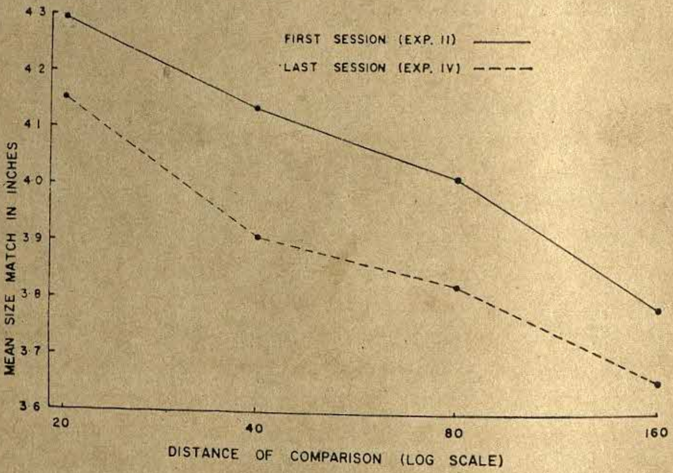

FIG. 3. MEAN SIZE-MATCHES AS A FUNCTION OF *S*'S DISTANCE FROM THE COMPARISON-OBJECT: EFFECTS OF PRACTICE

size,[7] and (2) that all *S*s consistently responded with decreased apparent size when convergence was increased, but in a session where distance judgments were required, gave different and conflicting reports about the relationships of the objects.

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[6] T. G. Hermans, The relationship of convergence and elevation changes to judgments of size, *J. exp. Psychol.*, 48, 1954, 204-208.

Findings such as these on the role of convergence in the perception of size plausibly suggest that in the present study, differences between the matches at the various distances of the comparison may be related to changing convergence. Since previous work has demonstrated that changing convergence is related to changing apparent size rather than to apparent distance, it would be reasonable to interpret the present data in a similar way. It must be remembered, of course, that the conditions of the present experiments were such as to allow a fairly accurate discrimination of the distances used for the comparison-object. The present argument is not that the *S*s failed to perceive the distances, but rather that they did not *progressively overestimate* the distances. The reduction in size of the match with distance is thus tentatively attributed to changing convergence and interpreted as changing apparent size.

Indirect support for this view comes from work which has challenged the hypothesis of size-distance invariance[8] and also from a recent study by Jenkin and Hyman.[9] The latter experiment used conditions very similar to those described in the present report. Judgments of objective size-distance were intercorrelated with verbal estimates of distance and a small but significant negative coefficient was found. The direction of the relationship indicates that *S*s tended to judge the distant object as being relatively small and relatively remote or as relatively large and relatively near. Such a trend is inconsistent with the view that in such a situation, size is overestimated on account of the overestimation of distance.

Size-constancy. The doctrine of size-constancy was formulated as a description of a commonly observed experience; namely, that objects which recede from a fixed viewing position remain constant in apparent size despite the reduction in size of the retinal image. Both Gestaltists and empiricists have tended to accept this description of events, based upon an observation which has more than 200 years of history.[10] When subjected to laboratory test, size-distance judgments are found to be influenced by so many different conditions that any simple notion such as 'size-constancy' is clearly inadequate to describe the phenomena involved. Some writers have found, for example, that distant objects tend to be systematically underestimated in size, a finding which led both Brunswik and Thouless to formulate 'indices' of size-constancy, or 'phenomenal regression,' as values between zero and unity.[11] On the other hand, some writers have found a fairly consistent tendency to *overestimate* the size of objects at considerable distances.[12] A recent report by Gilinsky suggests that contrasting task-attitudes are associated with the contrasting results,[13] *i.e.* the tendencies respectively to underestimate or overestimate the size of a distant object. Under 'retinal' instructions, Gilinsky's *S*s gave judgments smaller than the physical size of the standard, and under 'objective' instructions the same *S*s overestimated the standard when presented at distances of 100 to 400 ft.

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[7] E. G. Heinemann, Endel Tulving, and Jacob Nachmias, The effect of oculomotor adjustments on apparent size, this JOURNAL, 72, 1959, 32-45.

[8] F. P. Kilpatrick and W. H. Ittelson, The size-distance invariance hypothesis, *Psychol. Rev.,* 60, 1953, 223-231; H. E. Gruber, The relation of perceived size to perceived distance, this JOURNAL, 67, 1954, 411-426; The size-distance paradox: A reply to Gilinsky, this JOURNAL, 69, 1956, 469-476.

[9] Noël Jenkin and Ray Hyman, Attitude and distance-estimation as variables in size-matching, this JOURNAL, 72, 1959, 68-76.

In the present series of experiments, 'objective' instructions were used, but the distances employed were very short. Nevertheless, the same tendency toward overestimation was found to a significant degree. It is no more true to describe this behavior as 'size-constancy' than it is so to designate systematic underestimates from 'retinal' instructions. Instead of revising existing theories which attempt to account for 'size-constancy,' it would therefore be better to aim at theory which accounts for the empirical evidence on size-distance judgment under various conditions. The confusing term 'size-constancy,' which is highly questionable as a description of events, might well be abandoned by psychological literature.

The present evidence, which shows that overestimation is progressive, even at short viewing distances, suggests a possible explanation for the enhancement of estimated size with distance. The following theoretical outline takes into account two properties of objective binocular size-distance judgments: first, their failure to correspond with visual-angle relationships; and secondly, their systematic trend toward overestimation with distance.

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[10] E. G. Boring, *A History of Experimental Psychology,* 1950, 614.

[11] Egon Brunswik, Distal focussing of perception: Size-constancy in a representative sample of situations, *Psychol. Monogr.,* 56, 1944 (No. 254), 29-30; R. H. Thouless, Phenomenal regression to the real object, *Brit. J. Psychol.,* 21, 1931, 344.

[12] A. H. Holway and E. G. Boring, Determinants of apparent visual size with distance variant, this JOURNAL, 54, 1941, 21-37; J. J. Gibson, *Motion Picture Testing and Research,* 1947, AAF Aviation Psychology Program Res. Rep., No. 7; E. L. Chalmers, Monocular and binocular cues in the perception of size and distance, this JOURNAL, 65, 1952, 415-423; W. M. Smith, A methodological study of size-distance perception, *J. Psychol.,* 35, 1953, 143-153.

[13] A. S. Gilinsky, The effect of attitude upon the perception of size, this JOURNAL, 68, 1955, 173-192.

Cognition of size in distance. The hypothesis to be offered involves a distinction between two events: first, a perceptual event in the sense of an immediate experience; and secondly, a judgmental event. The latter is considered a relatively slow process, taking up to several seconds of time for its completion, and one which includes *search* for whatever cues *S* considers important, then *categorization* in terms of previous experience, and finally a *decision* which places the comparison-object either within or without the category selected for the standard object. Introspective data from the present experiments give support to this account and also confirm the earlier suggestion of Jenkin and Hyman that categorization usually occurs in verbal and metric terms, *e.g.* "a white square card, 4 in. high."[14] No precise statement can be made about what cues *S* uses to establish a category, since these vary from person to person. In the present series of experiments, for example, some *S*s mentally projected an image of the comparison-card to the position occupied by the standard and in that way compared them. Others imagined foot rules placed adjacent both to the standard and the comparison and still others imagined that they were holding the card and used the separation of their thumb and fingers as a scale of reference.

It is proposed that the standard object is apprehended not as an abstract or meaningless two-dimensional area but is identified and categorized as an object with certain probable dimensions and properties. Since it has been categorized, the object perceived at 30 ft. is equivalent to the same object perceived at many other distances, even though these presentations are discriminably different in terms of retinal size.[15] We should therefore not be surprised if a closely similar match is made when the standard is removed to a new distance.

Once the standard has been categorized, a search is made for a comparison-object which fits the same category. As again borne out by introspective evidence from the present experiments, the search is based to a greater extent on the identification given the standard object than upon a perceptual comparison, *i.e.* a matching of areas sensorily given. In other words, *S* looks for a comparison card "four inches square" (or whatever he considers it to be) with certain identifiable properties.

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[14] Jenkin and Hyman, *op. cit.*, 750.
[15] Jenkin and Hyman, *op. cit.*, 72-76; Gilinsky, *op. cit.*, 179-185.

The present hypothesis appears to account for objective size-distance judgments being close approximations to the distal size rather than the proximal size. Expressed as it is in the language of concept-formation rather than perception it avoids some of the difficulties of earlier theories which attempted to explain "size-constancy." It remains to account for the evident expansion of size with increasing distance. It appears that this is a *perceptual* as distinct from a *conceptual* phenomenon and is apparently related to binocular convergence.

Work has been earlier cited indicating that an increase in convergence is accompanied by a decrease in apparent size, independently of any effects of distance.[16] In size-distance judging under reduced-cue conditions, one would therefore expect to find matches which are not quite reduced to visual-angle proportions, a result which in fact was found by Heinemann, *et al.*[17] Under conditions of free binocular regard, however, one would expect to find a superimposition of the effect of convergence-contraction upon the approximate constancy of size given by the process of identification and categorization.

Answers to the questions as to why the effect of convergence-contraction is called perceptual rather than categorical and why the *S* does not correct for this tendency are based upon the concept of a normal viewing distance.[18] *S*'s idea of what constitutes a given area, say 4-in. square, is anchored to a fairly close distance, one at which he is accustomed to making physical measurements and observing objects, such as metrical scales. This is within the arm's reach, and probably for most *S*s is approximately the length of the shortest distance used in the present experiments, 20 in. When, however, the eyes are converged for a distance of 40 in., the area is judged in terms of the subjective scale established for the normal viewing distance, 20 in. Since a slight expansion of apparent size has occurred with the decreased convergence, a smaller match is selected. The same is true with still greater distances. The constant error of progressive overestimation is regarded as a perceptual and not as a categorical event, since *S*s were unaware of this trend (as implied by the data and confirmed by subsequent questioning) and hence made no inferences or deductions in regard to it and evidently were unable to correct for this tendency. In summary, the point of view expressed above states that a process of categorization renders objects of nearly similar physical sizes equivalent at disparate distances despite the wide range in retinal sizes and that the bounds of the category are anchored to the normal viewing distance and shaped by perceptual susceptibility to the convergence-contraction effect.

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[16] See Footnotes 6 and 7.

[17] Heinemann, Tulving, and Nachmias, *op. cit.*, 39-43.

[18] Gilinsky, Perceived size and distance in visual space, *Psychol. Rev.*, 58, 1951, 463, 467.

SUMMARY

A series of four experiments was conducted to examine further the relationship previously shown to exist between increased distance and increased apparent size over a short interval in the near range of view. The comparison-object was viewed in Experiment I at 20, 40, and 160 in. from *S*'s eyes, with the standard constant at 320 in. Objective binocular size-matches were made under ample illumination by normal adult *S*s. In Experiment II, repeated observations were made at four distances by six *S*s. Both experiments gave similar results: (a) matches at each distance were significantly different from the matches at each adjacent distance, (b) plotted against the logarithms of the distances, the mean size-matches gave points fitted by a straight line, (c) as distance from the point of observation increased, the size of the matches regularly decreased. The direction of this relationship is opposite to that which would have been found from matching the visual angle. Controls indicated that the effect cannot be attributed to changes in brightness with distance, to decreasing ratios of distance, to unfamiliarity with the actual size of the object judged, or to an interaction of distances with types of trial (ascending or descending). The results cannot properly be described as 'size-constancy' and are therefore discussed in terms of two associated processes: first perceptual, involving the effect of convergence-contraction; and secondly, categorical, involving identification in verbal and metrical terms, based on a subjective scale of size at the normal viewing distance.

THE NEGATIVE AFTER-EFFECT OF THE PERCEPTION OF A SURFACE SLANTED IN THE THIRD DIMENSION

By Richard Bergman and James J. Gibson, Cornell University

The vsiual phenomena termed "figural after-effects" by Köhler and Wallach were regarded by them as clues to the mystery of what happens in the brain when a form is perceived.[1] The after-effects have been verified by other experimenters,[2] and the theory of *cortical satiation* has aroused much interest.[3]

A similar set of visual phenomena had been earlier described by Gibson and termed "negative after-effects."[4] These had not been interpreted as evidence for any underlying physiological process but only for a psychophysical principle called *adaptation* or *normalization*. Although the experimental facts seemed to be related, the implications drawn from them were radically different. Gibson was interested in the correspondence between the geometrical properties of the stimulus-objects and the geometrical properties of the perception. These were treated as variables. Köhler and Wallach were interested in the congruence or incongruence of (a) stimulus-patterns to brain-patterns and (b) brain-patterns to phenomenal patterns. These were treated as entities, not variables.

Gibson found that the apparent curvature or bend or tilt of a line segment seemed to decrease during a period of continuous inspection, and that immediately thereafter a 'normal' line segment in the same region of the visual field looked curved or bent or tilted in the opposite direction. Since this line could be made to look

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* Received for publication August 16, 1958.

[1] Wolfgang Köhler and Hans Wallach, Figural after-effects: An investigation of visual processes, *Proc. Amer. Phil. Soc.*, 88, 1944, 269-357.

[2] E. R. Hammer, Temporal factors in figural after-effects, this Journal, 62, 1949, 337-354; M. R. Marks, A further investigation into the Köhler effect, *ibid.*, 62, 1949, 62-74; J. E. Hochberg and M. E. Bitterman, Figural after-effects as a function of the retinal size of the inspection figure, *ibid.*, 64, 1951, 99-102.

[3] C. E. Osgood and A. W. Heyer, Jr., A new interpretation of figural after-effects, *Psychol. Rev.*, 59, 1952, 98-118; D. O. Hebb, *The Organization of Behavior*, 1949, 38-59; K. S. Lashley, K. L. Chow, and Josephine Semmes, An examination of the electrical field theory of cerebral integration, *Psychol. Rev.*, 58, 1951, 123-136.

[4] J. J. Gibson, Adaptation, after-effect and contrast in the perception of curved lines, *J. exp. Psychol.*, 16, 1933, 1-31; J. J. Gibson and Minnie Radner, Adaptation, after-effect and contrast in the perception of tilted lines: I. Quantitative studies, *ibid.*, 20, 1937, 453-467; Gibson, Adaptation, after-effect and contrast in the perception of tilted lines: II. Simultaneous contrast and areal restriction of the after-effect, *ibid.*, 20, 1937, 552-569; Adaptation with negative after-effect, *Psychol. Rev.*, 44, 1937, 222-244.

normal by curving, bending, or tilting it slightly in the *same* direction as the inspected line, the general principle seemed to be a shift of the norm, that is, a displacement of the psychophysical correspondence between physical straightness and phenomenal straightness.[5] The opposite after-effect, like negative after-images and many other phenomena of successive contrast, seemed to be merely a reflection of the shift of the neutral geometrical quality.

Köhler and Wallach found, on the other hand, that the shape, spacing, or position of the lines of a test-figure in a certain region of the visual field were distorted as a result of exposing an inspection-figure in that region. The alteration, they believed, resulted from a tendency for apparent contours to be displaced away from the locus of previous contours. The method did not involve an absolute judgment of a normal quality such as straightness, but a comparative judgment of the shape of two physically identical figures, one on either side of a fixation-point. Hence the inspection-period required constant fixation of a point to one side of the inspection-figure, instead of roving fixation of the figure itself as permitted by Gibson's method. A negative after-image of the inspection-figure in the supposedly satiated region of the field is inevitable with the Köhler-Wallach method, but is absent with the method of roving fixation.

The 'negative' after-effect, in short, applied to the properties of a visual contour while the 'figural' after-effect applied to the supposed prototype of perception, the figure-on-ground. Different assumptions about form-perception were implicit in the two experiments. Gibson conceived a contour analytically in terms of variable qualities such as direction (tilt) and change of direction (convex-straight-concave). He was impressed by the fact that tilt and curvature could be mathematically expressed as the first and second derivatives of a line. Köhler and Wallach seemed to conceive a line or a figure as an irreducible entity. The negative after-effect implied a merely dimensional correspondence between stimulation and perception; the figural after-effect implied isomorphism between a phenomenal field and a cortical field, with projection between the cortical and retinal fields.

The theory of neural satiation and of permanent satiation in the visual cortex has the attraction which a physiological explanation always presents for psychologists.[6] There are, however, many obscurities in it, and it accounts for only some of the characteristics of pattern-vision. The theory of dimensional psychophysical correspondence, on the other hand, with the possibility of normalizing shifts in correspondence, is broadly applicable to many fields of perception. The formula that the evocative power of a persistent abnormal stimulus will change toward that of a previously normal stimulus holds for (1) color vision, (2) temperature sensitivity, and (3) motion perception—to mention only three instances.[7] The negative after-effects of color, temperature, and motion are surely not 'figural' after-effects. They are dimensional and they involve opposite qualities separated by a neutral quality. Admittedly, however, there are difficulties in applying this formula to all kinds of perception. Although it seems to hold for the tilt and curvature of a contour, its applicability for closed contours or forms as such is dubious. Here Köhler and Wallach's approach seems to be more appropriate.

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[5] Gibson, *op. cit.*, *Psychol. Rev.*, 44, 1937, 222-244.

[6] Köhler and Wallach, *op. cit.*, 341-346.

[7] Gibson, *op. cit.*, *Psychol. Rev.*, 44, 1937, 222-224.

The plausibility of a dimensional approach to form perception has nevertheless been increasing in recent years. Moreover, the results of the Innsbruck experiments on adaptation to and the after-effects of the complex optical distortions produced by wearing prismatic spectacles are much more easily explained by a theory of the normalizing of geometrical qualities than by a theory of the relocation of visual contours due to cortical satiation.[8] An optically biassed visual world does tend to become normal in the course of time with respect to color, curvature, density, and stability, and when the bias is removed it looks abnormal in just these respects and in just the opposite way.

A single drawn figure, on the other hand, does not appear to become normal in the course of time, when inspected either by Gibson's or Köhler and Wallach's methods. Inspection of an ellipse, for example, does not result in the ellipse's becoming circular. Köhler and Wallach did not observe any normalization in their original experiments, but they did observe contour-displacement as an after-effect. In the interests of parsimony, they assumed that the negative after-effects for a line segment were explained by this formula. Most subsequent investigators have accepted this explanation. Negative after-effects have generally been subsumed under figural after-effects.

A few experimenters have attempted to isolate the negative effect from the figural effect. For example, Prentice and Beardslee reported evidence to demonstrate that adaptation or normalization of a tilted line occurred under circumstances precluding an explanation in terms of contour displacement.[9] Heinemann and Marill, however, have questioned this conclusion.[10] It may therefore be in doubt whether after-effects which are genuinely negative and dimensional in character exist in the realm of pattern vision. Is there only one class of 'figural' after-effects, or is there a distinct type of after-effects which point to a normalizing of the psychophysical relationship?

The aim of the present paper is to present new evidence for the existence of a negative after-effect as such, not explainable in terms of the cortical contour process. Since, however, it involves surface-perception rather than abstract form-perception, the problem of space must first be reviewed.

After-effects in the third dimension. After-effects occur for the phenomenal properties of objects in space. They are not necessarily visual. This fact was suggested by Gibson's discovery that a negative after-effect could be obtained for curvature by running the fingers along an edge while blindfolded.[11] Köhler and Dinnerstein later found that a figural after-effect could be obtained for width when an object was merely felt between the fingers.[12] Köhler and Emery proceeded to the discovery that strictly visual after-effects appear in the third dimension of space.[13] Previous visual studies had been limited to lines or margins on a surface frontal to the eyes, such as a sheet of cardboard. They now observed that if such a surface were presented at an 'inclination' to the line of sight, and if a frontal surface were then substituted for it, the whole surface together with a line drawn on it would seem to have an opposite inclination.[14] This was analogous to Gibson's negative after-effect of a tilted line in the frontal plane, but the authors went on to demonstrate an additional effect of displacement in the third dimension independent of the effect of slant. A square of cardboard maintained at a fixed distance in a certain region of the visual field would have the subsequent effect of displacing forward another square presented at a lesser distance in that region, or displacing backward one presented at a greater distance in that region. Köhler and Emery concluded on further experimentation that such a displacement in depth could not be explained by the two-dimensional displacements of contours already discovered, *i.e.* by an apparent change in the angular size of the test-object.[15] They were nevertheless convinced that the whole set of phenomena was in some sense 'figural,' and that the theory of cortical satiation could be extended to explain all of them. The negative after-effect of a surface slanted in depth was considered a special case of the displacement in depth of a surface away from the locus of a previous surface. Köhler and Emery had used relatively untextured cardboard surfaces in their experiments and they laid stress on the perception of the edges of these objects as the determiners of their phenomenal depth.

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[8] Ivo Kohler, *Über Aufbau und Wandlungen der Wahrnehmungswelt,* 1951, 1-118.

[9] W. C. H. Prentice and D. C. Beardslee, Visual 'normalization' near the vertical and horizontal, *J. exp. Psychol.,* 40, 1950, 355-364

[10] E G. Heinemann and Thomas Marill, Tilt adaptation and figural after-effects, *J. exp. Psychol.,* 48, 1954, 468-472.

[11] Gibson, *op. cit., J. exp. Psychol.,* 16, 1933, 1-31.

[12] Wolfgang Köhler and Dorothy Dinnerstein, Figural after-effects in kinesthesis, *Miscellanea Psychologica Albert Michotte,* 1949, 196-220.

It is possible, however, to obtain an impression of a slant without any accompanying impression of an edge, if the surface has a visible *texture*. The surface need only be observed through an aperture or window which hides its edges. Observation can be either monocular or binocular. This is the method used to investigate 'optical slant' as a fundamental variable of the visual world supposedly dependent on gradients of optical stimulation.[16] With this setup, the margins of the window remain constant in the visual field despite changes in the slant of the surface behind the aperture. The question is whether a negative after-effect of apparent slant can be demonstrated under these conditions and whether a normalization or adaptation of slant will occur with prolonged inspection. If so, a theory of psychophysical shift would seem to be required, and it is not clear how a theory of cortical satiation could explain the result.

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[13] Wolfgang Köhler and D. A. Emery, Figural after-effects in the third dimension of visual space, this JOURNAL, 60, 1947, 159-201.

[14] *Ibid.*, 162.

[15] Köhler and Emery (*op. cit.*, 194) asserted this independence of the depth-displacements primarily on the basis of the following experiment. They opposed the displacement in the frontal plane to the three-dimensional displacements by using a large distant inspection-object which surrounded the test-object retinally. They reported a forward shift of the test-object which, under these conditions, could not be explained in terms of angular displacements. These results have been challenged by Osgood and Heyer (*op. cit.*) and this question is not yet settled.

[16] Gibson, The perception of visual surfaces, this JOURNAL, 63, 1950, 367-384; J. J. Gibson and Dickens Waddell, Homogeneous retinal stimulation and visual perception, *ibid.*, 65, 1952, 263-270.

EXPERIMENT I: THE NEGATIVE AFTER-EFFECT

The purpose of Experiment I was to demonstrate adaptation and a negative after-effect in the perception of a slanted surface when no lines, margins, or edges were visible.

Apparatus. The setup for binocular (or monocular) inspection consisted of a large flat rectangular slanting board and a pair of round viewing tubes through which its surface could be seen. The surface consisted of coarsely textured burlap, stretched tightly over the slanting board, and placed 34 in. from *O*.

The margins of the slanting board were not visible. Nothing was visible but a burlap surface subtending a visual angle of 17° 33′, bounded by a circular aperture, and this contour remained constant throughout the experiment. There was a small nailhead in the center for a fixation point, which was used in later studies. Convergent adjustment of the viewing tubes was possible. A head-rest kept *O*'s head motionless. Foam rubber padding surrounded the viewing tubes.

The burlap surface could be slanted either forward or backward from the frontal plane on a horizontal axis at its center. The slant was controlled by a large hand wheel geared to this axis. This wheel could be operated by either *O* or *E*. A protractor permitted measurement of the slant, accurate to about a tenth of a degree. The rough texture of the cloth was illuminated by two invisible fluorescent lamps fastened to either side of the slanting board. This texture did not have rectilinear or regular elements, but the density of the elements was the same in all directions. Exposure-time was controlled by a shutter placed at the end of the viewing tubes.

Stimulation for the perception of slant consisted of the perspective compression of the optical texture, that is, a certain gradient of density along the up-and-down meridian, supplemented by a gradient of binocular disparity whenever two eyes were used. Zero slant corresponded to a zero of these gradients.

Procedure. The procedure was to present *O* with the surface at a slant, which he inspected for 4 min.[17] In this experiment, *O* was given no instructions about fixation. He was encouraged to report freely all that he saw. The only questions asked him were "What do you see?" and "Can you tell me more?" At the end of this period, the shutter was closed briefly and reopened, exposing the surface in a frontal position, perpendicular to the line of sight. The *O* was then required so to adjust the surface that it appeared frontal. He was told always to move the surface too far forward and too far backward before making his final setting, even if it already appeared frontal. Each *O* was given six such trials with a 2-min. rest between successive trials, alternating forward and backward slant.

For each pair of trials, opposite but equal slants were presented, and the negative after-effect was taken to be half of the difference between the two settings. This method gives a very conservative measure of the after-effect, but it had the advantage of preventing a cumulative or persistent effect, that is, an increasing constant error in the perception of the frontal plane. The constant error, if any, is thus known at all times during the experiment, and can be compared with that obtained before the beginning of the experiment. Another advantage is that the calibration of the protractor to a physical vertical need not be precise. Even with this successive cancellation by opposite stimulation, the amount of negative after-effect obtained after a 4-min. inspection was considerable, as will appear.

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[17] The 4-min. viewing period was chosen to give a reasonable after-effect. Preëxperimental trials with a few *Ss* suggested that a longer period led to discomfort, while strong after-effects were difficult to obtain with shorter periods.

A negative or opposite after-effect is definable as follows. After inspecting a forward slant (facing floorwise) a zero slant will appear to slant backward (facing ceilingwise) and the setting will have to be forward if the surface is to appear at zero. With our method, however, forward and backward are taken relatively, not absolutely.

Degree of slant. Three different degrees of viewing slant, 15°, 30° and 45°, were presented to each *O*. Each slant was presented once forward and once backward, in adjacent trials, thereby constituting six viewing periods for each *O*.

Control measures. Two other measurements of error were made for each *O* at the beginning of the experiment. The constant error of the frontal plane was determined by requiring *O* to make 10 quick adjustments of the surface to the apparent frontal position. The error was always small, and the mean of all these adjustments was not significantly different from zero (1.6° forward slant). He was then given two trials in which he inspected the surface in a frontal position for 4 min. and then adjusted it to the frontal position. As expected, there was no significant after-effect from this type of inspection period.

Observers. Six experimentally naïve college students, four men and two women, went through the above procedure. All had uncorrected vision. After receiving instructions, *O* made the 10 constant error adjustments, then 2 adjustments after frontal inspection, and finally 6 adjustments after viewing the 15°, 30°, and 45° slants. The order of these presentations was counterbalanced among the *O*s, and the forward and backward order of viewing was also counterbalanced.

Results. With the first exposure of the slanted surface, all the *O*s correctly described the direction of slant. The texture of the surface as such provided sufficient stimulation for the perception.

Adaptation. A clear finding was that every *O* spontaneously reported a straightening up of the apparent slant during his inspection-period. *O* usually expressed surprise after about a minute, stating that the surface had become less slanted.[18] There were no reports of an apparent increase in slant. Once a complete straightening of the surface was reported. A process of adaptation or normalization in this situation did occur without question.

Negative after-effect. A negative after-effect was evident after each period of inspection of the slanted surface. The difference between the adjustment following the forward slant and that following the backward slant (keeping account of signs) was always in the predicted direction.

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[18] Some of these reports were as follows: "The longer I stare at it the straighter it looks." "It is still at an angle, but not so great." "Is it straight up and down now?"

This difference is double the after-effect proper. The means of these differences were 5.3°, 5.7°, and 5.5° after viewing the 15°, 30° and 45° slants respectively. Each of these differences is significant to at least the 2.5% level of a one-tailed *t*-test.[19] Evidently the degree of slant has no significant effect on the amount of the after-effect.

Individual differences. Although each *O* showed a negative after-effect, it can be seen from Table I that the amount of the after-effect obtained varied considerably. The mean difference in the last column, which must be halved to yield a measure of the effect, varied from 2.7° to 10.0°.

TABLE I

THE NEGATIVE AFTER-EFFECT OF SLANT

The difference in degrees between the adjustments after inspecting a forward and a backward slant.

O	Viewing slant			Mean
	15°	30°	45°	
MS	10.5	13.0	6.5	10.0
BR	3.0	3.5	10.0	5.6
GS	2.0	6.5	6.5	5.0
JM	5.0	0.5	2.5	2.7
HD	5.5	3.0	1.5	3.3
JR	6.0	7.5	6.0	6.5
Mean diff.	5.3	5.7	5.5	5.5
Significance	.01	.025	.01	

EXPERIMENT II: THE NEGATIVE AFTER-EFFECT OF A SLANTED SURFACE UNDER VARIOUS CONDITIONS OF OBSERVATION

We next sought answers to the following questions. Is the projection of a textured surface on a single retina sufficient to produce a negative after-effect of slant, or is binocular projection necessary? Is it necessary to keep the retinal pattern stationary in order to produce an after-effect or can it be freely transposed across the retina and still do so? In other words, is the after-effect as much evident when the surface is scanned as when it is fixated during the inspection-period?

We also wished to discover whether the after-effect process in the nervous system is central to the 'combination-zone' of the two eyes. We therefore tested for transfer of the after-effect from a stimulated eye to an unstimulated eye. If the after-effect transferred between eyes, we could infer that the process occurred at or beyond the point of combination of the individual retinal excitations.

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[19] The magnitudes reported are probably not the maximal after-effect, for reasons given. Also, as Köhler and Wallach point out (*op. cit.*, 301), the method of adjustment in studies of after-effects involves a time-lapse between the removal of the inspection-object and the test-adjustment, which would reduce the after-effect.

Procedure. The same apparatus was used in this experiment as in the first experiment, above. As before, *O* was presented with the surface at a slant, which he viewed for 4 min., and then with the surface in the frontal plane which he had to adjust so as to appear frontal. A 15° slant was always used. Each *O* was tested with four different conditions of observation. These were: (1) binocular scanning; (2) binocular fixation; (3) monocular scanning; and (4) monocular fixation.

For binocular observation *O* peered through both viewing tubes so adjusted that the fields coincided. For monocular observation one tube was plugged. For the maintaining of fixation, *O* was given the following instructions:

> As you look, focus on this small dot on the surface. Never let the dot escape your attention. You must continue to fixate for a few minutes, so try your best to maintain your fixation. If you should happen to move your eyes, then return instantly to the dot. [For moving fixation, or scanning.] Keep looking over the whole surface; don't concentrate on one part of the field. Continually move your eyes up and down, top to bottom. Do not rest your eyes on one spot.

There seemed to be no difficulty in following these instructions. Each condition of observation was presented twice to each *O*, once with forward slant and once with backward slant. The order of forward and backward was counterbalanced among the *O*s, and the order of the four conditions of observation was also counterbalanced. These eight trials, with 2-min. rest-periods intervening between successive trials, were preceded by a determination of the constant error but, as in Experiment I, it was always small. Eight new experimentally naïve *O*s, six men and two women, participated in the experiment.

Transfer between eyes. Two days after the above judgments were made, each *O* returned for another pair of trials. He fixated the slanted surface through one tube only and was then required to make his adjustmnt with the other tube open. The resulting after-effect was compared with the after-effect obtained two days earlier, when *O* had viewed monocularly and had been tested with the same eye.

Results. A negative after-effect was obtained under all four of the conditions of observation. The mean difference between adjustments after viewing forward and backward slant (the doubled after-effect) was significant to the 1% level for each condition (see Table II).

Binocular and monocular inspection. The doubled after-effect was 4.4° for binocular inspection and 5.0° for monocular inspection. The differences between them was not significant ($p > 0.4$, by a two-tailed *t*-test). Evidently the presence of binocular cues for depth is not necessary for the occurrence of the after-effect in depth. The perspective foreshortening of the optical texture in the window, whether or not accompanied by a gradient of binocular disparity, was sufficient to induce a process of adaptation and a subsequent negative after-effect.

This result suggests that stimulus conditions as seemingly diverse as texture density on the one hand and texture disparity on the other are functionally similar. Not only are they equivalent for slant perception; whatever processes they induce in the nervous system are alike in being susceptible to adaptation.

Fixation and scanning. The doubled after-effect was 5.1° when *O* fixated the center of the surface and 4.3° when he continually moved his fixation up and down the surface. Although the former after-effect was slightly stronger, this difference was not significant ($0.2 > p > 0.1$, by a two-tailed *t*-test). Evidently the textural stimulation is about as effective in producing an after-effect when it is repeatedly transposed over the retina as when it is fixed and motionless on the retina.

TABLE II

THE NEGATIVE AFTER-EFFECT AS A FUNCTION OF CONDITIONS OF OBSERVATION

	Viewing condition			
	Binocular scanning	Binocular fixation	Monocular scanning	Monocular fixation
Mean diff.	3.9	4.8	4.6	5.4
Significance	.01	.01	.01	.01

These results may seem surprising, but actually they are consistent with the fact that negative after-effects occur for phenomenal *objects* as well as for the phenomenal *sensations*—for an object whose surface is either felt by the fingers or explored by the eyes. The after-effect of slant, unlike an after-image of color, is inherent to the percept of the slanted surface, which remains constant with eye-movements. The after-effect is located in the visual *world,* not the visual *field.* Since the neural substrate of the visual world, which is stable with eye-movements, is completely unknown, it would be futile to speculate in what way the after-effect might be localized in the brain. The visual field is in a projective relationship to the visual cortex, but the visual world is not. It would appear, then, that physiological theorizing about figural perception as a cortical process is premature until the eye-movement paradox is resolved. It might be argued that a strictly psychophysical theory of figural perception is more promising.

Transfer from one eye to the other. When the surface was fixated with one eye and the adjustment was made with the same eye, the doubled after-effect was 5.4°. When the adjustment was made with the other eye the doubled after-effect was 4.3° The difference is not statistically significant. Evidently much or all of the after-effect produced by stimulating one eye can be observed with the other eye. The negative after-image of color does not transfer in this way, but the negative after-effects of motion, of linear curvature, and of linear tilt do so.[20]

In the chain of neural events comprising vision, there must be a stage after which it makes no difference which eye has been stimulated. Whatever the normalization process may be, it occurs prior to this stage with hue and brightness and at or after this stage with slant in the third dimension.

DISCUSSION

The slant of a surface from the frontal plane is a variable with two directions from a zero point. This variable can ordinarily be judged with accuracy. Prolonged inspection of a frontal surface does not affect this accuracy, but prolonged inspection of a slanted surface leads to a systematic error of judgment. The slanted surface appears noticeably less slanted, a surface at some lesser slant appears frontal, and a frontal surface appears slanted in the opposite direction. With a return to the ordinary inspection of environmental surfaces, the effect dissipates.

As demonstrated in our experiment this seems to be a negative after-effect resulting from adaptation, rather than a figural after-effect resulting from satiation. The normalizing process could be directly reported. Moreover the conditions of stimulation were such that contour-displacement could not explain the effect. We conclude that negative after-effects in vision cannot be subsumed under figural after-effects.

The stimulation in this experiment was an optical texture, not an optical contour or figure. The only form presented was the circular edge of the window, which remained constant throughout. Important as contour and closed contour may be for visual perception, it is not the only, or necessarily the most fundamental, kind of stimulus in the array of light to an eye. A theory of the contour process in the nervous system will therefore not provide a complete physiological explanation of perception. What is lacking in contemporary speculations is a theory of surface perception.

A strictly psychophysical principle or law will explain the negative after-effect observed, along with many other types of after-effect. The formula is that the evocative power of a persistently applied abnormal stimulus will tend to change toward that of a previously normal stimulus.

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[20] Gibson, *op. cit., J. exp. Psychol.,* 16, 1933, 25-27, and also Gibson, *op. cit., ibid.,* 20, 1937, 561.

The *norm*, in this formula, can be defined in terms of statistical frequencies relative to an enviroment. The *stimulus* must be defined in terms of higher order mathematical variables, like gradients, that is, variables which carry information about the enviroment. The principle would be that a persistent condition of the stimulating environment tends to become the normal quality of the phenomenal world. A psychophysical explanation of this type is no less valid, in its way, than one couched in physiological terms.

With regard to after-effects in the third dimension and the after-effects of feeling solid objects with the fingers, the formula of contour displacement is surely not adequate to cover all the facts. Köhler and Emery were persuaded to include them in the class of figural after-effects, but surely they are not figural in the historical meaning of the term. They have to do not with forms or drawing but with the perception of environmental solids, either by the eye or the skin.

The eye and the fingers are both exploratory sense organs. They register the structures of things, not pictures of things or patches of color in the visual field. This fact makes it intelligible that the after-effects of object perception should be unlike the after-images of color perception, that is, independent of eye movements. The apparent normalizing of the surface in our experiment was, like the apparent normalizing of objects distorted by the wearing of prismatic spectacles, a change in the phenomenal world, not a change explainable as a local alteration in the retinal or cortical field. There must have been, certainly, an alteration in the receptive system to correspond with this change in the appearance of the enviroment. The negative after-effect proves that, but none of the current neurological models gives us any hint as to what this alteration might be.

MONOCULAR POLYOPIA INDUCED BY FATIGUE

By JOHN F. DASHIELL, Wake Forest College

This paper describes several cases of monocular polyopia induced by fatigue (ocular work) and it attempts to determine the physiological bases of the phenomenon. This phenomenon seems to have escaped notice, as no reference to it can be found in the psychological or ophthalmological litterature.

CASE RECORDS

The following case records of monocular polyopia that resulted from ocular fatigue have come to the writer's attention.

(1) From childhood the writer has frequently noted that after a prolonged reading the visual images of the printed text, as well as of other visual stimuli, become doubled for each eye as examined separately, and sometimes even tripled for one of them. Specifically, after steady reading for an hour or two, the right eye sees a clearly defined and delimited but less intense shadow-image of the letters displaced rightward and downward. After a longer period, a similar shadow-like image appears to the left eye. Then occasionally, following a protracted period of the reading (three or more hours), a second supernumerary image may appear in the right eye's field displaced slightly further rightward and downward, and again clearly defined though further reduced in intensity. Fig. 1 presents these phenomena lettered in order of their appearance.

(2) Next is furnished an excerpt from the report of another observer, a graduate student in a different field, but one who has studied some psychology with the present writer.

> Without spectacles I see two distinct images . . . with each eye alone. I never experience the phenomenon unless I have been using my eyes (ordinarily in reading and study) for a considerable length of time. I have 20/20 vision in both eyes. The phenomenon is nothing new to me; I have experienced it numerous times throughout my life, and I have never had any eye trouble. Moreover the double image is not a blur. When my eyes blur, I can generally refocus them by merely blinking them a few times. In this polyopia the double image remains unaltered after any number of blinkings. The phenomenon always accompanies fatigue of the eyes after prolonged reading. It always takes the same form: a duplication of a line of type half-way above the line actually printed, and a good bit less distinct in impression, though clearly and unmistakably a duplication with a definite boundary (see Fig. 2).
>
> While I have no color blindness, I must say that the polyopia is induced almost always when I look at the spine of a dark-colored book which has been

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> stamped either in gold or in yellow. Gold against blue or red makes the clearest and best impression. The image did not appear at all when I beheld a book covered with a paper jacket printed in dark blue letters on a light blue field.[1]

(3) An oral report from an undergraduate student of psychology can be briefly summarized.[2] Miss D. states that after protracted reading she observes a supernumerary image in the right eye's field only. This second image appears to the right and upward of the original image. Incidental details of this case are: spectacles are worn which are corrective for slight astigmatism and for slight nearsightedness; the right eyelid is harder to close voluntarily; heterophoria is found in which the images of the two eyes are separated (under the condition of relaxation of eye muscles) in the upper-right, lower-left meridian.

(4) A father-mother team of physiological psychologists has reported to the writer, both orally and by correspondence, a case in an infant.[3]

> Last December when Lynne was 23 mo. old, and talking very well, we were looking at the moon (about one-half full). Lynne said spontaneously, "I see two moons." I covered one of her eyes and asked her how many moons she saw. She said, "Two, a big one and a little one." Further questioning indicated that the little moon was below the other one. The day had been tiring, but not because of reading! She reported "two moons" again about two weeks ago. She has no noticeable muscular imbalance, and sees better at a distance than I. I get the same symptom when *very* tired, so I shouldn't be astonished if she did too.

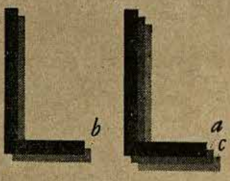

FIG. 1. ORIGINAL AND SUPERNUMERARY IMAGES OF *JFD*

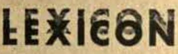

FIG. 2. ORIGINAL AND SUPERNUMERARY IMAGES OF *JMB*

It is to be emphasized that the observations to be discussed in this paper do not refer to monocular diplopia or polyopia as observed with a normal rested eye. Although not commonly recognized by psychologists or ophthalmologists, such multiple images in the field of one eye alone are well known to the more experienced of them.

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[1] From correspondence from Mr. John M. Bullard of the Divinity School of Yale University. The occurrence of some colored supernumerary images among the black when the stimulus-material is black has only occasionally been reported for polyopias in non-fatigued eyes; but the facts are obscure and not essential to the problem before us.

[2] Miss Mary Frances Davis of the University of North Carolina.

[3] Dr. F. Nowell Jones and Dr. Margaret Hubbard Jones of the University of California at Los Angeles.

Monocular polyopias in unfatigued eyes have been recorded in many places in the literature of ophthalmology, and in association with a variety of ocular structural irregularities, such as heterophoria and squint, cataract, opacities of the cornea, differences in refraction in the vitreous, high errors of astigmatism, dislocations of the lens, or with general pathologies as encephalitis lethargica, post-encephalitis, multiple sclerosis, basal meningitis, in cerebral tumors, and (of course?) in hysteria. To repeat, the present writer has found no report of polyopia or even diplopia that is consequent upon prolonged eye-work (fatigue).

Apology is in order for the use of the term 'fatigue.' As has been recognized for a half-century, that term has been used with a variety of meanings; and no universally accepted explanation of what it may denote is at hand. In particular, Bartley has emphasized the danger of employing it in reference to visual and ocular phenomena.[4] The present writer is here using it as a shorthand name for "a condition consequent upon prolonged ocular work."[5]

ALTERNATIVE EXPLANATIONS

Monocular polyopia appears in the normal rested eye under the following conditions. They may suggest possibilities of interpreting the occurrence of the fatigue-induced phenomenon.

(1) Brain injuries: Hemianopsia. A unilateral lesion in the calcarine region of the occipital cortex may produce blindness for one half of the field of vision, at least in the macular or central area. This is an annoying condition, hence the afflicted individual may tend to use an adjacent portion of the retina for fixation. The organism's objective, so to say, being that of getting to see the stimulus-object as a whole, to complete the half-image, its adjustive efforts may lead to the forming of a new functional macula, a pseudo fovea. The psychophysiological nature of this formation would be analogous to that which we have just discussed; it is an achievement worked out on the perceptual level. Neurologically, Goldstein has theorized that a process of diffusion or spreading of the excitation is taking place in the cortex.[6] A new image (*i.e.* its cortical correlate) gets established about a new spatial point in the cortex, while the old one persists, and so two images are had.

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[4] S. H. Bartley, The basis of visual fatigue, *Amer. J. Optom.,* 24, 1947, 372-384.

[5] Since the observing of sensory images is essentially and even exclusively a matter of subjective introspective observing the writer is handicapped by his inability in the nature of the case to demonstrate the phenomenon directly to others. Skepticism on the part of some psychologists and ophthalmologists may therefore be expected, though the writer has met with an open-mindedness on the part of certain competent authorities. He has not solicited confirming reports in a general manner heretofore; but any phenomenological reporting of similar or related observations in the sensory experience of others would of course be significant in the further exploration of this small corner of the scientific field.

[6] Kurt Goldstein, *The Organism,* 1939, 50-51.

Cases of diplopia from brain injury reported by Bender and Teuber remind us of motor involvements in addition to the formation of new maculae.[7] Among the cases they report are the polyopias reproduced in Fig. 3. They note that such images appeared after a latency of some seconds, and that they occurred typically when there was some motion of the eyes, or of the object of regard, or of a neighboring object. It seems clear that even in cases of cerebral lesions, then, polyopia is directly and immediately a matter of peripheral disturbance, a derangement of motor adjustment. Bender has mentioned other evidences also of disturbance of "the dynamic equilibrium of the perceptual organization (including the oculomotor component)";[8] and they entertain the possibility that both the diplopia and the nystagmoid movements that occur during fixations are consequences of a more basic causal factor—a disruption of equilibrium of perceptual functions by a toxic or inflammatory condition.

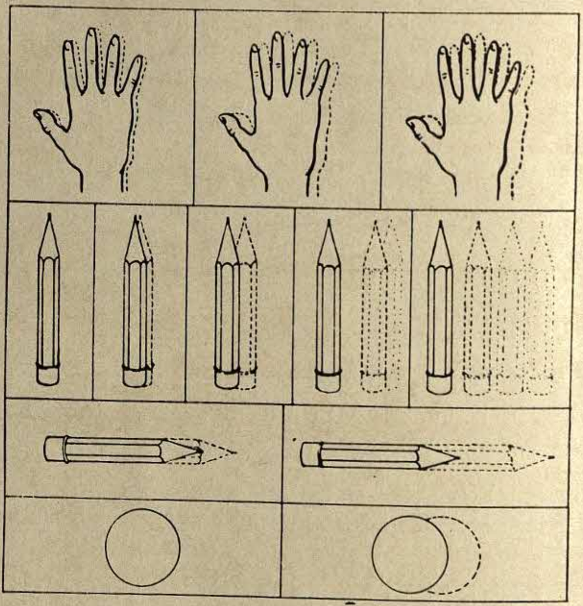

FIG. 3. DIPLOPIAS RESULTING FROM BRAIN INJURY
(From Teuber and Bender, *op. cit.*, 51; reproduced by permission of editor.)

To bring this line of interpretation to bear on our present problem: can it be that overwork of the eyes can have much the same consequence? Can it be that the organism, after making ocular reflex movement corrections for slightly displaced images, eventually becomes so fatigued (either generally or locally) that the corrections relax or lapse and a latent tendency to polyopia is allowed to become manifest, with resultant diplopia-polyopia?

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[7] M. B. Bender and H. L. Teuber, Nystagmoid movements and visual perception: Their interrelation in monocular diplopia, *Arch. Neurol. & Psychiat.*, 55, 1946, 511-529; Teuber and Bender, Alterations in pattern vision following trauma of occipital lobes in man, *J. gen. Psychol.*, 40, 1949, 37-57.

[8] M. B. Bender, Polyopia and monocular diplopia of cerebral origin, *ibid.*, 54, 1945, 323-338.

(2) Spasms of ocular muscles. A possible lead in our quest for interpretation of the polyopias appearing subsequent to eye-work presents itself in the recognized spasms that occur in either of two kinds of muscular adjustment for adequate seeing.

(a) Convergence. One kind is to be found in the extrinsic musculature of the globe: spasm of convergence. Tremors of an individual extraocular muscle (primary spasm) is as a matter of fact quite rarely observed; but the consequences for vision appear to be similar to those in case of secondary spasm, which is in turn a consequence of a paresis of an opposed muscle and overreactions by the muscle in question. Walsh cites the following example:[9] Suppose the superior rectus muscle be partially paralyzed (Fig. 4): then a spasm may be observed in the contractions of the inferior rectus or the inferior oblique, which are antagonists of the superior rectus. The symptoms of the spasm or tremor are: (1) excessive movements of the eyeball that are traceable to the muscle mentioned; (2) appearance of a double or multiple image; resulting in (3) a false projection of the object as seen.

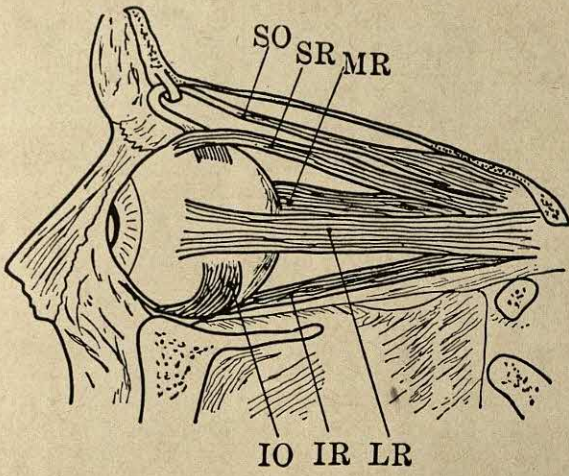

FIG. 4. THE EXTRAOCULAR MUSCLES OF THE LEFT EYEBALL

(b) Accommodation. Another kind of spasm that is productive of polyopias is one involving intrinsic musculature, that of the ciliary body and controlling the lens: spasm of accommodation (see Fig. 5). When the eye is adjusting for vision of a near object, and a maximal accommodation of the lens (increased convexity) is being attempted through the ciliary muscle working on the suspensory ligaments and lens capsule, in some cases the lens is observed to be trembling or wobbling,

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[9] F. B. Walsh, *Clinical Neuro-Ophthalmology,* 1947, 265 f.

presumably under the excess of tensions of a tired ciliary muscle being forced to activity when it can act only in a spasmodic manner.[10] An incidental feature may be some dislocation (subluxation) of the lens, which itself furnishes a source of errors in the projection of the light rays to the retina. With the lens wobbling, the light rays being conveyed from each point from the object of regard will suffer some dispersion within a kind of circle; and the image that develops on the retina will be an indistinct one. From the various points of the seen object, then, there will be cast upon the retina light rays producing many such indistinct images overlapping and telescoping each other; and since those at the margin of the mass of images will appear more distinct than the others, there will be produced multiple images of the object of regard.[11]

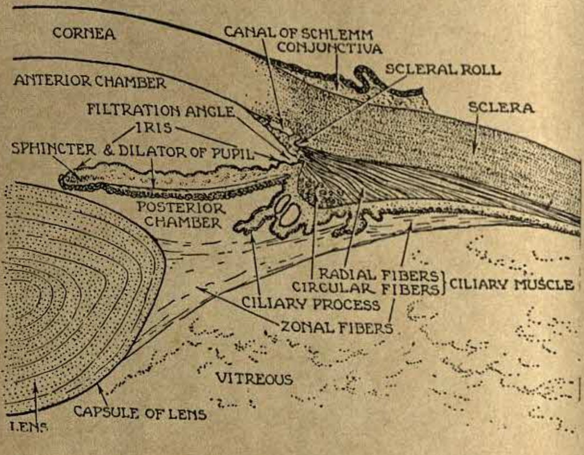

FIG. 5. STRUCTURES INVOLVED IN ADJUSTMENTS OF THE LENS AND PUPIL (From J. F. Fulton, *Text-Book of Physiology*, 17th ed., 1955, Philadelphia, Saunders; reproduced by permission of author and publisher.)

A characteristic of the spasm or excess of accommodation which is only incidental to our quest is an abnormally close near point, far point, or both. In the clinical cases reported, there are also associated pains and headaches, which are not a feature of the cases before us here. More relevant is the fact that the condition of accommodative spasm is caused by long continued strain of the ciliary muscle in attempting to adjust to insufficient or improper correction.

(3) Refractive errors: Lenticular irregularities. Some of the cases of multiple images in the visual field of one eye that are fairly well recognized by most ophthalmologists, as mentioned in an earlier paragraph, are those in which irregular curva-

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[10] A. de H. Prangen, Spasm of the accommodation, *Trans. Sect. Ophthal., Amer. med. Assn.*, 1922, 282-292; W. H. Luedde, The The mechanism of accommodation, *Arch. Ophthal.*, 1932, 7, 30-70; M. P. Koke, The mechanism of accommodation, *ibid.*, 1942, 27, 950-968.

[11] Prangen, *op. cit.*, 224, 291; E. A. Spiegel and I. Sommer, *Neurology of the Eye, Ear, Nose, and Throat*, 1944, 357.

ture of the surfaces of the lens transmits more than one cone of light rays. The condition called lenticonus, which is not uncommon, affords a simple introduction to this matter. One extreme kind is shown in Fig. 6. A bulge on the anterior or posterior face of the lens constitutes a surface of greater convexity than that of the surrounding lens, and will produce a cone of light rays at a focal point nearer than that for which the lens as a whole is for the moment adjusted, the retina. Lens surfaces are often found, however, more complex and irregular than this. A surface may possess a number of bulges, so that with a given degree of accommodation the many light rays from a single point of the object of regard will be brought to focus at multiple points on the retina. Polyopia will be experienced.

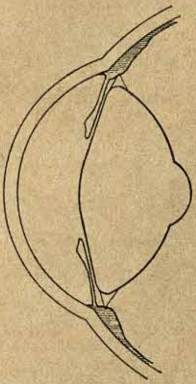

FIG. 6. A CASE OF SIMPLE LENTICONUS (Redrawn from cut in Ida Mann, *Developmental Abnormalities of the Eye,* 1937, Cambridge University Press.)

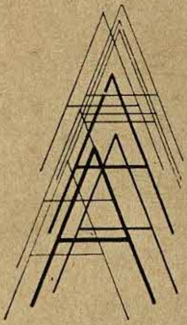

FIG. 7. A CASE OF MONOCULAR POLYOPIA (Subject *KSL.*)

To illustrate a common ophthalmological point, the writer finds that when the polyopias have been set up (by fatigue in this case) in his two fields of vision, he can eliminate one or more of the supernumerary images of either eye by holding a straight edge close to the eye and brought toward the center of vision from below or from above. Other observers can eliminate other multiple images in their unfatigued visual fields by moving the straight edge in from other directions. A related phenomenon of normal vision is to be found in Scheiner's classical demonstration. A slender object viewed through two pinholes separated by less than the width of the pupil will be seen doubled; and anyone can find for himself that blurred looking print can easily be read through a single pinhole. Then there is the everyday experience of clearing up indistinct vision by drawing the eyelids closer together, as well as the explanation of the higher visual acuity in a well illuminated room as due to the contraction of the pupils which serves to intercept excess rays.

An interesting case of original monocular polyopia, obtained from Lashley, is in point. He writes:

> On one occasion I noticed a fairly distinct triple image of a neon sign; on removing my glasses, I was able to count nine separate images (left eye) of which three were intense, the rest faint but clearly separate (see Fig. 7). Tried a pinhole pupil; it cut out all but one image. The edges of my lenses must have facets like a rose-cut diamond. The puzzling thing: is the similarity of the refractive defects in both eyes. It suggests symmetrical innervation of segments of the ciliary muscles of the two eyes.
>
> Lens defects may be very great. I have examined some hundreds of rats' lenses in a strain that had microphakia. Occasionally a lens may even look like a badly cut diamond with numerous facets, or even like a ball of putty squeezed out of shape and marked with deep grooves. Even a couple of small indentations in the lens might result in several cones of light.[12]

DISCUSSION

Turning now from cases of original polyopia to the fatigue-induced cases which are the subject of this paper, we find the appropriate query is: What happens during the earlier and the later stages of the eye-work?

It is a familiar psychological fact that in the very initial process of giving sensorial attention to anything there occur prompt reflex adjustments of preparatory nature calculated to obtain maximally accurate reception of the stimulation and functioning in the manner of servomechanisms. In listening, there are both the head turning and pinnae directing and also changed tension of the tensor tympanae of the middle ear. So in seeing, there is quick rotation of eyeballs, convergence-divergence of eyeballs, contraction-dilation of pupils and increased-decreased accommodation of the lens. These and other reflex adjustments of receptors occur in response to feedback or monitoring afferent neural impulses. Applying this general function to our particular problems, it is certainly conceivable that with the projection of supernumerary images upon the retina as the visual apparatus is directed to the business of reading, prompt automatic motor adjustments will occur of a nature to correct the inadequacy.

Then, if the reading be long continued under the sustained tension, these muscular organs of correction undergo fatiguing, relax or weaken in their functioning and reappearence of the original disturbance in seeing results.

Such is the speculative theory. Are there as a matter of fact any motor structures that would seem capable of playing this role of adjustive correction of lenticular surface irregularities, and then of relaxing under fatigue? We may consider the ciliary process. The lens in its capsule held in place by the suspensory ligaments is controlled in its degree of convexity by the contraction-relaxation of the ciliary muscle, which consists principally of both circular (sphnicter) and radiating (meridional) fibers, as shown in Fig. 5. The arrangement of the fibers is even more complex, as there are in the ciliary body also tangentially or spirally arranged muscle fibers.[13] Are the fibers capable, we ask, of varying amounts of contraction along different meridians in consequence of differential innervation?

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[12] K. S. Lashley, Private communication.

A moot point among ophthalmologists is that of cylindric accommodation, *i.e.* adjustive reactions of the ciliary which accommodate the lens to correct errors of refraction along meridional lines, making the lopsided non-spherical refractive system of lens and/or eyeball, as in astigmatism, less lopsided and more symmetrical radially. In brief, the possibilities of finely differential accommodatory reflex responses in the muscle tissues controlling the shape of the lens may be considerable. That the length to which the present writer is speculating is not excessive is suggested by the following excerpt from a communication from Bartley.

> It is alleged (by some ophthalmologists) that meridional fluctuations occur in refraction in keeping with the overall behavior of the individual. The individual responds unequally well in the various meridians at various times and under various conditions. Thus astigmatism is allegedly not as fixed a condition as it was once thought to be. It is to be taken as an expression of some sort of organismic adjustment in the seeing process.[14]

To recapitulate the writer's hypothesis advanced to explain fatigue-induced uniocular polyopia: (a) there are irregularities in the surface of the lens, casting multiple images upon the retina; (b) among the adjustive functions of the visual apparatus are accommodatory reflexes that tend to correct the multiple imagery; (c) but the neuromuscular adjustments are subject to fatiguing, resulting in a recurrence of the original polyopia.

SUMMARY

Multiple images in monocular vision after protracted ocular work (reading)—phenomena not previously reported in the literature—are described here from experiences reported by the writer and other observers. Since this is a matter of limited observation, confirmatory reports from other observers are needed.

Various possible physiological explanations of the phenomena are considered, which involve psychological interpretations of ophthalmological events. The basic processes are ocular reflex adjustments which correct native conditions of polyopia; when these adjustments are blocked by fatigue, original defects are released and polyopia (multiple images) reappear.

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[13] G. K. Smelser, Private communication.
[14] Bartley, Private communication.

THE ROLE OF SET IN THE PERCEPTION OF SIMULTANEOUS TACTILE STIMULI

By HYMAN KORIN and MAX FINK, Glenn Oaks, Long Island

The influential role of 'mental set' in determining a subject's response to a perceptual task has been well documented.[1] In studies of the perception of simultaneous, tactile stimuli, various patterns of response have been observed which seemed to be the result of a set induced by suggestion. This investigation was undertaken to determine the relation between different conditions of 'set' and the frequency and type of perceptual error elicited in tests with simultaneous, tactile stimuli.

Recently the advantages of the simultaneous stimulation of different body-parts in tests of tactile perception have been stressed.[2] Simultaneous stimulation may elicit perceptual errors under conditions in which successive single stimulations are correctly perceived. When two stimuli are applied to body-parts at the same time, only one stimulus may be reported —an error referred to as 'extinction'; or one stimulus may be perceived correctly and the other mislocalized—an error called 'displacement.' Occasionally, if a single stimulus is interspersed in the testing-sequence, it may be reported correctly, but an additional, extraneous stimulus may also be reported—an error of 'confabulation.' Such errors of extinction, displacement, and confabulation are significantly increased in patients with brain dysfunction.

When errors of extinction and displacement occur, they are elicited in a consistent pattern. Thus, on stimulation of the hand and face, the stimulus to the face is usually reported correctly, while that to the hand is mislocalized or not reported. By testing various combinations of body-parts, an 'order of dominance' may be determined in which stimuli to the face and genital areas are most often perceived and those to the hand are

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[1] J. J. Gibson, A critical review of the concept of set in contemporary experimental psychology, *Psychol. Bull.*, 38, 1941, 781-817; Robert Leeper, Cognitive processes, in S. S. Stevens (ed.), *Handbook of Experimental Psychology*, 1951, 730-757.

[2] M. B. Bender, *Disorders in Perception*, 1952; M. B. Bender, M. A. Green, and Max Fink, Patterns of perceptual organization with simultaneous stimuli, *A.M.A. Arch. Neurol. & Psychiat.*, 72, 1954, 233-255; Fink, Green and Bender, The face-hand test as a diagnostic sign of organic mental syndrome, *Neurol.*, 2, 1952, 46-58.

least often perceived. Between these extremes, stimuli to the shoulder, foot, buttock, breast, back, thigh and abdomen are perceived in a gradient.[3]

Theories involving factors of rostral dominance,[4] maturation,[5] inattention,[6] and inherent body-image,[7] have been advanced to explain the organization of these perceptual patterns, but no one theory has adequately explained all the facts. We have ascribed significance to the relative intensity of the stimuli and the threshold-value in the frequency and the pattern of the 'extinction' error, when electrical stimuli are applied at threshold and suprathreshold intensities.[8]

The present study was undertaken to assess the relation between 'set' induced by suggestion and errors of 'confabulation' and 'displacement.' The specific problem studied is whether an 'inquiry' into the testing procedure is significantly related to the frequency and type of these errors. Since these errors are most prominent in *Ss* with cerebral dysfunction, patients undergoing convulsive and subconvulsive therapies were studied.

Subjects. The *Ss* were 61 consecutive psychotic patients referred for electroconvulsive therapy. Their ages ranged between 21–67 yr., mean age being 46 yr. Thirty-seven of them received convulsive therapy; 14 first received subconvulsive therapy and then convulsive therapy; and 10 received subconvulsive therapy alone. The *Ss* were selected for convulsive or subconvulsive treatment on a random basis by the supervising psychiatrist.

Procedure. Two model S-4B Grass square-wave stimulators were synchronized to deliver either single or simultaneous electrical stimuli. An isolation unit was connected with each stimulator to eliminate artifacts and the output was visually monitored by an oscilloscope. A switch-box was inserted in the circuit to permit independent selection of the various parts of the body. The active and indifferent electrodes for each part were small ⅜-in. steel disks, placed 1-in. apart and secured with tape. Bentonite electrode paste was rubbed into the skin of each area before the electrodes were applied.

The patient was placed on a couch in a relaxed and supine position. To alleviate undue anxiety, the nature of the testing was described. It was emphasized that a slight tap-like sensation would be felt. The electrodes were then placed on (a) the dorsum of the hands, (b) the mandibular area of both cheeks, and (c) the medial aspect of the calves of the legs.

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[3] Bender, Green, and Fink, *op. cit.,* 233-255.

[4] R. Cohn, On certain aspects of the sensory organization of the human brain: I. A study in rostral dominance as determined by ipsilateral simultaneous stimulation, *J. nerv. ment. Dis.,* 113, 1951, 471-484; II. A study in rostral dominance in children, *Neurol.,* 1, 1951, 110-122.

[5] Louis Linn, Some developmental aspects of the body image, *Int. J. Psychoanal.,* 36, 1955, 1-7.

[6] Macdonald Critchley, The phenomenon of tactile inattention with specific references to pariental lesions, *Brain,* 72, 1949, 538-561.

[7] Bender, *op. cit.,* 77-88.

[8] Hyman Korin and Max Fink, Role of stimulus intensity in perception of simultaneous electrical cutaneous stimuli, *J. Hillside Hosp.,* 6, 1957, 241-250

Thresholds for the various body-parts were first determined. At a frequency of 0.3 cycles per sec. and a pulse-duration of 50 m.sec., the voltage was increased in uniform increments of 5 v. to the hands and 1 v. to the cheeks every 6.7 sec. (2 pulses) until *S* perceived 100% of the stimulations. After a 10-sec. interval, voltages were decreased until the sensation was no longer reported. After another 10-sec. interval, voltages were increased by 1 v. every 6 sec. until the patient again reported 100% of the stimulations. This reading was considered the minimal voltage required to produce threshold-sensation.

After thresholds were determined, testing with a random series of 4 single and 6 double simultaneous stimulations followed. The body parts tested were the right hand and left cheek (heterologous stimulation) and the right cheek and the left cheek (homologous stimulation). Stimuli were applied either simultaneously or to one part singly, in a mixed order, for 10 trials. The order of presentation of the heterologous and homologous stimulation was alternated.

Failure to report the interspersed single stimuli served as an index that the perceptual threshold had changed. At such times the threshold was again determined, and the 10 test-trials were repeated. Threshold changes, however, occurred infrequently during testing.

The patients were tested in two groups: an 'inquiry' group and a 'no-inqury' group. The 'inquiry' group, consisting of 24 convulsive and 9 subconvulsive *Ss*, was asked the question "anywhere else?" after each response to a stimulation. No question was asked of the 'no-inquiry' group, which consisted of 27 convulsive and 15 subconvulsive *Ss*. (The total number of *Ss* exceeds 61, since 1 *S* in the 'inquiry' group and 13 *Ss* in the 'no-inquiry' group were included both in the convulsive and the subconvulsive series.)

Electroencephalograms were obtained weekly, on a day following a treatment. These records were quantitatively measured for the degree of induced slow-wave (delta) activity.[9] Both the convulsive and the subconvulsive treatments were administered three times weekly on alternative days.

Results: (1) Errors of confabulation. A response was scored as a confabulation if two stimuli were reported when only a single stimulus was applied. The observations are noted in Table I.

In the 'inquiry' group, confabulatory errors were elicted before treatment from both types of *Ss*—convulsive and subconvulsive. During treatment, the mean error increased from 0.08 to 0.72 among the 'convulsive' *Ss* and from 0.22 to 0.70 among the 'subconvulsive' ones. After treatment, the mean number of confabulations persisted in the 'subconvulsive' *Ss* (1.00) but declined in the 'convulsive' ones (0.10); the difference 0.90 being significant at better than the 5% level.[10] In the 'no-inquiry' group, few confabulations occurred at any interval of testing for either the convulsive or the subconvulsive *S*s.

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[9] Max Fink and R. L. Kahn, Relation of EEG delta activity to behavioral response in electroshock: Quantitative serial studies, *A.M.A. Arch. Neurol. & Psychiat.*, 78, 1957, 516-525.

[10] The Mann-Whitney 'U'-test was used to test the significance of these data and those that follow as the scores were not drawn from a normally distributed population. Since the 'U'-test is based on rank-order of the scores, the differences between means are only grossly related to level of significance.

Before treatment, the subconvulsive, 'inquiry' *S*s made significantly more confabulatory errors than the subconvulsive, 'no-inquiry' *S*s. In a comparison of the 'inquiry and 'no-inquiry' procedures during treatment, the differences were significant both in the convulsive and subconvulsive groups of *S*s. The differences during treatment are based on the substantial increase in the number of confabulations of the 'inquiry' group. After treatment, the confabulations of the convulsive, 'inquiry' group decreased to the pretreatment level, and the differences between the convulsive, 'inquiry' and 'no-inquiry' groups were not significant. Though the mean number of errors of the subconvulsive, 'inquiry' *S*s increased after treatment, a comparison between the 'inquiry' and 'no-inquiry' subconvulsive *S*s could not be made. Data were not obtained after treatment from the subconvulsive 'no-inquiry' *S*s because they were transferred to convulsive treatment and were not available for testing.

TABLE I

MEAN NUMBER ERRORS OF CONFABULATION

Period	Inquiry		No inquiry		Diff. between inquiry and no-inquiry		Diff. between convulsive and subconvulsive	
	convulsive ($N=24$)	sub-convul. ($N=9$)	convulsive ($N=27$)	sub-convul. ($N=15$)	convulsive	sub-convul.	inquiry	no-inquiry
Pretreatment	.08	.22	0	0	.08	.22‡	.14	0
Treatment	.72	.70	.11	.05	.61†	.65†	.02	.06
Post-treatment	.10	1.00	.06	—	.04	—	.90*	—

* $p<0.05$; † $p<0.03$; ‡ $p<0.01$.

(2) Errors of displacement. A response was scored as a displacement if the locus of one of two stimuli was reported correctly and the other incorrectly. Displacements were rarely elicited from the *S*s in any of the groups (Table II). The mean number of displacements tended to increase during treatment for the convulsive *S*s, but the differences from the pretreatment period lack significance.

(3) Errors of extinction. An error was scored as an extinction if only one of two simultaneously applied stimuli was reported. The difference in the number of errors of extinction between the 'inquiry' and 'no-inquiry' groups was not significant at any period during the course of therapy both for the convulsive and subconvulsive *S*s (Table III). During treatment, the mean number of extinctions decreased in all groups. At this period,

the difference between the convulsive and subconvulsive, 'inquiry' *S*s was significant. After treatment the errors of all the groups decreased further.

(4) Errors of confabulation and changes in EEG. An analysis was made of the number of confabulatory errors elicited in convulsive *S*s in relation to the degree of electroencephalographic change. 'Inquiry' *S*s with high degrees of delta activity made significantly more confabulatory errors than inquiry patients with moderate and low degrees of delta activity (Table IV), while few errors were reported by the 'no-inquiry' *S*s regardless of the change in the *EEG*. The mean scores of the moderate and low *EEG* among the 'inquiry' *S*s was similar to the mean scores of the 'no-inquiry' ones.

No *EEG* slow-wave activity or low degrees of such activity occurred in the subconvulsive *S*s. As had been indicated, however, the number of confabulatory errors of the subconvulsive group increased significantly during and after treatment. This increase resulted from increasing confabulatory errors in four of the nine patients.

TABLE II

MEAN NUMBER ERRORS OF DISPLACEMENT

Period*	Convulsive		Subconvulsive	
	inquiry	no-inquiry	inquiry	no-inquiry
Pretreatment	.06	.07	0	0
Treatment	.09	.10	.02	.01
Post-treatment	.08	.02	.06	0

* Inter- and intra-group differences are not significant at any period.

Discussion. Errors of displacement, confabulation, and extinction are elicited when sequences of multiple and single tactile stimuli are applied to various parts of the body. In clinical tests with touch stimulation, these errors are most prominent in patients with cerebral disease.[11] Theories which have been advanced to account for the occurrence of such errors have therefore emphasized endogenous factors involving the central nervous system. Numerous studies of the role of set in perception indicate, nevertheless, that the frequency and type of response to a perceptual task may be markedly altered by the immediate aspects of a situation.[12] In this study the stimulus-situation has been varied to bring about differing conditions of mental set. The endogenous factors have not, however, been neglected and the relation between the effects of different degrees of brain dysfunction has also been determined.

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[11] Fink, Green, and Bender, *op. cit.*, 46-58.

[12] Leeper, *op. cit.*, 752-757; Max Pollack, W. S. Battersby, and M. B. Bender, Tachistoscopic identification of contours in patients with brain damage, *J. comp. physiol. Psychol.*, 50, 1957, 220-227.

In the course of convulsive therapy a marked increase in the number of confabulatory errors is brought about by the *E*'s query: "anywhere else?" which followed every stimulation. Of the convulsive *S*s who were asked this question, confabulations were elicited primarily in the group with high degrees of *EEG* slow-wave activity (marked cerebral dysfunction) and not in the group with low and moderate degrees (minimal cerebral dysfunction). The importance of the inquiry is emphasized by the consideration that, regardless of changes in the *EEG*, there was little tendency for confabulatory errors to occur among the convulsive *S*s when no inquiry was made. Thus both inquiry and high degrees of *EEG* delta activity provided the milieu favorable to evoking confabulatory errors in the convulsive therapy *S*s.

TABLE III

MEAN NUMBER ERRORS OF EXTINCTION

Period	Inquiry		No inquiry		Diff. between inquiry and no-inquiry		Diff. between convulsive and subconvulsive	
	convulsive (N=24)	sub-convul. (N=9)	convulsive (N=27)	sub convul. (N=15)	convulsive	sub-convul.	inquiry	No inquiry
Pretreatment	2.16	1.67	2.76	2.37	.60	.70	.49	.39
Treatment	2.03	1.14	1.71	1.27	.32	.13	.89*	.44
Post-treatment	1.37	.89	1.44	—	.07	—	.48	—

* $p<0.05$.

TABLE IV

RELATION BETWEEN *EEG* DELTA ACTIVITY AND MEAN NUMBER OF CONFABULATORY ERRORS

Group	Degree of Delta activity			
	high	moderate-low	Diff.	Signif.
inquiry	.81 (N=9)	.19 (N=10)	.62	$p<0.05$
no inquiry	.10 (N=21)	.07 (N=6)	.03	N.S.

Subconvulsive *S*s present a different picture. Although virtually no delta activity is induced by subconvulsive therapy, the number of confabulatory errors of four of the nine subconvulsive *S*s queried increased substantially during the treatment. Furthermore, while the confabulatory errors of these four subconvulsive *S*s persisted and even increased following the course of therapy, the errors of the convulsive *S*s queried, in contrast, decreased to the pretreatment level. Patterns of reversible error manifested by convulsive *S*s have been reported in the various studies of the effects of electroshock on different types of mental functioning.[13] It was expected, however, that confabulations would not be elicited in subconvulsive *S*s at any period, in view of earlier observations that cerebral dysfunction is not induced in these patients.[14]

An explanation for the differences between the 'convulsive' and subconvulsive 'inquiry' *S*s is that their therapies had differing effects on the factor of practice. In 'convulsive' *S*s, treatment diminished the practice-effect, including those both with low and high degrees of slow-wave *EEG* activity. For each test-interval, it was as if the 'convulsive' *S*s were starting anew. Under these conditions, only *S*s with high *EEG* delta activity manifested a confabulatory set within a single test-period. After the course of therapy, with the disappearance of the delta activity, convulsive *S*s were performing at the pretreatment-level. In the 'subconvulsive' *S*s, the set established in the pretreatment-interval was reinforced during each test-period during treatment. Thus the subconvulsive *S*s made even more confabulatory errors after treatment.

The results for the subconvulsive group of *S*s indicate that certain of them may make confabulatory errors without an alteration of brain-function. Such *S*s are apparently influenced by the *E* and may be described as being suggestible or acquiescent. The failure of the convulsive *S*s to establish a set which persisted for prolonged intervals of time, as did the subconvulsive *S*s, suggests a basis for the therapeutic effect derived from convulsive therapy. If *S*'s symptoms are regarded as a pathological, mental set, such an interpretation is particularly appropriate. From the point of view of concepts of mental set, the effect of induced convulsions is to bring about a disruption of maladaptive patterns of behavior.

The number of displacement-errors remained the same regardless of whether an inquiry was made. These errors occurred much less frequently than confabulations. During treatment, approximately 30% of the convulsive *S*s of both the 'inquiry' and 'no inquiry' groups responded with at least one displacement. This finding compares closely with the results of 33% with displacements obtained in a study of a similar population of electroshock *Ss* in which touch-stimuli were applied by the clinical method. Errors of displacement are not a prominent type of error in an electroshock population.

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[13] Hyman Korin, Max Fink, and S. Kwalwasser, Relation of changes in memory and learning to improvement in electroshock, *Conf. Neurol.*, 16, 1956, 88-96; Max Fink, R. L. Kahn, and Hyman Korin, Effects of diffuse altered brain function on perception, *XV Conf. of Psychol. Proceed.*, 1958, 238-239.

[14] Fink, Kahn, and Green, Experimental studies of the electroshock process, *Dis. Nerv. Sys.*, 19, 1958, 113-118.

With regard to errors of extinction, differences were not significant between the 'inquiry' and 'no inquiry' groups. The high number of errors of extinction before treatment and the subsequent decrease in errors during treatment, noted in this study, is in contrast to the results obtained with clinical tactile techniques. If clinical methods are used, few errors of extinction are elicited before treatment and there is a marked increase in error during treatment. The results obtained in this study are probably related to the initial difficulty experienced by *Ss* in perceiving electrical stimuli at threshold and the rapid adaptation to the technique in further testing. These factors play a greater role than the changes induced by the treatment.

In initial studies with threshold electrical stimuli, it was believed that a more sensitive test of changes in brain-function than the clinical tactile method could be devised.[15] For clinical purposes, however, the perceptual patterns obtained with electrical stimulation lack sufficient discriminability as indices of brain dysfunction. In part, the deficiencies of the method may be ascribed to the necessity for using fixed electrodes and limitations in switching arrangements at threshold. For clinical testing, therefore, simultaneous tactile stimuli applied rapidly in a varied sequence remains the best index of altered brain function.[16]

Summary and conclusions. This study of the perception of simultaneously applied tactile stimuli was undertaken to determine the relation between the frequency of perceptual errors to the inquiry made by *E.* The relations among inquiry, perceptual response, and the degree of brain dysfunction were also considered.

In the test-procedure, the threshold (100% point) for square-wave electrical stimuli applied to the hand and cheek of 61 psychiatric patients was determined. Sequences of two simultaneous and single stimuli were applied in a mixed order for the hand and cheek (heterologous stimulation) and both cheeks (homologous stimulation). Heterologous and homologous trials were alternated for each patient. For one group, an inquiry was made following each response to a stimulation, while in a second group, no inquiry was made. The *S*s were treated either by convulsive or subconvulsive courses of therapy, at three times a week for 12–20 applications.

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[15] Fink, Green, and Bender, *op. cit.,* 46-58.

[16] Green and Fink, Standardization of the face-hand test, *Neurology,* 4, 1954, 211-217.

There was a significant relationship between the frequency of confabulatory errors and the inquiry ('suggestion-induced set') in both convulsive and subconvulsive patients. The confabulatory tendencies of these patients, however, differed greatly. Although the errors for both increased during treatment, errors decreased after treatment for the convulsive group, but in the subconvulsive group errors increased further. The differences between 'inquiry' and 'no inquiry' groups with regard to errors of extinction or displacement were insignificant. In 'convulsive-inquiry' *S*s, the confabulatory errors of those with high degrees of *EEG* slow-wave activity were significantly more frequent than those with a low or moderate degrees of slow wave activity.

The results of this study lead to the following conclusions:

(1) In tests with simultaneous electrical tactile stimuli the number of confabulatory errors is related to an induced set suggested by *E*'s inquiry.

(2) The number of confabultory errors is increased in *S*s with brain-dysfunction in relation to an inquiry, but may also be induced in patients without brain-dysfunction who are acquiescent and susceptible to suggestion.

(3) The frequency of errors of displacement or extinction is not related to the 'inquiry' procedure.

THE EFFECT OF MEANING ON BINOCULAR RIVALRY

By A. H. HASTORF and G. MYRO, Dartmouth College

Until very recently, studies of binocular rivalry paid little attention to the role of meaningful content in determining binocular resolution. It is true that when there is a sufficient difference between the stimulus-pattern presented to one eye and the stimulus-pattern presented simultaneously to the other eye, the pattern seen by the observer can be interpreted as being a function of the formal properties of the two stimuli presented (apart from the physiological condition of the eyes).[1] It has, however, recently been demonstrated that when the two monocular stimuli have definite meaningful content, what is seen by the observer is no longer a function merely of the formal stimulus-properties.

Engel has used a stereoscopic arrangement to present a photograph of a human face in the usual upright orientation to one eye of *O* and the same photograph upside-down to the other eye.[2] The two monocularly presented faces stimulated corresponding areas of the two retinas and, therefore, occupied the same portion of the binocular field. Results indicated that the upright face was seen more often, more clearly, and more completely, wholly or in part suppressing the inverted face. The effect had a distinct 'sensory' character and did not appear to be a question of judgment or interpretation. The right-side-up face was *seen* as *excluding* the upside-down one, rather than recognized in a superimposed pattern.

Similarly Engel found that when a photograph of the head of a statue and the photograph of the genital area of the same statue were presented by the same technique, the genital area tended to be 'suppressed,' *i.e.* the head tended to be seen to the total or partial exclusion of the genital area.[3] Bagby investigated the variable of 'cultural meaning' by presenting scenes of American and Mexican life by the same stereoscopic technique to American and Mexican *O*s. He found that American *O*s tended to see American scenes, and Mexican *O*s tended to see Mexican scenes.[4] Pettigrew, Allport, and Barnett used the stereoscopic technique to investigate racial identification in South Africa.[5] They presented pairs of non-identical photographs

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[1] M. D. Vernon, *A Further Study of Visual Perception,* 1952, 204-205; and R. S. Woodworth, *Experimental Psychology,* 1935, 573, 659.

[2] Edward Engel, The role of content in binocular resolution, this JOURNAL, 69, 1956, 87-91.

[3] Engel, personal communication.

[4] J. W. Bagby, A cross-cultural study of perceptual predominance in binocular rivalry, *J. abnorm. soc. Psychol.,* 54, 1957, 331-334.

[5] T. F. Pettigrew, G. W. Allport, and E. D. Barnett, Binocular resolution and perception of race in South Africa, *Brit. J. Psychol.* (in press).

of different racial types stereoscopically and found a significant correlation between the *O*'s own race and his judgment of the race of the individual seen binocularly in the stereoscope. They also found a tendency among the Afrikaaners to perceive race in terms of extremes, as compared with the other *O*s.

The implication of these studies seems to be that the organism tends to resolve the contradictory information of the two monocular stimuli in such a way as to perceive that content which is the most meaningful or the most important for him. There are also indications that the importance of the content for a given *O* may influence the degree to which he suppresses alternate content, and that there may be individual differences in tolerance of incongruities in the visual field.

The present study was done to confirm the results of Engel's first-mentioned experiment under conditions which would exclude as much as possible reporting error due to interpretation. Engel exposed the paired right-side-up upside-down faces in the stereoscope for a period of 1 min. His own results indicate that changes in the visual field occurred during this interval for many *O*s, parts or even the whole of the right-side-up face giving way to the upside-down one, and *vice versa*. Under such conditions deciding which face was predominant would be a judgment involving the possibility of error. An upside-down face, even seen binocularly without interference, is incongruous and hard to make out. The right-side-up face might be reported to predominate over the confusing upside-down one, although both might be equally strongly represented in the visual field.

The present experiment was designed to answer three different but interrelated questions: (1) Would the effect of meaningful stimuli as described by Engel hold up when exposure speeds were reduced to 0.1 and 0.2 sec.? (2) If stimuli differ in color as well as in meaningful content, can any interaction be shown between these two aspects of the stimuli? For example, is there any evidence that the color of the meaningful stimulus will predominate or are these two aspects of the stimuli relatively independent? (3) What is the effect of increasing exposure-time? As exposure-time is increased from 0.1 sec. to 0.2 sec. are there any consistent effects such as an increase or a decrease in the reporting of ambiguity?

METHOD

In Series I and II, stamps of three different designs were employed as stimuli: the 1¢ green George Washington, designated 'G'; the 1½¢ tan-brown Martha Washington, designated 'M'; and the 2¢ red John Adams, designated 'A'. In each stamp the figure is in profile, facing toward the right, located in the left half of the stamp, and somewhat above the middle. A capital letter will be used to indicate that the stamp designated by that letter is right-side-up, and a lower case letter to indicate that the stamp designated by that letter is upside-down. When the stamps are paired, the first letter in each pair indicates the stamp presented to the left eye, the second letter the stamp presented to the right eye.

The stamps were paired in the following sets of combinations:

Set A		Set B		Set C	
Card	Stamps	Card	Stamps	Card	Stamps
1	G–g	7	G–m	15	G–M
2	g–G	8	m–G	16	M–G
3	M–m	9	M–g	17	G–A
4	m–M	10	g–M	18	A–G
5	A–a	11	G–a		
6	a–A	12	a–G		
		13	A–g		
		14	g–A		

In Set A, stamps of the same design but of inverse orientation were paired against each other. In Set B, stamps of different designs and inverse orientation were paired against each other. In Set C, stamps of different designs but same orientation were paired against each other. For each pairing there were two instances, differing only in the eye to which a given member of the pair was presented (*e.g.* M-g and g-M).

Set A was used to determine whether, of two stimuli differing only in orientation and otherwise identical, the right-side-up one would predominate. Set B was likewise used to investigate the effect of orientation and to determine in addition whether the tendency to perceive the right-side-up member of a pair would increase the tendency to perceive the color of the right-side-up stamp rather than the color of the upside-down stamp (whether 'right-side-upness would pull in color'). For example, if red should be seen only when the right-side-up stamp is red and never when the red stamp is upside-down, then this would indicate that the meaning of the stimulus structures the apparent color of the stimulus. Set C was used to establish the relative predominance of colors alone (or of the figures and colors together), when the monocular stimuli did not differ in orientation.

In Series III, two pairs of photographs of men's faces were mounted on the stimulus-cards in the manner described above. The two faces in each pair were identical, but were mounted in inverse orientation. The right-side-up face was presented to the left eye in one pair and to the right eye in the other.

Apparatus. A modified Engel stereoscope was employed. It permits adjustments for *O* that corresponding points in the two monocular visual fields coincide in the binocular field, and the relative illumination in the two monocular fields is such that they are equally well represented in the binocular field. The modifications consisted of accommodations for using stimulus-cards (described below) and of the attachment of an interval timer for operating the stereoscope lights for a specified interval. The stimuli were mounted on 7 × 3⅝-in. blank Stoelting stereoscopic slides symmetrically on both sides of the shorter mid-line of the card, the center (point of symmetry) of each stimulus being 1⅜ in. from the mid-line and equidistant from the two longer edges of the card.

Observers. The *O*s were 6 women students of nursing in their late teens and early twenties, and 6 men, ranging from the late teens to the early thirties. None showed any indication of visual defects which might prevent them from fusing and seeing clearly stimuli presented in the stereoscope.

Procedure. *O* was seated comfortably before the stereoscope and instructed to keep his forehead pressed against the metal view-piece. A stereogram having a small circle as one monocular stimulus and a larger circle as the other was inserted into the stereoscope, and *O* was asked to manipulate the adjusting knob until the two circles became concentric. Since the centers of the circles corresponded to the centers (points of symmetry) of stimuli on other cards, this setting insured that any two paired hetero-ocular stimuli occupied the same area in the visual field. Next a Veerhoeff Test stereogram was inserted, and the *O* was asked to report what he saw. The illumination for each monocular stimulus was varied independently until *O*'s report indicated that both eyes were used equally. Due to a minute unevenness of light-distribution over the stereogram, the exact positive result could not always be obtained, and instead of a precise adjustment at which both eyes were equally used, only a range between clear predominance of one eye and clear predominance of the other eye could be obtained.

The Veerhoeff Test gives the point of equal use for achromatic hetero-ocular stimuli, and this point differed somewhat for chromatic stimuli for most *O*s. If it was found, when the first several experimental stimuli were presented, that *O* saw consistently only stimuli presented to one of the two eyes, further adjustments in illumination were made. The control for eye-dominance was deemed sufficient when, in a series of several presentations, *O* reported at least once the left-eye member of a pair of hetero-ocular stimuli and at least once the right-eye member of a pair, *i.e.* when *O* had seen at least one stimulus with each eye.

Instructions. After the adjustments the following instructions were given:

> This is an experiment in visual recognition of objects under different conditions, such as the amount of illumination and the period for which they are presented. The stimuli will be presented successively and will be on only a short period of time. When you hear the word 'ready,' open your eyes and be prepared to see the object. When you are so prepared, answer 'ready,' and the stimulus will be flashed on. Do not try to say anything during the short period that the stimulus is on. When the stimulus disappears, report first the *color* of what you saw, then the *figure,* if any, and any other characteristics. For example, you may see a face several times during the series. Report which way it is facing: right, *i.e.* toward (one of the *Es*), left, *i.e.* toward (the other of the *Es*), or straight ahead, *i.e.* at you (the *O*). Remember, report the color, the figure, if any, and which way it is facing.

Following the instructions and readjustments, whenever necessary, Series I was presented (Cards 7, 13, 8, 14, 10, 11, 12, 9, 1, 4, 6, 3, 5, 2, 16, 17, 15, 18). Each stimulus-card was presented for 0.1 sec. Series II was presented without interruption at the end of Series I; the same cards were used in the same order, but each presentation lasted 0.2 sec. At the end of Series II, again without interruption, the two stimulus-cards of Series III were presented for intervals of 0.2 sec. each.

One of the *Es* operated the apparatus, while the other recorded the reports of the *Os*. *O* was interviewed during the experiment for one or more of the following reasons: (1) he was reminded to report color, figure, and direction; (2) he was asked whether the figure was "all right" whenever his report did not indicate clearly whether the figure was right-side-up or not; (3) if he reported seeing both figures, he was asked: "did either appear clearer or stronger?"; (4) after predetermined presentations he was asked: "do you see anything else?" Of the additional information, whether in response to this question or volunteered at other times, remarks about denomination, lettering, and the like, were usually not recorded, but hues appearing in addition to the predominant color and indication of figures or patterns other than the ones seen clearly were.

RESULTS

From the verbal report by each *O* for each stimulus-card the following data were abstracted whenever possible: (1) the color of the stamp, (2) the orientation of the figure or figures, (3) the relative predominance of one (in terms of relative intensity and clarity) or the equivalence of the two figures.

The colors reported were categorized into (a) green, (b) reddish, (c) both green and red, (d) other colors—*e.g.* purple, gray, (e) no color. No distinction was made between the red of Stamp A and the tan of M since the two were never paired.

The reported orientations were grouped into six categories: (a) Right-side-up exclusive—when *O* reported seeing a right-side-up figure only, without indications of any other figure, or very infrequently (2 times at 0.1 sec; 8 times at 0.2 sec.) "something else" not another figure. (b) Right-side-up predominant—when *O* reported two figures, both visible, but the right-side-up one definitely stronger and clearer. (c) Equal—when *O* reported both faces visible. (d) No-face—when *O* reported no figure or "don't know what it is." (e) Upside-down predominant—when *O* reported both figures as visible, but the upside-down definitely stronger and clearer. (f) Upside-down exclusive—when *O* reported an upside-down figure only, without indications of any other figure, or very infrequently (5 times at 0.1 sec., 4 times at 0.2 sec.) with "something else" other than a figure.

Three criteria were available in *O*'s report for judging whether he saw a right-side-up or an upside-down figure:

(i) Usually very early in the test *O* reported with some surprise that a stamp he had just seen was upside down. It is reasonable to suppose that the first seen upside-down figure was thus explicitly reported. Usually after this *O* volunteered whether a stamp he was describing was right-side-up or upside-down. In the rare instances in which *O* did not volunteer information about orientation after the first seen-upside-down stamp, he was asked whether what he reported as having seen had been right-side-up or upside-

down. Before *O* reported his first seen upside-down stamp, he was often asked: "was it all right?" when he described a stamp or a figure on it. Thus the first criterion for determining orientation was the *O*'s *explicit report of it*.

(ii) The second criterion was the *facing direction* of the profile on the seen stamp. The faces on all stamps are in profile facing toward the right. Consequently, when the stamp is upside-down, the figure is facing toward the left. Hence facing right indicates right-side-up orientation, and facing left upside-down orientation. It will be remembered that *O* was explicitly instructed to report in which direction the figure, if any, was facing.

(iii) The third criterion, obtained whenever necessary by additional interviewing of *O* immediately after a particular report, was the *location of the face or figure on the stamp*. The profiles on the stamps are all in the left half, somewhat above the middle of the stamp. Consequently, a report of a figure in the left half or above the middle tended to indicate right-side-up orientation. The third criterion is the least reliable, since a few times *O* fused both figures in the center of the stamp ("anomalous fusion"). When the localization was unambiguous, *i.e.* upper left or lower right, it was used to supplement other information.

TABLE I

RELATIVE FREQUENCY OF PERCEIVING THE STIMULI

Series	Exposure	Right-side-up exclusive	Right-side-up predominant	Equals	No-face	Upside-down exclusive	Upside-down predominant	Total
I	0.1 sec.	65	15	15	33	36	4	168
II	0.2 sec.	56	24	27	16	39	6	168
III	0.2 sec.	11	7	3	0	1	2	34

In the great majority of cases there was no question about the orientation of the figure seen. Infrequently, one of the first two criteria in conjunction with an unambiguous localization were deemed sufficient, even in the absence of the third criterion. All cases which could not be clearly decided, were considered 'anomalous fusions' and assigned to the equal category. Where there was only localization, without specification of orientation or facing direction, the cases were assigned to the no-face category.

An inspection of Table I indicates that righ-side-up figures are seen, whether exclusively or as the predominating of two figures, more often than upside-down figures, and that this true both at the 0.1 sec. and 0.2 sec. periods of presentation.

The sign test was applied to the data by taking each *O* separately and determining if he reported more right-side-up than upside-down figures.[6] For both the Exclusive category and the Predominant category, more *Os* saw the right-side-up patterns by a difference significant at 5% level of confidence.

When the color reported and the figure reported are compared to the stimulus-card presented, the data obtained are as presented in Table II.

It should be noted that the only cases used in this analysis are those in which one figure was seen exclusively and the color was reported as clearly greenish or clearly reddish. The reports for both time intervals were combined. The data indicate that in about two-thirds of the cases the color reported was the same as that of the figure reported. It is interesting to note, nevertheless, that there were a considerable number of cases (42) when *O* apparently separately selected the figure from one stimulus and the color from the other. There is no indication in these data that the more familiar right-side-up figure tends to pull in its color any more often than the upside-down figure.

TABLE II

RELATION OF COLOR REPORTED TO FIGURE REPORTED

Figure seen	Color reported that of figure seen	Color reported that of figure not seen	Total
Right-side-up	41	25	66
Upside-down	33	17	50
Total	74	42	116

TABLE III

RELATIVE FREQUENCY OF REPORTING AMBIGUOUS OR UNAMBIGUOUS PERCEPTION AS A FUNCTION OF STIMULUS-IDENTITY AND PRESENTATION-TIME

Reports	Identical stimuli		Non-identical stimuli	
	0.1 sec.	0.2 sec.	0.1 sec.	0.2 sec.
Exclusive, either right-side-up or upside-down	40	28	61	67
Both visible, equals or one predominating	20	35	15	23
No-face	12	9	20	7

When the reports from Set A are compared with the reports from Set B, we obtain the results reported in Table III. Set A consists of two identical stimuli of which one is inverted. In set B, one stimulus is inverted but it is also a stamp different from the normally inverted one.

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[6] Sidney Siegel, *Nonparametric Statistics for the Behavioral Sciences*, 1956, 68-75.

These appears to be a clear tendency to report one figure exclusively less often as the interval of presentation is increased. It is reasonable to suppose that the shorter the interval during which information is available the smaller is the amount of information an *O* can obtain, and that under such circumstances it is highly selective. It should be noted that all *O*s went through the 0.1-sec. series prior to the 0.2-sec. series hence they had greater knowledge concerning the stimuli during the 0.2-sec. series. There is also an apparently greater incidence of reporting one or the other figure alone for the non-identical stimuli than for the identical.

SUMMARY

Pairs of stimuli (stamps and photographs) containing human faces were presented hetero-ocularly, *i.e.* a different stimulus to each eye, with one face right-side-up, the other upside-down. The stimuli were presented stereoscopically for periods of 0.1 and 0.2 sec. It was found that significantly more right-side-up faces were seen then upside-down ones.

The present study confirms the results obtained by Engel in a similar experiment in which the stimuli were presented for periods of 1 min. The shorter period of presentation seems to insure that the relative predominance of right-side-up figures is not a function of mere estimation of the balance between two alternating stimuli. It appears that the meaningful content of stimuli play a significant role in the organization of experience.

THE ABSOLUTE RESPONSE IN TESTS OF GENERALIZATION IN NORMAL AND RETARDED CHILDREN

By RITA G. RUDEL, New York University

Even a brief acquaintance with the vast literature on transposition of response is sufficient to convince the reader that the manipulation of the variables—stimulus and subject(S)—can produce absolute or relational responses, and concomitant evidence for absolute (S–R) and relational (Gestalt) theories. In most transposition-experiments, S is trained first to respond consistently to one of two stimulus-objects varying in a single dimension *e.g.* to choose the larger or the smaller of two objects, and then is tested by asking him to make a choice between the object originally rewarded and the one that is still larger (or smaller). Choice of the original object constitutes an absolute response; choice of the other (the new object) is a relational response.

What makes for absolute responses? (Stimulus-variables). Leaving aside the alternative theoretical explanations of transposition, there are definite stimulus-conditions which predispose S to respond to the same object in the test as he did during training, *i.e.* to make an absolute response. It has been shown that a large difference between the stimulus-objects[1] or their successive presentations[2] tends to elicit absolute responses. In a recent paper on the transposition of intermediate size,[3] a further condition was made explicit, *i.e.* that absolute responses are more frequent when testing immediately follows training. When testing is delayed, the same stimulus-difference that tended to evoke predominantly absolute responses now leads to relational re-

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[1] K. W. Spence, The differential response in animals to stimuli varying within a single dimension, *Psychol. Rev.*, 44, 1937, 430-444; D. L. Wolfle, Ability of white rats to learn an absolute brightness discrimination, *Psychol. Bull.*, 33, 1936, 728-729; Wolfgang Köhler, Aus der Anthropoidenstation auf Teneriffa: IV. Nachweis einfacher Strukturfunktionen beim Schimpansen und beim Haushuhn, *Abh. preuss. Akad. Wiss., phys. math. Kl.*, 2, 1918, 3-101.

[2] R. A. Baker and D. H. Lawrence, Differential effects of simultaneous and successive stimulus presentations on transposition, *J. comp. physiol. Psychol.*, 44, 1951, 378-382; T. A. Jackson and Edward Jerome, Studies in the transposition of learning by children: IV. A preliminary study of patternedness in discrimination learning, *J. exp. Psychol.*, 26, 1940, 432-439. H. E. Jones, and Dorothy Dunn, The configural factor in children's learning, *J. genet. Psychol.*, 41, 1932, 3-15.

[3] R. G. Rudel, Transposition of response by children trained in intermediate-size problems, *J. comp. physiol. Psychol.*, 50, 1957, 292-295.

sponses. This evidence points to the likelihood that 'absolute' and 'relative' reactions are but two sides of the discriminative coin; they may not be the independent, or even contrasted, outcomes they have been assumed to be. The data suggest, furthermore, that an absolute discrimination not only arises together with the relational, as Klüver has hypothesized,[4] but is somehow dependent on the perception of the relationship between the stimulus-objects. It is with this problem that the present paper is primarily concerned.

Who makes the absolute response? (Subject-variables). The question of who is more likely to respond in absolute terms (to the object originally rewarded) may be posed on three levels: phylogenetic, ontogenetic, and pathologic. Bierens de Haan concluded that the bee could respond only in absolute and not in relative terms,[5] a conclusion subsequently challenged by Hertz.[6] Ontogenetically, the absolute choice has been said to occur in the presence of language,[7] and in the absence of language.[8] It has been characterized as the simpler, more elementary response,[9] and as the more complex.[10] Goldstein's assumption that brain-damaged *S*s are abnormally "concrete" leads one to expect that these *S*s should respond primarily in absolute terms.[11] This expectation follows as well from Mednick's hypothesis that gradients of generalization are more constricted in patients with brain damage.[12]

While our first problem is concerned with some stimulus-conditions for absolute response, we have also tried to clarify some of the *S*- variables. We have posed the question to two groups of *S*s, normal and retarded (Mongoloid) children.

METHOD AND PROCEDURE

We arranged three situations (based on the procedure of Jackson and Eckhardt,[13] to see how an opportunity for comparison of the stimulus-objects during training and testing affects selection of an object from a continuum varying in a single dimension. These three experimental tasks (Situations A, B, and C) are shown diagrammatically in Fig. 1. They are:

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[4] Heinrich Klüver, *Behavior Mechanisms in Monkeys,* 1933, 83-84, 320-328.

[5] J. A. Bierens de Hann, Ueber Wahl nach relativen und absoluten Merkmalen (Versuche an Affen und Bienen), *Z. vergl Physiol.,* 7, 1928, 462-487.

[6] Mathilde Hertz, Die Organisation des optischen Feldes bei der Biene, *Z. vergl. Physiol.,* 8, 1929, 693-720.

[7] A. H. Gayton, The discrimination of relative and absolute size by albino rats, *J. comp. Psychol.,* 7, 1927, 93-105; August Riekel, Psychologische Untersuchungen an Hühnern, *Z. Physiol.,* 89, 1922, 81-115.

[8] Elizabeth Alberts and David Ehrenfreund, Transposition in children as a function of age, *J. exp. Psychol.,* 41, 1951, 30-38; M. R. Kuenne, Experimental investigation of the relation of language to transposition behavior in young children, *ibid.,* 36, 1946, 471-486.

[9] William Line, The growth of visual perception in children, *Brit. J. Psychol., Monog. Suppl., 5,* 1931 (No. 15), 80-84.

[10] Jackson and Jerome, Studies in the transposition of learning by children: VI. Simultaneous vs. successive presentations of the stimuli to bright and dull children, *J. exp. Psychol.,* 33, 1943, 431-439.

[11] Kurt Goldstein, *The Organism,* 1939, 30-32.

[12] S. A. Mednick, Distortions in the gradient of stimulus generalization related to cortical brain damage and schizophrenia, *J. abnorm. soc. Psychol.,* 51, 1955, 536-542.

[13] T. A. Jackson and M. E. Eckhardt, Studies in the transposition of learning by children: V. The number of stimuli in the training series as a factor in generalization, *J. exp. Psychol.,* 27, 1940, 303-312.

Situation A. Single object training (one stimulus-object) followed by choice from an array of objects presented together and arranged in order of size (no comparison).

Situation B. Differential training (two stimulus-objects of which the smaller is positive) followed by choice from an array of objects presented together and arranged in order of size (comparison-training, comparison-testing).

Situation C. Differential training (two stimulus-objects of which the smaller is positive) followed by choice from an array presented together and arranged in mixed order (comparison-training, non-comparison testing).

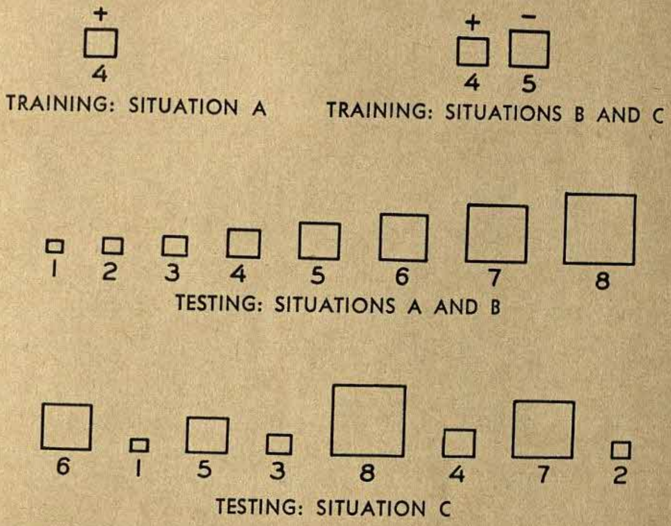

FIG. 1. EXPERIMENTAL SITUATIONS A, B, C, DIAGRAMMATICALLY ILLUSTRATED

There can be no relational choice in Situation A, although there may be 'generalization' to objects adjacent to the one originally presented. Choice of object first shown is an absolute response. In Situations B and C, choice of the object originally rewarded also constitutes an absolute response, while choice of the smallest stimulus-object is considered a relational response since it represents transfer from "in the smaller" (of two objects during training) to "in the smallest" (of all the objects during testing).

Subjects: (1) Normal children. Thirty *S*s, between 4.0–5.0 yr. old, were selected from a day nursery. No factor other than age was involved in their selection. The nursery, which admits only normal children, was part of a city low-income housing development, and most of the children's mothers were employed.

(2) Retarded children. Twenty Mongoloid *S*s, ranging in age from 7:00 to 18.6 yrs, with *IQ*s (Stanford-Binet, Form L) from 28 to 61, were selected from a

private school for the retarded and from a low-*IQ* class in a public school. Their *MA*s ranged from 2.6 to 6.4, with 10 of the 20 *S*s having *MA*s between 4.0 and 5.6 yr.

Equipment. The stimulus-objects were eight square wooden boxes, painted gray, with a swinging panel in front. Once the front panel was pushed in, a round hole (2.54 cm. in diameter) was revealed in the floor of each box, in which the reward, a raisin, was placed. The boxes ranged in height (and width) from 8.26 cm. (Box 1) to 34.29 cm. (Box 8), increasing in area of front surface at the constant ratio of 1:1.5. All were 7.62 cm. deep. Boxes were placed on a board in a row before *S*, who sat opposite *E*. Upright rods at the ends of the board supported opaque curtains which were drawn before each trial. All training and testing took place in individual sessions which were completed without interruption.

Training trials: (1) Single object training (Situation A). Ten normal *S*s were shown how to remove a raisin from a stimulus-box and were then presented with Box 4 alone for the number of trials to equal the average reward received by 20 normal *S*s exposed to differential training.

(2) Differential training (Situations B and C). Twenty normal *S*s were shown how to remove a raisin, and 20 Mongoloid *S*s were shown how to remove a bead from a stimulus-box and were then presented with Boxes 4 and 5, the training pair.[14] Only Box 4 was baited, and *S* continued making choices, without correction, until he reached a criterion of 9 correct out of 10 responses. The positions of the rewarded and unrewarded boxes were varied from trial to trial according to a prearranged schedule which precluded better than 50% success if *S* were to respond by single or double alternation. No verbal instructions were given in either training situation, and nothing was said to the child except an occasional 'good' for encouragement.

Testing trials: (1) Presentation of boxes in order of size (Situations A and B). Immediately following the end of training, the 10 normal *S*s trained with presentations of Box 4 alone, as well as the 10 normal and 10 Mongoloid *S*s exposed to differential training, were presented with Boxes 1 to 8 placed in order of size. Each *S* was instructed: "Look at all of them," and was then asked, "Where is the raisin or bead now?" *S* was permitted three choices the curtains being closed after each. All responses were rewarded.

(2) Presentation of boxes in mixed order (Situation C). Immediately following training, the remaining 10 normal and 10 Mongoloid *S*s, all trained differentially with Boxes 4 and 5, were presented with the eight boxes placed in the following order: 6–1–5–3–8–4–7–2. Except for the relative position of the boxes, testing proceeded exactly as with the boxes arranged in order of size. These training and testing situations are shown in Fig. 1.

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[14] A bead instead of raisins was used for the Mongoloid children. *S* would find it, give it to *E* who thanked him for it and indicated it would be hidden again. Such a reward was necessary because many of the Mongoloid children had difficulty eating, drooled considerably, and could not attend to the task while preoccupied with their gustatory experience.

RESULTS

Training. To learn to choose Box 4 when it was presented with Box 5, the 20 normal *S*s required an average of 25.0 trials (range 9-66 trials) while 20 retarded *S*s needed 38.5 trials (range 10-92). This difference is significant at <0.01 level.

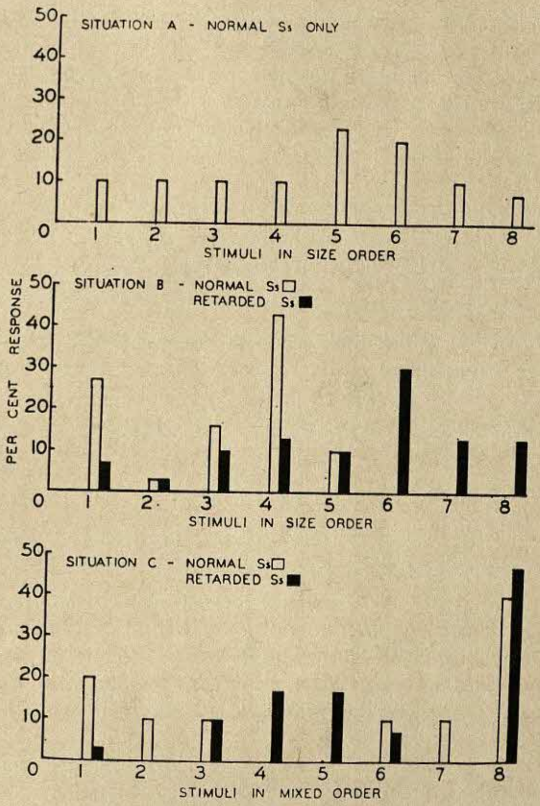

FIG. 2. PERCENTAGE CHOICES OF EACH OF SEVEN STIMULI BY 30 NORMAL AND 20 MONGOLOID *S*S IN SITUATIONS A, B, AND C.

Testing. Fig. 2 graphically summarizes the choices of the 30 normal and 20 mongoloid *S*s in three experimental situations.

Situation A. Single-stimulus training, size-order testing (10 normal Ss). Except for some slight preponderance of responses to Boxes 5 (23.3%) and 6 (20%), single-stimulus training evoked approximately equal choices of all other stimuli on the continuum.

Situation B. Differential training followed by testing with boxes in order of size (10 normal, 10 Mongoloid Ss). The normal *S*s chose the originally positive box (Box 4) most frequently, *i.e.* 43.3% of the time. Next in order of frequency was Box 1 the smallest of the series (26.7%). These *S*s did not choose any box larger than Box 5. The choices of the Mongoloid *S*s were quite different, with Box 6 being chosen most frequently (30%); Box 4, the absolute stimulus, was chosen 13.3% of the time, and the smallest box of the series, Box 1, the truly relational choice, constituted only 6.7% of their total choices. It should be noted that with the boxes placed in order of their size, Box 6 stands in the middle of the row (see Fig. 1).

Situation C. Differential training followed by testing with boxes in mixed order (10 normal and 10 Mongoloid Ss). None of the 10 normal *S*s responded to the originally positive stimulus, Box 4, in this situation. Maximal response (40%) was to the largest stimulus of the series, Box 8. As with testing in order of size, the normal *S*s did respond at times to the smallest box, Box 1 (20%). The most popular choice of the retarded children in this situation was also Box 8 (46.7%), the largest one, and they chose the originally rewarded, Box 4, no more frequently than they might have by chance (16.7%). In the mixed order presentation, Box 8 is just about in the center of the array (see Fig. 1).

DISCUSSION

Absolute responses. Only normal *Ss* were able to select, in the test-situation, the stimulus-object they had been trained to select, and they were able to do so only in Situation B, where training was differential and the test-objects were arrayed in order of size. This was the only situation of the three which permitted comparison of the positive and negative stimulus-boxes during both training and testing. Single-stimulus training did not result in maximal test-response to the positive box, but in displacement of response to larger boxes. Choices following differential training depended on whether the test -objects were presented in order of size or in mixed order, the latter, apparently, preventing identification of the object originally rewarded.

Relational responses. Relational respones to the smallest of the eight boxes were made by normal *S*s following differential training to about the same extent (27% and 20%), regardless of the manner of test-presentation. The relational choice brings up two further points of interest in the test-responses of the normal *S*s. First, they made a greater effort to respond to the stimulus-object that was originally positive than to transfer from "in the smaller" of training to "in the smallest" of testing. Whatever there was, however, of this tendency to respond relationally survived the 'scrambling' of the stimulus-objects in mixed-order presentation, while the absolute response was apparently lost in the shuffle.

The retarded *S*s responded neither relationally nor absolutely. Whether test-stimuli were arrayed in order of size or in mixed order, there was a trend to select whichever large stimulus happened to be in the middle of the array. There is remarkable similarity between the responses of our normal and retarded *S*s in Situation C, where differential training was followed by testing in mixed order. With this limitation on the possibility for comparison of objects, both our normal and retarded *S*s tended to choose the large box in the middle, a response irrelevant to the training.

One may conclude that response to an absolute stimulus depends upon the opportunity for comparison of stimuli during training. Lashley and Wade and Saldanha and Bitterman have already shown this for the relational response.[15] The results of the present study suggest that opportunity for comparison is equally important during testing if the absolute response is to be maintained. The fact that not all responses were made to the originally positive stimulus or to the smallest stimulus of the series suggests some sort of "generalization" to other "similar" stimuli. Such generalization, however, fails to conform with expectations from Spence's gradient of generalization and is, perhaps, best characterized as incomplete stimulus-differentiation.[16] "Stimulus intensity dynamism" may be operative in the selections of large stimuli in the absence of absolute or relative responses.[17]

Even with differential training and testing with stimuli in order of size, *i.e.* conditions which appear to maximize stimulus differentiation for normal *S*s, Mongoloid *S*s respond neither absolutely nor relatively. Just as it appears that absolute and relative responses arise together, both are also apparently minimized by particular training- and testing-situations, as well as by cerebral malfunction.

SUMMARY

Twenty normal *S*s (4.0 to 5.0 yr. old) were required to learn to choose the smaller of two boxes and were then presented with eight of them either in the order of size or in mixed order. When the boxes were arranged in

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[15] K. S. Lashley and Marjorie Wade, The Pavlovian theory of generalization, *Psychol. Rev.*, 53, 1946, 72-87; E. L. Saldanha and M. E. Bitterman, Relational learning in the rat, this JOURNAL, 64, 1951, 37-53.

[16] Spence, *op. cit.*

[17] C. L. Hull, Stimulus intensity dynamism and stimulus generalization, *Psychol. Rev.*, 56, 1949, 67-76.

order of size, *Ss* tended to choose either the originally positive box (absolute response) or, less frequently, the smallest box (relational response). When test-objects were in mixed order, *Ss* did not respond to the originally rewarded box; they tended to choose instead the largest or, less frequently, the smallest.

Twenty Mongoloid children (7.0 to 18.6 yr. old), in the same experimental situations, tended to choose haphazardly, showing some preference for whichever large stimulus was in the center of the array. This pattern of response was typical also of 10 normal *Ss* who were trained with presentations of a single box and then tested with eight boxes standing in order of size.

Results support the hypothesis that the absolute response depends upon opportunity for comparison of stimulus-objects during training and testing. Further, the cerebral maldevelopment of Mongoloid *Ss* was reflected not so much in the loss of relational response as in the loss of response to the originally positive object.

STUDIES OF RETENTION OF CONNECTED DISCOURSE

By NORMAN J. SLAMECKA, University of Vermont

The tradition of using nonsense-syllables as items for the study of retention is understandable, since meaningfulness and prior experience are controlled for all subjects (*S*s). The use of unconnected words as items is also widespread, since they afford control over sequential dependencies and units of ideas. The bulk of the studies on retention make use, therefore of these kinds of materials. What light, however, do such studies cast upon the retention of material that is meaningful and familiar and connected? Do the variables that govern retention of a list of nonsense-syllables also govern rote retention of ordinary speech? One is hesitant to make any statement about this after a consideration of the available evidence. References to the literature on retention reveal a relative paucity of research focusing upon connected discourse, particularly upon its rote retention.[1] It has been suggested that the study of connected materials is a domain separate from that of unconnected, and that retention should be measured in terms of idea-units or logical memory.[2] Such an approach is possible, of course, but it is equally as legitimate to ask whether the retention of a passage of prose could not be analyzed in much the same manner as is the classical serial list. Retention of a list of nonsense-syllables cannot be scored on the basis of idea-units, but any sort of verbal material can be scored for its rote retention. If it is found that the two 'domains' share some common governing variables, the generality of prediction would be extended, and the two types of stimulus-materials could be treated as one.

It is the purpose of the present paper to report some exploratory studies of the verbatim recall of prose, as a function of three classic variables that have been found to govern retention of unconnected materials. It was also desired to assess the adequacy of the scoring system devised to measure recall.

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* Received for publication August 28, 1958.

[1] S. H. Britt, Retroactive inhibition: A review of the literature, *Psychol. Bull.*, 32, 1935, 381-440; E. J. Swenson, *Retroactive Inhibition: A Review of the Literature*, 1941; J. A. McGeoch and A. L. Irion, *The Psychology of Human Learning*, 1952, 355-467.

[2] E. L. Welborn and H. B. English, Logical learning and retention: A general review of experiments with meaningful verbal materials, *Psychol. Bull.*, 34, 1937, 1-20.

In the first part of this study (Experiment I), the effect of similarity of topic of an interpolated passage to the original was investigated. Two early studies by McGeoch and McKinney are pertinent.[3] Those authors reported that when prose or poetry was the original material, the amount of forgetting attributed to interpolated nonsense-syllables was as great as that attributed to interpolated prose or poetry respectively. Strict controls over intertrial and intratrial intervals were not, however, imposed, since the material was printed on sheets which *S* studied in his own manner. Osgood's transfer and retroaction surface summarizes results of many retention studies, and makes a case for the influence of similarity. It is noteworthy that no corroborative evidence was cited for connected discourse. Osgood states that the surface "removes the need for distinguishing between meaningful and nonsense materials," whereas Miller notes, "When the original learning is for connected prose or poetry, intervening study produces small amounts of retroactive inhibition, and similarity may have little to do with it."[4] More data are obviously needed.

In the second part (Experiment II), the effect of the degree of learning of a passage upon its subsequent recall was investigated. The literature affords evidence that the retention of serial and paired-associates lists, as well as that of stanzas of poetry, is directly related to the amount of original learning.[5] It seemed desirable to ascertain whether the present method would yield similar results, as well as revealing the shape of the curve of acquisition.[6]

In the third part (Experiment III), the effect of degree of learning of an interpolated passage upon retention of the original was determined. Sufficient evidence has been reported that such retention is a negative function of the degree of interpolated learning, but again, largely for unconnected materials.[7]

EXPERIMENT I

It was hypothesized that the recall of a passage would decrease as the similarity of topic of an interpolated passage to the original increased. This is a deduction in accord with the Osgood surface prediction for serial lists, with the presumption that a passage of connected discourse can be viewed as having the properties of a serial list.

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[3] J. A. McGeoch and Fred McKinney, Retroactive inhibition in the learning of poetry, this JOURNAL, 46, 1934, 19-33; The susceptibility of prose to retroactive inhibition, this JOURNAL, 46, 1934, 429-436.

[4] C. E. Osgood, *Method and Theory in Experimental Psychology*, 1953, 533; G. A. Miller, *Language and Communication*, 1951, 220.

[5] J. A. McGeoch, The influence of degree of learning upon retroactive inhibition, this JOURNAL, 41, 1929, 252-262; J. A. McGeoch and A. L. Irion, *The Psychology of Human Learning*, 1952, 377-380. G. B. Briggs, Retroactive inhibition as a function of the degree of original and interpolated learning, *J. exp. Psychol.*, 53, 1957, 60-67.

[6] C. C. Cofer, A comparison of logical and verbatim learning of prose passages of different lengths, this JOURNAL, 54, 1941, 1-20.

[7] McGeoch, The influence of degree of interpolated learning upon retroactive inhibition, this JOURNAL, 44, 1932, 695-708; A. W. Melton and J. M. Irwin, The influence of degree of interpolated learning on retroactive inhibition and the overt transfer of specific responses, this JOURNAL, 53, 1940, 173-203; L. E. Thune and B. J. Underwood, Retroactive inhibition as a function of degree of interpolated learning, *J. exp. Psychol.*, 32, 1943, 185-200.

Method. The *Ss* for this part of the study were 184 students in psychology. The study was conducted in a classroom at the beginning of the semester. Group 1 had passages of 'high similarity' interpolated; Group 2, of 'intermediate similarity,' Group 3, of 'low similarity'; and Group 4, the control, had no passages interpolated. The *Ss* of the Control Group were asked to listen to music in the interval and to jot down the moods it evoked in them.

The *Ss* were instructed to commit to memory a passage that would be presented three times on the tape-recorder, and after the last presentation, to reproduce it in writing. The passage took 28 sec. to present; a 5-sec. interval elapsed between successive readings. Immediate recall—which was the measure of original learning (*OL*) was begun 1 sec. after the third presentation. A 3-min. recall-period was used for all groups. The *Ss* were then asked to turn over that sheet of paper, and to put the next one on top of it. They were then instructed to commit to memory another passage that would be presented three times on the tape-recorder, and after the last presentation to write it down. This was the interpolated learning (*IL*). The time elapsed from the end of original recall to the start of the interpolated passage was 15 sec. The interpolated passage had the same intra- and inter-trial intervals as the original, and the immediate recall procedure was also the same. The *Ss* were then instructed to turn their top paper over, and to write down as much as they could of the first passage that they heard. This was the delayed recall (*DR*). The time elapsed from the end of interpolated recall to the start of delayed recall was 15 sec. After a 3-min. recall-period, which was found ample for all *Ss* to finish, the papers were collected. Afterwards, no *S* claimed to have anticipated the intent of the procedure.

Materials. Five passages were obtained from various magazines and so arranged that each one consisted of one sentence, 28 words in length. Twelve judges were asked independently to place them in rank order according to their similarity of topic to the original passage. On the basis of these ordinal rankings, three passages were chosen as representing, respectively, high, intermediate, and low similarity to the original. These passages will henceforth be referred to, respectively, as Passages 1, 2, and 3.

It was then necessary to determine whether the passages to be interpolated differed in difficulty, from each other—a possible confounding variable. Each of these passages was read into a tape-recorder at the rate of 1 sec. per word, with a 5-sec. interval between each of three readings. The *Ss* for these tests were three groups of students in psychology, each group hearing and recalling only one passage. Recall-time was 3 min., starting 1 sec. after the end of the last presentation. That the passages did not differ significantly in difficulty is shown by the fact that an *F* of 1.2 was obtained.

The original and the interpolated passages that were used are as follows:

Original. American and European capital is likely to be invested in volume only in the production of oil and raw materials, which Western industries will continually and inescapably need.

Interpolated: Passage 1 (high similarity). Probably another area for investment shall open up in the foreign production of simple consumer goods, so as to avoid the tariff and currency difficulties of direct support.

Passage 2 (intermediate similarity). The smallness of their social groups encourages comparative leisure, and their need to amuse each other by their wits results in a sharp observation of the human scene.

Passage 3 (low similarity). One scarcely mentioned component in domestic cookery, cabbage exhibits unique affinity for pork products, and this combination of sharply spiced ham with boiled cabbage should amply verify that.

Scoring. The scoring system devised to measure acquisition and unaided written recall of all passages is expressed in the following rules. The score was the number of words correct, with a possible maximum of 28 for these passages. A word was counted as correct only if: (a) it actually occurred in the presented passage; and (b) its position relative to the one immediately adjacent word on both sides was correct. In determining the latter, word-omissions and extraneous intrusions were ignored, and in the case of words beginning or ending a recalled passage, only the one possible adjacent side was considered. Here are two short examples: "American and European capital is invested," would be scored 6, whereas "American capital was invested," would be scored 3, for Words 1, 4 and 6, in the correct passage. Reliability of the scoring method was assessed by calculating rank-order correlations between rankings obtained by the author and three independent scorers on a sample of 10 protocols. The obtained rhos were 0.99, 0.99, and 0.98, and it was concluded that the scoring system was sufficiently reliable to be used by others.

TABLE I

ORIGINAL LEARNING (*OL*), INTERPOLATED LEARNING (*IL*), AND DELAYED RECALL (*DR*) FOR EXPERIMENT I

Group of Ss	*N*	*OL*		*IL*		*DR*	
		Mean	*SD*	Mean	*SD*	Mean	*SD*
1	48	18.6	4.1	18.1	4.8	11.5	5.4
2	49	18.6	4.7	13.4	6.3	14.6	5.2
3	44	18.4	4.2	16.7	4.4	15.4	4.9
4	43	20.1	4.0			19.8	4.5
		$F=1.6$		$F=9.9$	$P<0.01$	$F=20.3$	$P<0.01$

Results. The relevant data for all three parts of Experiment I are given in Table I. Means of the groups on original learning did not differ significantly. Means of the interpolated learning did differ. Tukey's test showed a significance gap of 2.17 at the 5% level.[8] The means did not, however, differ in any consistent fashion with regard to the passage rankings, as might be expected if associative inhibition were a function of degree of similarity—but that is not the focus of interest in this study.

The means of delayed recall of the original passage differed significantly, in the expected direction, with a significance gap of 1.78 for the 5% level. The hypothesis that recall decreases directly as the similarity of the interpolated passage to the original increases was tenable, according to these data.

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[8] A. L. Edwards, *Statistical Methods for the Behavioral Sciences,* 1954, 331.

EXPERIMENT II

It was expected that the recall of a passage of prose would increase as the degree of its original learning was increased. Also it was expected that the curve of trial-by-trial acquisition would be negatively accelerated, as is the usual finding.

Method. The *S*s were 140 students in psychology, divided into six groups. Each group was given a different number of presentations of the original passage and three presentations of an interpolated passage. Introduction of an interpolated passage should be construed only as having the effect of controlling the experience of all *S*s during the interval between original learning (*OL*) and delayed recall (*DR*).

TABLE II

ORIGINAL LEARNING (*OL*), INTERPOLATED LEARNING (*IL*), DELAYED RECALL (*DR*) FOR EXPERIMENT II

Presentations	*N*	*OL*		*IL*		*DR*	
		Mean	*SD*	Mean	*SD*	Mean	*SD*
1	21	12.8	3.5	17.4	4.4	7.1	4.7
3	28	19.8	5.3	18.6	5.1	15.2	7.5
5	20	22.1	4.4	18.9	5.8	15.9	8.2
6	32	22.9	3.6	17.3	5.5	19.1	5.5
7	17	24.4	3.4	19.6	4.0	20.4	6.3
9	22	24.4	3.9	16.8	6.4	22.8	5.5
		$F=24.3$	$P<0.01$	$F=1.1$		$F=15.2$	$P<0.01$

No conclusions about retroactive inhibition (*RI*) should be inferred, since relative *RI* was not measured. Such a measure would require the use of a control group (*i.e.* no *IL*) at each level of *OL*. Degree of original learning was varied by the number of presentations of the passage, which were, respectively, 1, 3, 5, 6, 7, and 9. Intra- and inter-trial intervals, recall times, and time elapsed between successive steps of the study, as well as the scoring system, were the same as those of Experiment I.

The original and interpolated passages used were the 'Original' and 'Interpolated Passage 1' of Experiment I. These passages were presented on the tape-recorder at the same rate and the same sequence of steps as before; namely, original presentation, a single original recall, three interpolated presentations, a single interpolated recall, and a delayed recall of the original passage.

Results. The relevant data for all three parts of the study are shown in Table II. Mean scores among the groups on *OL* differed significantly, in the expected direction, with a significance gap of 2.01 for the 5% level. This indicates that number of presentations did vary the level of acquisition. Plotting of these acquisition means as a function of the number of presentations generated a very smooth negatively accelerated curve, consistent with the usual findings obtained upon unconnected material. Mean scores on *IL* did not differ significantly, indicating no consistent practice- or warm-up effects. The means of the delayed recall of the original passage differed significantly in the expected direction, with a significance gap of 3.19 for the 5% level. The expectation that recall increases as *OL* is increased is thus borne out for this technique as well.

EXPERIMENT III

It was expected that recall of a passage of prose would decrease as the degree of learning of an interpolated passage increased, in accord with previously cited findings based on unconnected and nonsence-materials.

Method. The *Ss* were 117 students in psychology divided into five groups. Every group was given a different number of presentations of an interpolated passage, and three presentations of the original passage. Degree of interpolated learning was varied by the number of presentations of the interpolated passage, which were, respectively, 0, 1, 3, 5, and 7. The original and interpolated passages were the same as those in Experiment II. Intra- and inter-trial intervals, recall intervals, time elapsed between successive stages of the study, method of presentation, and the scoring procedure were the same as the preceding experiments. The groups in this experiment differed in the amount of time each spent on the interpolated task. To keep the interval between initial and final recall of the original passage constant, a filler task was added at the end of the interpolated learning of the groups that had 0, 1, 3, and 5 presentations, so that it equalled the time spent by the 7 repetition group. These *Ss* were asked to listen to music in the interval, and to write down the moods it evoked in them. As in Experiments I and II, single recalls were taken at the end of the presentations of the original and of the interpolated passages.

TABLE III

ORIGINAL LEARNING (*OL*), INTERPOLATED LEARNING (*IL*), AND DELAYED RECALL (*DR*) FOR EXPERIMENT III

Presentations	*N*	*OL*		*IL*		*DR*	
		Mean	*SD*	Mean	*SD*	Mean	*SD*
0	23	18.4	5.2			16.8	4.3
1	20	17.2	5.9	10.8	5.3	14.2	6.7
3	26	18.7	4.3	19.5	3.9	13.8	5.7
5	26	18.8	4.1	19.8	5.8	13.0	5.3
7	22	18.7	3.9	21.6	3.3	11.4	7.6
		$F=0.4$		$F=21.3$	$P<0.01$	$F=2.3$	

Results. Data for this study appear in Table III. Mean scores among the groups on *OL* did not differ significantly, clustering quite closely within a narrow range. Mean scores on *IL* did differ significantly in the expected direction, with a significance gap of 2.35 for the 5% level. The means of the delayed recall (*DR*) of the original passage exhibited the appropriate directionality, but the analysis of variance produced a borderline F of 2.3 which falls just short of that required for the 5% level significance. These data were also analyzed by means of the extension of the median test and produced X^2 of 9.8, which at 4 *df.* was significant beyond the 5% level in the expected direction.[9] On this basis, the null hypothesis that the medians came from a common population, is rejected. The experimental hypothesis that recall would decrease with increase in the interpolated learning, is given some credence.

DISCUSSION

The results of the present studies indicate that the verbatim recall of a passage of connected discourse is a negative function of the degree of similarity the topic of the original passage bears to the interpolated topic. The deduction from the Osgood surface is given support, at least insofar as points along a continuum of similarity of topic are concerned. It has also been indicated that the recall of a passage is a positive function of its degree of original learning, and that its acquisition generates a negatively accelerated curve. Finally, it is also probable that the amount of forgetting of the original passage is directly related to the degree of interpolated learning.

These findings are not conclusive, but they are suggestive in indicating the feasibility of treating a prose passage much as a serial list, since they parallel the effects of variables that govern retention of discrete-item lists. A more extensive approach is next called for, making use of other passages and *Ss* used not in groups but individually that trial-by-trial measures and intrusion-counts can be taken. If such work upholds the trends of the present results, greater confidence about generalizing the results regarding rote retention to connected discourse would be justified. Such systematic comparisons should be carried further to embrace other classic variables that have been found to govern unconnected items, such as length of list, and spacing of trials.

The development of an adequate scoring system is an important task in realizing these goals. Attempts have been made in the past to score on the basis of idea-units and these have led to methodological difficulties, especially those of reliability.[10] The present method of scoring for rote retention shows some promise of being a useful tool, by virtue of the regular relationships that it has suggested in the data.

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[9] Sidney Siegel, *Nonparametric Statistics for the Behavioral Sciences*, 1956, 179-184.

[10] E. E. Levitt, A methodological study of the preparation of connected verbal stimuli for quantitative memory experiments, *J. exp. Psychol.*, 52, 1956, 33-38.

SUMMARY

Three experiments upon the verbatim recall of connected discourse were reported. Experiment I indicated that recall is a negative function of the degree of similarity of topic that the interpolated passage bears to the original. Experiment II showed that the susceptibility to forgetting is inversely related to the degree of original learning, and that the curve of acquisition is negatively accelerated. Experiment III showed that forgetting is directly related to the degree of interpolated learning. The value of the scoring system used, and the feasibility of analyzing prose in the manner of a serial list were discussed.

MIRROR-TRACING: HANDEDNESS, SCORING, AND SET

By JOHN I. JOHNSON, JR., Marquette University, and
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The results of previous studies on the relative proficiency of the dominant and non-dominant hands in mirror-tracing are ambiguous.[1] This is believed to be due to at least two factors: ignorance of the relationship between the two measures of proficiency—speed and accuracy; and failure to measure and to take account of the effects of practice.[2]

The present experiment was undertaken to determine: (1) the relative proficiency of the preferred (dominant) and non-preferred (non-dominant) hands; (2) the relationship of speed to accuracy; (3) the relative proficiency of right- and left-handed subjects (*S*s); and (4) the effects of direction of tracing. These effects were so measured that the effects of practice could be determined.

METHOD: *Subjects.* Sixty students of psychology (26 women and 34 men) served as *S*s. Of these, 40 wrote with the right hand and 20 with the left. The median age of the *S*s was 19 yr.

Materials. The traditional mirror-tracing apparatus was used: a vertical mirror on a horizontal base, with a screen so mounted at the front that *S*, when seated before the apparatus, could see the reflection of the base, but not the base itself. The figure used for tracing was the radially symmetrical 6-pointed star used by Simon,[3] consisting of 12 encircled black dots 1¼ in. apart and connected by a broken line with two arrows printed along the side of the star to indicate the direction in which to trace. The printed stars were centered on sheets of paper 8 × 9½ in., the same size as the mirror base. There was one star on each sheet.

Procedure. Upon arrival in the experimental room, *S* was asked whether he was right- or left-handed, and was told to write his name on the first tracing form. In all cases, *S* wrote with the hand he had indicated as the preferred. This procedure was deemed sufficient to determine handedness for the purposes of this experiment since the habit-structure providing transfer effects would be a result of practice in using a pencil with a particular hand as much as from any other activity.

Instructions. Every *S* was instructed as follows:

> You will trace eight of these stars as I place them before the mirror. The way you do that is to follow the dotted line [pointing] in the direction the arrows point. The object is to connect each of the black dots with the one after it till you have completed the star.

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* Received for publication July 28, 1958.

[1] C. W. Simon, Proactive inhibition as an effect of handedness in mirror-drawing, *J. exp. Psychol.*, 38, 1948, 697-708; Sherman Ross, Handedness and mirror-drawing, this JOURNAL, 64, 1951, 103-105.

[2] Simon, The effect of handedness in mirror-drawing, this JOURNAL, 1951, 64, 428-431.

[3] Simon, *op. cit.*, *J. exp. Psychol.*, 38, 1948, 697-708.

> You will find it more difficult than you expect. Do not worry if you get stuck, or have trouble. Once you start, work steadily and keep your pencil on the paper at all times, so that wherever your hand moves it will leave a mark.
>
> If you will pick up the pencil in your right (or left) hand, and close your eyes, I will put the sheet in and put your pencil on the starting point. When I say 'go' open your eyes and start drawing. You may use your other hand to steady the paper.

The star was placed in the apparatus with one point directed toward and closest to the mirror. This point was used as the starting point.

Note that no instructions were given regarding speed or accuracy, to avoid influencing their interrelationships. The time to complete each star was recorded. Errors were measured by the method suggested by Scheidemann, using a plastic ruler as a "cross-sectioned scale for scoring both extent and duration of tracing errors by means of uniform units."[4]

TABLE I

TRIAL-PATTERNS OF THE DIFFERENT GROUPS OF *S*s

R=right hand; L=left hand; CL=traced clockwise, CO=traced counterclockwise.

Trial	Right-handed *S*s*				Left-handed *S*†			
	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6	Group 7	Group 8
1	R, CL	R, CO	L, CL	L, CO	R, CL	R, CO	L, CL	L, CO
2	R, CO	R, CL	L, CO	L, CL	R, CO	R, CL	L, CO	L, CL
3	L, CL	L, CO	R, CL	R, CO	L, CL	L, CO	R, CL	R, CO
4	L, CO	L, CL	R, CO	R, CL	L, CO	L, CL	R, CO	R, CL
5	R, CL	R, CO	L, CL	L, CO	R, CL	R, CO	L, CL	L, CO
6	R, CO	R, CL	L, CO	L, CL	R, CO	R, CL	L, CO	L, CL
7	L, CL	L, CO	R, CL	R, CO	L, CL	L, CO	R, CL	R, CO
8	L, CO	L, CL	R, CO	R, CL	L, CO	L, CL	R, CO	R, CL

* The *N* of every group was 10 *S*s. † The *N* of every group was 5 *S*s.

Every *S* performed two trials, one in each direction, with one hand and then repeated the performance with the other hand. This procedure was repeated twice; thus there were eight tracings from every *S*. Direction was alternated after every trial and hand after every other trial. The hand- and direction-orders were counterbalanced, along with the *S*s' handedness, as shown in Table I.

Snedecor's method of unweighted means was applied to the 2 × 2 × 2 factorial design of this arrangement.[5] The variables analyzed were: (1) the hand used (preferred vs. non-preferred); (2) handedness (right-handed vs. left-handed *S*s); and (3) the direction of tracing (clockwise vs. counter-clockwise). To determine the influence of the effects of practice, a separate analysis of variance was performed for each trial using the time-data; and for each trial using the error-data. Both time- and error-data were transformed to log values to normalize the distributions.

Results. The analyses of the time- and error-scores are summarized in Table II.[6]

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[4] N. V. Scheidemann, A ruler for scoring mirror-tracing errors, this JOURNAL, 63, 1950, 445-446.

[5] G. W. Snedecor, *Statistical Methods*, 1946, 287.

[6] A complete table of mean squares and *F*-ratios is available from the authors.

The effect of the hand used, when measured by time, is illustrated in Fig. 1. As Table I shows, this is the only effect that is significant throughout all trials. The superiority of the preferred and non-preferred hands was reversed every other trial, and this reversal occurred each time the *Ss* changed hands. Thus those *Ss* who started with the non-preferred hand took consistently less time on all trials, using either hand, than did those who began with the preferred hand.

When measured by errors, the pattern (see Fig. 1) is the same, but it is inverted. Again the relative proficiency of the hands was reversed every other trial; but those *Ss* who started with the preferred hand made consistently fewer errors than did those who started with the non-preferred hand.

TABLE II

Summary of Analyses of Main Variables

Values of *F* are entered for every effect found to be significant: $F=4.03$ significant at the 5% level; $F=7.17$ significant at the 1% level. The subscripts, (p = preferred hand; n = non-preferred hand; r = right-handed; l = left-handed; cl = clockwise; co = counterclockwise) denotes the lesser mean log total, which indicates the more proficient performance.

	Variable	Trial 1	2	3	4	5	6	7	8
Time scores	(1) preferred vs. non-preferred hand	53.3_n	59.4_n	21.1_p	20.3_p	47.7_n	53.4_n	33.6_p	22.1_p
	(2) right-handed vs. left-handed	—	—	—	4.5_l	—	7.6_l	—	—
	(3) clockwise vs. counterclockwise tracings	—	—	—	—	—	—	—	—
Error scores	(1) preferred vs. non-preferred hand	—	11.7_p	—	7.2_n	12.5_p	23.0_p	8.0_n	6.6_n
	(2) right-handed vs. left-handed	10.1_r	—	5.1_l	—	—	—	—	—
	(3) clockwise vs. counterclockwise tracings	—	—	—	—	7.7_{co}	6.4_{cl}	—	7.2_{cl}

Table III indicates the effects of handedness. Left-handed *Ss* took consistently less time in all of the trials than right-handed *Ss*. Left-handed *Ss* made more errors on Trial 1 than the right-handed *Ss*, about the same number as the right-handed on Trial 2, fewer errors than the right-handed on Trial 3, and about the same number again as the right-handed on Trial 4. This pattern was repeated on Trials 5–8, but without significant differences.

The effect of direction, also presented in Table III, is irregular and insignificant when measured by time. When measured by errors, the superior performance again alternates, according to the *Ss* alternation of direction, on each trial. In other words, a greater number of errors was made, on every trial, by those *Ss* who had traced in a clockwise direction on the first trial.

Since the analysis of the effects of the hand used indicate that two different populations are involved, independent correlations of time with errors were computed for each trial for those *Ss* who started with the non-preferred hand, and for those who started with the preferred hand. The coefficients of correlation are presented in Table IV.

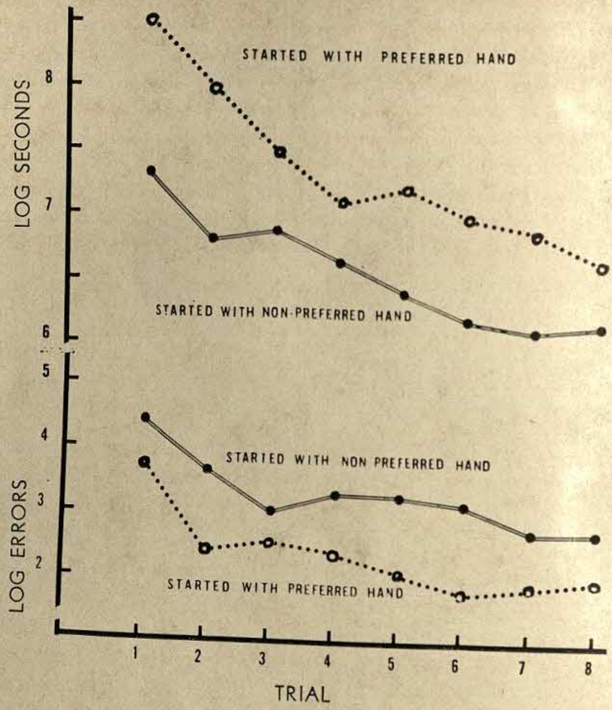

FIG. 1. PERFORMANCE AS A FUNCTION OF HAND USED IN TRACING
The time-scores are the mean log seconds for each group; the error-scores are the mean log errors for each group.

TABLE III

EFFECT OF HANDEDNESS AND DIRECTION OF TRACING ON TIME- AND ERROR-SCORES
(RH=right-handed; LH=left-handed; CL=clockwise; CO=counterclockwies)

Trial	Handedness				Direction			
	Time*		errors†		time*		errors†	
	RH	LH	RH	LH	CL	CO	CL	CO
1	7.96	7.89	3.36	4.44	7.98	7.87	4.21	3.59
2	7.54	7.25	3.06	3.05	7.31	7.47	2.84	3.27
3	7.25	7.15	3.27	2.46	7.19	7.20	3.20	2.53
4	7.01	6.79	2.99	2.81	6.92	6.88	2.63	3.16
5	6.88	6.67	2.49	2.94	6.85	6.70	3.20	2.23
6	6.75	6.44	2.60	2.47	6.51	6.68	2.15	2.92
7	6.60	6.56	2.70	2.15	6.19	6.67	2.60	2.25
8	6.48	6.38	2.43	2.47	6.37	6.39	2.03	2.88

* Mean log seconds for each group.
† Mean log errors for each group.

Discussion. Proactive inhibition or negative transfer, if it is to be effective at all, can be expected to display its influence on the first trial of the series in this experiment. The results indicate that if time is the criterion of proficiency, then negative transfer of previously learned eye-hand relationships may be said to be responsible for the poorer performance of the preferred hand. If, however, accuracy is the criterion, then there is no such transfer effect.

The data show that, no matter which hand is being used at a particular time, starting with the preferred hand results in more time being taken throughout the eight trials. Apparently the first trial serves to establish the base line against which further performance is made. *S*, confronted with a new task, has no criterion of performance and uses the first trial to establish one. Both error- and time-scores support this hypothesis.

TABLE IV

CORRELATION BETWEEN TIME- AND ERROR-SCORES

	Started with			
Trial	preferred hand		non-preferred hand	
	hand used	*r*	hand used	*r*
1	preferred	.310	non-preferred	.025
2	preferred	.489†	non-preferred	−.054
3	non-preferred	−.038	preferred	−.073
4	non-preferred	−.123	preferred	−.367*
5	preferred	.115	non-preferred	−.675†
6	preferred	−.299	non-preferred	−.620†
7	non-preferred	−.295	preferred	−.450*
8	non-preferred	−.593†	preferred	−.757†

* Significant at 5% level. † Significant at 1% level.

Further evidence of the importance of the first trial is seen in the effect of the direction traced. Tracing clockwise on the first trial resulted in a greater number of errors made on all subsequent trials—regardless of which hand was being used, or even which direction was being traced, on any particular trial.

The correlation-patterns may be the most revealing indicator of what is going on in the experimental situation. As practice proceeds, the increasing negative correlations suggest that speed and accuracy are progressing at each other's expense, supporting the findings of Starch and of Snoddy.[7] Snoddy explains that *S* chooses either speed or accuracy as his objective and seemingly neglects the other factor. It may be that *S*, starting with the preferred hand, is so completely blocked in speed that he turns to accuracy as the easier means of improvement, resulting in the lower error- and higher time-scores obtained.

The fact that it matters little which hand is being used at any time after the first trial further suggests that a perceptual factor, more than a motor-skill, is influencing performance. It may be that a negative correlation coefficient is evidence that *S* has made the speed-accuracy choice; and has thus, in a way, structured the situation. The correlation-pattern might then indicate that starting with the preferred hand inhibits this structuring of the situation and accounts for the delay in the appearance of the significant negative correlation until the eighth trial. Also, a switch from the preferred to the non-preferred hand tends to draw the correlation in a negative direction, while changing from the non-preferred to the preferred hand tends to send the correlation in a positive direction. This pattern might conceivably involve the structuring hypothesis as a further indication that the established habit-pattern of the preferred hand is interfering with the perceptual structuring that may be producing the negative correlation. This extended conjecturing could be tested by investigation of the time-error correlation under induced task-orientation.

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[7] Daniel Starch, A demonstration of the trial and error method of learning, *Psychol. Bull.*, 7, 1910, 20-23; G. S. Snoddy, An experimental analysis of a case of trial and error learning in the human subject, *Psychol. Monogr.*, 28, 1920, 1-26.

Summary. Sixty students in psychology (40 right-handed and 20 left-handed *Ss*) mirror-traced a 6-pointed star. Every *S* made eight tracings, alternating direction after every tracing and hands after every two tracings. *Ss* who started with the preferred hand took significantly more time to complete the eight tracings than did those who started with the non-preferred hand, no matter which hand was used on the later trials. *Ss* starting with the preferred hand made fewer errors, however, than did those who started with the non-preferred hand. Time and errors tended to become negatively correlated as practice progressed. This effect occurred sooner for the *Ss* who started with the non-preferred hand. The explanation appears to be that a directive perceptual set is induced by the experiences during the first trial, possibly involving proactive inhibition of habitual coördination of eyes and preferred hand.

A REVISION OF HULL'S TABLE OF ASSOCIATIVE VALUES FOR 320 SELECTED NONSENSE-SYLLABLES

By E. PHILIP TRAPP and DONALD H. KAUSLER, *University of Arkansas*

Hull, in 1933, calibrated the meaningfulness (as measured by associative values) of 320 nonsense-syllables[1] extracted from Glaze's list on the basis of their relative ease of pronunciation.[2] These calibrations have virtually become the standard reference for controlling intra-task competition and difficulty in the host of verbal learning studies using nonsense-syllables as the learning-material. There can be little doubt, however, that many of these syllables have undergone marked changes in associative value, particularly with the popular tendency in the past two decades to use abbreviations in ordinary speech and writing. For example, the syllable *RAF* now appears more regularly than the full title "Royal Air Force"; similarly, *VIP* has become a common colloquial expression for the phrase "very important person," etc. The government, the army, and the press have contributed to and popularized many of these new abbreviations. The effect has tended to hasten the obsoleteness of Hull's table of associative values.

In considering the revision, the authors tried to meet two serious limitations of the Hull study. The first one involved the sample-size. Using an individual approach to the gathering of the data, Hull was able to use but 20 *Ss*. We devised a group method which made it relatively easy to increase the sample-size to over 350 *Ss*. Secondly, Hull employed only men as *Ss*, so the use of his table is, in the strictist sense, limited to men. We therefore used both men and women in our sample. Since it would be expected that associative values would differ between the sexes because of differential habit-patterns, we made separate calibrations for the men and women.

Subjects. The *Ss* consisted of 353 students, 259 men and 94 women, students in general psychology at the University of Arkansas. The *Ss* were tested during regular class periods.

Procedure. The 320 nonsense-syllables in Hull's list were prepared for group administration in a booklet containing 20 pages with a set of 16 syllables arranged in random order to the page. Forty different booklets were constructed to control for page-order-and syllable-position upon a page. In other words, the booklets were so arranged that a given set of syllables appeared on a different page in each of the booklets, ranging from p. 1 to p. 20. An inverted serial order was, furthermore, so constructed for each set of syllables that the top half of a given set of syllables presented to one *S* became the bottom half for the next *S*.

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[1] C. L. Hull, The meaningfulness of 320 selected nonsense syllables, this JOURNAL, 45, 1933, 730-734.

[2] J. A. Glaze, The association value of non-sense syllables, *J. genet. Psychol.*, 35, 1928, 255-267.

In an effort to simulate the usual learning situation in which *Ss* are presented the nonsense-syllables one at a time on the window of a memory-drum, a cardboard template was constructed for each booklet and designed in such a way that only one syllable was visible at a time by means of a center aperture cut out the size of a memory-drum window. The template was manually manipulated by *E*.

Instructions. The instructions, patterned after those of Hull, were inserted in the front of each booklet so the *Ss* could see them as *E* read them aloud. They were as follows:

> This booklet contains a list of 3-letter syllables, which are numbered for your convenience. You will also notice a flat piece of cardboard which has a small section or window cut out in the center. This cardboard will serve as a guide. When you begin the task, please place this cardboard guide over the first page so that the window will show the first syllable and its number. You are to look at this syllable for just a *second or two,* and if it happens to make you think of anything in particular, write it down as briefly as possible on the answer sheet on the blank space beside number '1.' Then move the cardboard guide to the second syllable, and so on down the list. There are 16 syllables to a page and 20 pages in all.
>
> Please do not *try,* particularly, to think of associations for the various syllables, but if you do think of one, be *sure* to write it down. There is no time limit, but try not to spend more than a few seconds on each syllable.

The *Ss* averaged approximately 45 min. to complete the task.

Results. Table I presents the associative values for each of the 320 nonsense-syllables with separate entries for men and women, and for their combination. Hull's calibrations are also included for comparative purposes.

The method of obtaining an associative value consisted of recording the number of times a nonsense-syllable elicited a response and dividing this number by the total number of *Ss* in the sample. Hence, as similar to Hull, the associative values were computed in percentages.

An inspection of Table I reveals several facts of considerable interest. First, it does clearly indicate a marked sex-difference. In fact, the women report higher associative values on 306 of the 320 nonsense-syllables employed—about 96% of them. While the women tended consistently to exhibit higher associative values, the amount of difference on any given nonsense-syllable rarely exceeded 20 percentile points. For 303 syllables (95% of the total), the difference obtained fell under 20 points. On just one syllable did the difference between the sexes exceed 30 percentile points. This suggests that a rank-order of nonsense-syllables in terms of associative values would be fairly comparable for the two groups.

Another interesting observation pertains to the comparison of our sample with Hull's group. Since Hull had gathered data from men only, we restricted the comparison to the men. An inspection of Table I readily reveals that the *Ss* in our sample reflected considerably higher associative values. Specifically, our *Ss* showed higher values on 227 syllables and Hull's *Ss* were higher on 83 syllables. No differences were obtained on 10 syllables. A further breakdown on the degree of differences revealed that of the 227 syllables on which our *Ss* recorded higher values, there were between 20 and 30 percentile points' difference on 15 syllables; between 30 and 40 points on 5 syllables; and between 50 and 70 percentile points on 2 syllables. The breakdown on the 83 syllables on which Hull's *Ss* recorded higher associative values disclosed differences between 20 and 30 points on 9

TABLE I

ASSOCIATIVE VALUES FOR 320 SELECTED NONSENSE-SYLLABLES

	Associative value in percentage			
Syllables	sexes combined	women	men	Hull
BAF	46	62*	40	62
BAP	55	60	53	33
BAV	27	29	27	48
BAW	54	69	49	57
BEJ	18	24	15	5
BEP	49	61	45	10
BEV	66	85*	59	50
BEW	25	30	24	20
BIP	27	34	24	18
BIV	25	31	24	25
BIW	10	15	9	2
BOF	21	28	18	12
BOJ	16	23	13	17
BOP	77	82	75	25
BOV	31	44	27	45
BUP	29	37	25	12
BUV	19	22	18	12
BUW	19	28	15	5
DAF	41	59*	35	47
DAJ	14	21	12	18
DAK	46	47	45	52
DAP	35	49	30	62
DAX	19	27	16	12
DEJ	16	17	15	7
DEV	54	63	51	48
DEZ	44	51	42	35
DIB	28	37	25	18
DIJ	25	34	22	7
DOB	45	46	45	23
DOY	43	50	41	18
DUJ	18	23	15	7
DUR	46	59	41	45
DUT	39	52	34	50
DUY	31	41	28	15
FAJ	16	23	14	8
FAP	27	35	24	5
FAV	43	52	39	37
FAW	43	57	37	28
FEK	20	33	15	15
FEP	17	26	14	0
FEX	39	41	39	28
FID	44	51	41	43
FIK	31	41	28	33
FIM	36	47	32	32
FIP	39	47	36	12
FOD	58	63	56	42
FOJ	16	29	11	7
FOS	51	64	46	43
FOT	44	54	40	30
FOV	15	27	11	5
FOW	37	49	33	38
FUB	33	43	30	12
FUP	15	27	11	2
HAB	48	52	47	62
HAJ	19	30	15	0
HAN	66	79	61	43
HAP	69	78	66	73
HEB	43	47	42	33
HEF	32	41	29	40
HIB	23	32	20	28
HIF	17	30	13	13
HIJ	22	33	18	3
HUJ	17	24	14	0
HUV	14	24	10	12
HUX	31	40	28	57
JAT	32	36	31	12
JEB	33	29	35	8
JEK	30	41	26	20
JEX	29	39	26	8
JEZ	38	44	63	25
JID	15	19	13	3
JIH	14	20	12	3
JIK	17	21	15	15
JIS	26	41	21	12
JIX	35	38	34	25
JIY	14	17	12	7
JOD	35	40	34	15
JOF	21	28	18	27
JOM	25	40	20	17
JOP	53	65	49	20
JUB	29	38	25	22
JUF	12	17	10	12
JUP	27	55	42	25
JUR	52	60*	37	62
KEB	12	15	11	12
EEF	20	23	19	2
KEJ	14	20	11	0
KEM	38	34	39	23
KES	31	39	29	20
KET	40	49	37	52
KEX	29	41	24	7
KIB	21	30	17	22
KIF	14	20	12	10
KIH	15	20	13	5
KIJ	13	21	10	12
KIP	50	57	47	48
KIR	35	40	33	17
KIV	12	18	10	18
KIY	27	37	24	22
KIZ	35	46	31	15

	Associative value in percentage					Associative value in percentage			
Syllables	sexes combined	women	men	Hull	Syllables	sexes combined	women	men	Hull
KOH	25	23	25	33	PAM	69	79	65	28
KOJ	15	22	12	5	PEB	41	57*	35	42
KOS	27	40	22	38	PEF	14	17	13	17
KUJ	7	13	5	5	PEJ	18	28	15	2
KUN	31	47*	25	13	PEM	30	39	26	28
LAJ	23	36	18	7	PIB	18	32	13	12
LAR	50	64	46	42	PIJ	27	38	23	3
LEB	20	26	18	37	PIM	35	46	31	12
LEM	57	72	52	35	PIV	22	32	19	28
LIJ	19	33	14	17	POB	24	35	20	2
LOY	55	70*	49	50	POF	39	53	33	15
LOZ	33	46	28	38	POV	42	62*	34	28
LUB	47	57	43	45	POY	33	47	38	25
LUJ	23	30	20	12	PUD	59	81*	52	48
MAF	19	19	19	0	PUM	45	59	40	52
MAV	31	37	29	27	PUV	16	30	10	17
MEB	21	27	19	17	PUY	28	32	27	12
MEF	9	17	6	20	RAF	62	61	63	37
MEJ	15	24	11	13	REB	69	69	69	48
MEP	22	27	20	8	REJ	42	56	37	45
MEV	21	21	20	3	REZ	28	39	24	37
MEZ	31	38	28	27	RIK	40	40	37	28
MIB	15	27	11	7	RIX	35	44	31	13
MIP	27	31	26	12	ROF	39	49	35	32
MIV	11	16	10	0	ROJ	26	38	22	25
MOF	25	39	20	17	RUP	32	44	28	47
MOJ	14	21	12	7	RUV	20	24	19	15
MOY	31	43	27	12	RUY	21	32	17	15
MUB	33	44	29	15	TAH	20	27	17	10
MUJ	17	29	13	5	TAJ	20	23	19	40
MUP	29	43	24	15	TAV	35	38	33	43
MUX	21	31	18	20	TAY	37	43	34	42
MUY	29	38	25	18	TAZ	27	39	22	23
NAD	35	40	33	12	TEB	24	27	23	12
NAF	34	33	21	3	TEF	20	30	16	17
NAJ	19	21	19	12	TEP	41	53	37	20
NAR	28	35	25	48	TEV	41	52	37	3
NAS	39	44	37	37	TIB	27	38	23	28
NAX	32	38	29	20	TIJ	10	13	10	7
NEF	23	31	20	27	TIV	24	33	20	5
NEJ	11	19	8	15	TOB	35	41	33	35
NEM	20	26	19	23	TOJ	28	33	26	15
NEP	31	33	30	43	TOV	27	45*	21	7
NEV	67	70	66	53	TUD	25	44*	18	15
NIJ	11	13	11	7	TUJ	14	23	11	2
NIM	28	37	25	17	TUP	30	39	27	27
NIV	20	30	17	18	TUS	39	53	34	42
NOF	23	29	20	10	VAB	20	28	17	2
NOJ	15	23	12	13	VAD	35	41	32	12
NOY	34	51*	28	47	VAH	16	17	15	10
NUP	35	45	31	30	VAM	54	53	54	30
NUV	18	29	14	7	VAK	25	35	21	5
NUY	22	32	18	13	VAP	27	35	24	52
PAB	37	50	32	28	VAR	45	53	42	47
PAF	25	32	22	15	VAW	20	30	16	3

	Associative value in percentage			
Syllables	sexes combined	women	men	Hull
VAX	45	57	41	10
VAY	25	37	21	17
FED	26	31	24	23
VEF	15	18	14	0
FEK	18	26	15	20
VEN	41	47	39	28
VEP	30	29	30	5
VES	50	65*	44	42
VIB	19	18	20	8
VIF	11	12	11	12
VIP	57	55	58	5
VOD	42	45	41	23
VOF	14	17	13	0
VOK	35	37	34	8
VOL	74	78	72	53
VOM	38	47	35	18
VUD	18	29	14	12
VUK	11	15	9	0
VUM	22	34	17	8
VUP	15	24	11	7
VUR	20	33	15	13
VUS	27	40	22	22
VUT	23	38*	17	15
WAB	22	34	17	18
WEF	16	22	14	12
WEM	33	38	31	15
WES	66	80	61	55
WIB	23	32	20	5
WIM	39	62*	30	55
WOB	35	55*	76	7
WOF	50	56	47	2
WOJ	13	20	10	5
WOL	45	55	42	35
WOM	61	70	57	48
WOX	44	39	32	20
WOY	26	36	22	13
WUB	14	27	10	17
WUK	26	36	22	17
WUM	29	25	40	22
WUP	28	29	28	8
WUT	29	41	24	18
WUX	34	46	29	18
YAD	24	33	20	15
YAF	23	31	20	12
YAN	28	33	27	27
YAR	42	52	39	33
YAS	50	63	45	40
YAT	36	46	33	40
YAX	25	34	22	15
YED	18	20	18	17
YEK	22	22	21	8
YEM	23	31	20	13
YEV	13	18	11	7
YIB	14	20	12	7
YIK	15	22	12	7
YIL	25	39	20	12
YIM	24	28	23	17
YIN	41	40	42	18
YIS	39	48	36	63
YOB	27	30	26	10
YOD	25	40	20	18
YOJ	14	20	11	0
YOL	23	32	20	8
YOM	25	33	23	32
YOS	34	45	30	33
YUF	15	23	12	2
YUK	31	44	27	8
YUN	35	50*	29	20
YUT	14	19	12	5
YUX	17	23	15	7
ZAH	12	21	9	8
ZAK	28	41	23	13
ZAL	29	38	25	7
ZAM	38	52	33	17
ZAN	38	45	36	15
ZAW	36	50	31	30
ZAX	35	47	31	22
ZEB	33	35	32	20
ZED	20	26	19	18
ZEN	25	34	22	33
ZER	22	31	19	12
ZET	36	49	31	13
ZEX	31	29	32	32
ZIB	15	21	12	3
ZID	18	30	14	7
ZIH	9	14	7	5
ZIK	22	35	17	5
ZIL	25	35	22	10
ZIR	20	27	28	15
ZIT	25	30	23	20
ZIV	21	24	19	17
ZIX	31	37	29	27
ZOD	30	39	27	30
ZOF	12	14	11	8
ZOH	10	13	9	13
ZOJ	10	19	7	0
ZOL	28	37	25	18
ZOM	53	66	48	15
ZOP	31	45	26	7
ZOR	38	49	34	18
ZOT	32	44	28	20
ZOX	36	45	32	33
ZOY	30	41	26	5
ZUD	35	47	31	3
ZUK	15	17	14	5
ZUL	19	29	15	27
ZUN	45	53	42	25
ZUR	16	22	13	20
ZUT	27	30	25	12
ZUX	27	39	22	3

* Sex difference exceeds 20 percentile points.

syllables, and between 30 and 40 points on one syllable. Over-all, 34 syllables, about 10% of the total list, reflected differences in associative value that exceeded 20 percentile points. A difference of over 10 points occurred on 126 (approximately 40%) of the syllables. While part of this relatively high difference between the two groups may be accounted for in the experimental design (group administration versus individual administration), much of the variation is probably attributable to the changes in meaningfulness of the syllables that have occurred during the interim between the studies.

VISUAL RECOGNITIVE THRESHOLDS FOLLOWING SENSORY DEPRIVATION

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Recent studies of sensory deprivation have shown that reduced stimulus-variability may appreciably modify sensory and cognitive efficiency. The McGill experiments demonstrated that exposing human *Ss* for several days to an environment, which minimized stimulus-change and restricted visual functions to the perception of diffuse light, resulted in progressive intellectual deterioration, hallucinations, and a variety of visual disturbances.[1] Similarly, studies of visual monitoring have shown that impairments in attention increase with the length of the period of vigilance.[2] Hebb has suggested that these disturbances in sensory efficiency result from the lack of stimulation of the nonspecific arousal system of the brain stem.[3] According to his formulation, reduced exteroceptive stimulation may lead to a lowering of the generalized drive which presumably energizes the normal, perceptual functions.

On the other hand, contrary results were obtained by Vernon and Hoffman who reported that periods of relatively complete sensory deprivation (up to 48 hrs., in a lightproof, soundproof, floating room) resulted in *improvements* in learning ability which correlated directly with the length of deprivation.[4] Butler obtained similar results. He found an increase in the responsiveness of monkeys to visual incentives after periods of visual deprivation varying from 2–8 hrs. He ascribed his findings to the activation of a visual exploratory drive that increased with the duration of deprivation.[5]

The divergent results obtained in this area may be due to variations in the conditions of deprivation, or to differences in the measures of behavioral efficiency employed, or to both. In the present study, a comparison is made of the effects of two different conditions of sensory deprivation upon perceptual functioning in vision. Vernon and Hoffman's findings suggest that increments in visual efficiency would result from relatively complete visual deprivation.[6] The visual exploratory drive, invoked by Butler, could account for the increments in visual functioning.[7]

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* Received for publication June 2, 1958.

[1] W. H. Bexton, Woodburn Heron, and T. H. Scott, Effects of decreased variation in the sensory environment, *Canad. J. Psychol.,* 8, 1954, 70-76. Woodburn Heron, B. K. Doane, and T. H. Scott, Visual disturbances after prolonged perceptual isolation, *ibid.,* 10, 1956, 13-18.

[2] N. H. Mackworth, Researches on the measurement of human performance, *Med. Res. Council, Special Report No. 268,* London: His Majesty's Stationery Office, 1950.

[3] D. O. Hebb, Drives and the C.N.S. (conceptual nervous system), *Psychol. Rev.,* 62, 1955, 243-254.

[4] Jack Vernon, and John Hoffman, Effects of sensory deprivation on learning rate in human beings, *Science,* 123, 1956, 1074-1075.

[5] R. A. Butler, The effect of deprivation of visual incentives on visual exploration motivation in monkeys, *J. comp. physiol. Psychol.,* 50, 1957, 177-179.

[6] Vernon and Hoffman, *op. cit.,* 1075.

[7] Butler, *op. cit.,* 177.

Decrements in visual efficiency, however, were hypothesized when partial visual deprivation, defined as a condition permitting the continuous reception of unpatterned light, was used. Such restricted sensory inputs might be expected to engender perceptual satiation, fatigue, or boredom.

To test these hypotheses, recognitive thresholds for 5-digit numbers were obtained tachistoscopically after periods of 0, 5, 15, and 30 min. of total and of partial visual deprivation. It was anticipated that this measure of perceptual efficiency, the recognitive threshold, would be sufficiently sensitive to detect the small differences in visual responsiveness that might be produced by the periods of deprivation employed.

Method: (1) Experimental design. Thirty-two adult *Ss* with normal vision were divided equally into two groups, each containing 11 men and 5 women. One group was totally deprived of visual excitation; the other, partially deprived. The 16 *Ss* in each group were so arrayed in a diagonal 4 × 4 Latin square, with 4 replications, that each *S* was tested for visual efficiency on four different days following 0, 5, 15, or 30 min. of visual deprivation. This design permitted every *S* to serve as his own control for the four periods of deprivation, while allowing for independent comparisons of the effects of the four practice-days, the four orders of presentation, and the interaction of these variables with the main effects of total and partial deprivation.

(2) Deprivation. On the initial test-day, *S* was informed that the experiment was designed to measure the effects of different periods of relaxation upon the ability to read numbers. The four testing sessions were usually conducted during a single week. No interval between sessions was greater than three days.

During the period of deprivation, *S* was seated in a comfortable armchair in an isolated room. An electric fan not only kept the air in motion but provided a constant masking sound. Auditory stimulation was diminished by fitting *S* with rubber earplugs and padded earphones. For total visual deprivation, *S* wore blacked-out rubber goggles; for partial deprivation, he wore frosted goggles which permitted the perception of diffuse, formless light. A portable desk lamp, located on *E*'s side of a one-way screen, at a distance of 3 ft. from *S*'s head, provided diffuse light-stimulation from two 15-w. clear light neon lamps. All the *Ss* were instructed to move as little as possible and to keep their eyes open during the period of 'relaxation.' Overhead lighting was present during both the deprivation and testing.

(3) Testing visual efficiency. Removal of the goggles, plugs, and earphones, which immediately followed the periods of deprivation, provided several minutes for peripheral adaptation to normal light. Every *S*'s visual recognitive threshold for a list of 13 five-digit numbers was then determined on a standard Harvard tachistoscope. To minimize the effect of practice, a different list of numbers was used on each of the four test-days. Each of the four lists of numbers was constructed by selecting 13 5-digit numbers from a table of random numbers, limited only by the requirement that no digit be repeated within any 5-digit number. To control for differences in difficulty, each of the four lists was presented an equal number of times at each of the deprivation-periods and practice-days.

Recognitive thresholds were obtained by initially presenting each 5-digit number at 10 m. sec., and increasing the tachistoscopic exposure by 10 m. sec. on successive presentations until the number was correctly identified. A cross located in the center of the lighted preëxposure field served as the fixation-point. The measures of visual efficiency employed were (1) the median of the 13 recognitive thresholds obtained at each session and (2) the number of correct recognitions below 60 m. sec. The use of medians rather than means reduced the influence of occasional extreme scores resulting from fixation of response sets.

Results. The major results of the experiment are presented in Tables I and II, which show the effects of the two types of deprivation, the four periods of deprivation, and the four days of practice on both measures of visual efficiency. The values appearing in each cell of the tables represent mean recognitive scores obtained from four different *S*s, while the values in the marginal totals are mean scores obtained from the 16 different *S*s in each group. Since no appreciable differences resulted from the four different lists of numbers employed, these data have been combined in the comparisons shown.

TABLE I

Mean of Median Recognitive Thresholds in 10 m. sec.

Day	Minutes of total deprivation					Minutes of partial deprivation				
	0	5	15	30	Mean	0	5	15	30	Mean
1	7.00	5.50	6.75	3.50	5.69	6.75	5.25	5.75	6.25	6.00
2	3.25	4.75	3.75	4.75	4.13	6.75	4.75	6.00	5.50	5.75
3	5.00	3.00	5.00	5.00	4.50	4.50	5.25	5.25	7.50	5.63
4	4.75	3.50	3.25	5.25	4.19	5.25	4.25	5.00	5.00	4.88
All	5.00	4.19	4.69	4.63		5.81	4.88	5.50	6.06	

TABLE II

Mean Number of Correct Recognitions Below 60 m. sec.

Day	Minutes of total deprivation					Minutes of partial deprivation				
	0	5	15	30	Mean	0	5	15	30	Mean
1	4.50	6.50	6.00	9.25	6.56	4.00	7.25	6.50	6.25	6.00
2	11.00	8.75	10.25	8.50	9.63	6.00	7.75	6.00	8.00	6.94
3	8.25	12.00	7.00	7.75	8.75	9.50	6.75	7.50	5.25	7.25
4	9.00	9.75	12.00	7.50	9.56	7.75	8.75	9.25	8.25	8.50
All	8.19	9.25	8.81	8.25		6.81	7.63	7.31	6.94	

Inspection of the marginal values for days in Tables I and II for each type of deprivation reveals improvement in visual efficiency with successive days of practice. Duration of deprivation shows improvement with the 5-min. period, but with the 15- and 30-min. periods the results are not appreciably different from the 0-min. period. Separate analyses of variance of these 4 x 4 tables conducted for each of the deprivation-groups showed that differences in number of correct recognitions attributable to practice were significant at the 1% level in the total deprivation-group and at the 5% level in the partial deprivation-group. Duration of deprivation and order of presentation did not result in statistically significant differences in either group. Essentially similar results were obtained for median recognitive thresholds.

To test the hypothesis that total deprivation would enhance and partial deprivation would diminish visual efficiency, difference-scores were computed for every *S* between his base level of performance at 0-min. deprivation and at each of the other deprivation-periods. Analysis of variance of these scores showed no significant difference between total and partial deprivation for any of the deprivation-periods. When the data for the two groups were combined, however, 5 min. of visual deprivation resulted in a significant improvement in visual efficiency at the 5% level. Additional analyses, including scores at 0 min., showed that the interaction of type of deprivation with either duration of deprivation or days of practice was not statistically significant.

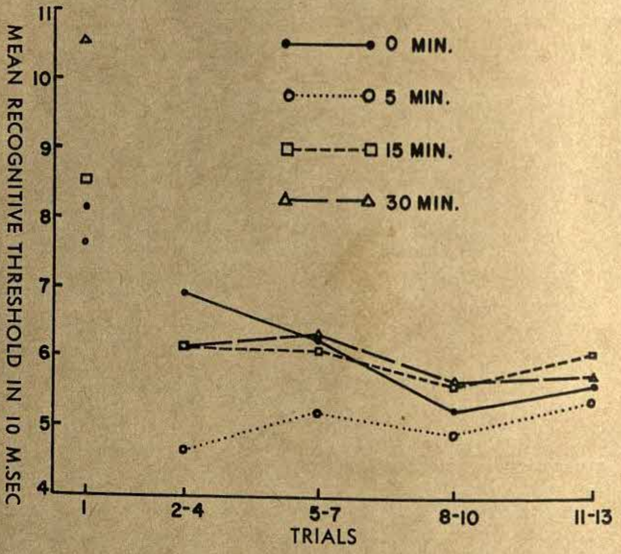

FIG. 1. RECOGNITIVE THRESHOLDS AT THE VARIOUS TEST-TRIALS UNDER THE FOUR CONDITIONS OF DEPRIVATION

In view of the possibility that the effects of deprivation may have dissipated within each test-session, performance-curves of mean recognitive thresholds obtained during the 13 trials were compared for all subgroups. Since type of deprivation did not differentially affect these curves, the data for the two groups have been combined in Fig. 1 to show the effects of the four periods of deprivation on the performance-curves for a given session. With the exception of Trial 1, in which accommodation- and orientation-effects have elevated visual thresholds, there appears to be little change in the recognitive thresholds as a result of practice during a given day. The general flatness of the curves indicates that visual efficiency during a single session was not affected by peripheral adaptation or warm-up factors. A 5-min. period of visual deprivation shows consistently lower recognitive thresholds at all points on the curve than any of the other periods, while the 15- and 30-min. periods approximate the curve for the zero level.

Discussion. The results of the present study indicate that periods of visual deprivation up to 30 min. produce no appreciable decrements in visual thresholds of recognition. Contrary to the hypothesis that the two types of deprivation would differentially affect visual efficiency, total deprivation of visual stimulation proved to be no different from partial deprivation in its perceptual effects. Both conditions produced an improvement in visual efficiency following 5 min. of deprivation, but longer deprivation periods resulted in a return to normal efficiency. The sensitivity of the tachistoscopic measure of perceptual efficiency was reflected by significant improvements with successive days of practice.

These findings indicate that increasing temporal adaptation to a state of reduced visual stimulation does not produce linear decreases in visual responsiveness. If the visual impairments reported in previous studies of prolonged visual deprivation were attributable simply to sustained lack of visual arousal, some evidence of reduced visual efficiency might have been expected within the deprivation-periods of the present study. It seems more likely that such visual disturbances are a complex derivative of a prolonged isolation-experience in which the reduced variation of all exteroceptive stimulation and the stress-effects of social isolation interact to produce the phenomena observed.

The alternative hypothesis that visual deprivation serves to motivate visual responsiveness was partially supported by the present findings. Five minutes of deprivation appears to have produced either a cognitive expectation or deficit state which enhanced visual efficiency. The data of the present experiment do not permit an unequivocal conclusion as to whether the motivating effects of visual deprivation upon visual efficiency reach a limit around 5 min., or whether a rest-interval of this duration is ideally suited for producing a performance-enhancing set in human *S*s. The results do indicate that these effects appear to dissipate with longer periods of deprivation. Observations and reports of *S*s during the longer periods of deprivation suggest that the decline to normal efficiency may have been produced by increased boredom and emotional interference accompanying lengthened isolation.

MANIFEST ANXIETY, PALMAR PERSPIRATION-INDEX, AND STYLUS MAZE-LEARNING

By F. J. McGUIGAN, ALLEN D. CALVIN, and ELIZABETH C. RICHARDSON, Hollins College

According to the S–R theory of Spence and his associates, total effective drive (*D*) is one of the important variables that determines the strength of response. This theoretical formulation has received support from a number of experiments which demonstrated that variation in drive-level, as measured by the Taylor Anxiety Scale (*TAS*), affects performance.[1] One such study conducted by Farber and Spence on stylus maze-learning,[2] was not confirmed by the findings of Axelrod, Cowen, and Heilizer.[3]

The *D* in the S–R theory is a summation of all of the drives that are operating in a given situation. In a recent investigation designed to determine the relationship between the *TAS* and the Palmar Perspiration Index (*PPI*), a physiological measure of tension developed by Mowrer and his associates,[4] a zero-order correlation was found. On the basis of this finding and other experimental work, it was hypothesized that the *TAS* and *PPI* measure two different facets of behavior. First, where anxiety is of a temporary 'ego-involved' situational nature, the appropriate measure of *D* is the *PPI*; and secondly, where anxiety is a stable, permanent characteristic of personality, the appropriate measure of *D* is the *TAS*.

The present experiment was conducted for several purposes: (1) to resolve the discrepancy between the Farber-Spence findings and those of Axelrod *et al.*; (2) to test further the S–R theory by using the *PPI* as a measure of *D*; and (3) to investigate the summative aspect of the S–R theory by determining whether *S*s who have low scores on both the *TAS* and the *PPI* are superior in maze-learning to *S*s who have high scores on both of these measures of anxiety.

Subjects. The *S*s were 56 women, students from introductory courses in psychology.

Apparatus and procedure. The apparatus (a 10-choice stylus-maze) and procedure were the same as described by Farber and Spence, except that the present *S*s were blindfolded instead of being placed behind a cloth screen.[5] Immediately following the sessions devoted to learning the maze, *PPI*-measures were taken by the procedure suggested by Mowrer.[6]

Results: (1) Analysis of the TAS. According to the S–R theory, it is to be expected: (1) that *S*s with low anxiety will perform better on the complex task of

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* Received for publication August 11, 1958.

[1] J. A. Taylor, A personality test for manifest anxiety, *J. abnorm. soc. Psychol.*, 48, 1953, 285-290.

[2] I. E. Farber and K. W. Spence, Complex learning and conditioning as a function of anxiety, *J. exp. Psychol.*, 45, 1953, 120-125.

[3] H. S. Axelrod, E. L. Cowen, and F. Heilizer. The correlates of manifest anxiety in stylus maze learning, *ibid.*, 51, 1956, 131-138.

[4] O. H. Mowrer, *Psychotherapy*, 1953, 591-640.

[5] Farber and Spence, *op. cit.*, 121.

[6] Mowrer, *op. cit.*, 559f.

learning the stylus-maze than those with high anxiety; and (2) that the greater the difficulty of the choice-points in the maze, the greater the superiority of the performance of the *Ss* with low anxiety.[7]

Table I presents the error-scores in the maze and the number of trials required to reach our criterion of learning for the 'high-anxious' group (10 *Ss* with *TAS*-scores of 28 and above) and for the 'low-anxious' group (10 *Ss* with *TAS*-scores of 10 and below). The *t*-tests between the two groups on error-scores and trials to criterion were not significant.[8]

TABLE I

MEANS AND *SD*s OF PERFORMANCE ON THE STYLUS-MAZE OF HIGH- AND LOW-ANXIETY GROUPS

	Anxiety group	Errors		Trials	
		mean	*SD*	mean	*SD*
TAS	high	16.70	11.57	9.80	5.71
	low	27.70	20.56	13.20	8.37
PPI	high	23.40	19.17	10.40	6.47
	low	19.20	10.92	10.70	4.97

TABLE II

PERCENTAGE OF TOTAL ERRORS AT EACH CHOICE-POINT FOR HIGH- AND LOW-ANXIETY GROUPS

	Anxiety group	Choice-points									
		1	2	3	4	5	6	7	8	9	10
TAS	high	8	4	10	5	15	7	18	7	21	5
	low	6	6	11	10	17	8	14	8	21	5
PPI	low	6	3	9	6	20	6	16	6	26	2
	high	5	6	9	12	10	6	16	8	27	1

Table II presents performance of the high- and low-anxious *Ss* at each choice-point, using the percentage-method suggested by Axelrod *et al.* A correlation was run between level of difficulty and superiority of the low-anxious group. The resulting rho of 0.09 was not significant (as was Kendall's *Tau*[9]). An additional analysis was made (similar to that of Farber and Spence) not using percentages and in this case also the relationship was not significant.

A further analysis conducted in the Farber and Spence, and Axelrod *et al.*

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[7] For an elaboration of these predictions and a discussion of their derivation from Hullian theory, see Farber and Spence, *op. cit.*, 120.

[8] According to Axelrod *et al.*, Farber and Spence eliminated all *Ss* with scores above 7. This procedure was not followed in the present experiment, since it would have eliminated only one of our high-anxious *Ss*, with no appreciable effect on the significance-level of the *t*.

[9] According to Axelrod *et al.*, the last choice-point should have been to the right and not to the left, as reported by Farber and Spence. Our maze was constructed according to Farber and Spence's specifications, and thus we had a different order of difficulty than Farber and Spence and Axelrod *et al.* The order of difficulty of our choice-points was: 9, 5, 7, 3, 4, 6, 8, 1, 10, and 2. We assigned difficulty-levels to the choice-points on the basis of our sample.

studies was to match *Ss* from the 'high' and 'low' groups on the basis of theii total number of errors. In the present study this procedure resulted in five matched pairs whose total error-scores did not differ by more than one error. Each *S*'s errors were then divided into those that occurred at the five most difficult choice-points and those that occurred at the five least difficult points. A *t* was then computed between the high and low groups on the number of errors made on the easy choice-points. This *t* was not significant. Of necessity, then, the difference between the groups on the difficult choice-points was not significant.

Analysis of PPI. High- and low-anxiety groups were also constructed on the basis of *PPI*-scores. These groups were formed from the highest 10 *Ss* and the lowest 10 *Ss* of the total distribution. The error-scores and the number of trials to criterion are presented in Table I. The *t*-tests between the high and low groups on both criteria were not significant.

A choice-point analysis was conducted for the *PPI*-scores in the same manner as for *TAS*-scores. Table II presents the performance of the high and low *PPI Ss* at each choice-point in terms of percentages. The rho between difficulty-level and superiority of the low *PPI* group was 0.27 which, like a *Tau*, was not significant. An analysis not using percentages was also not significant. It was not possible to conduct a matched groups analysis for the *PPI* due to an insufficient number of similar total error-scores in the high and low groups.

PPI *and* TAS. The correlation between *PPI*- and *TAS*-scores was 0.06, which confirmed previous findings.[10] Since S–R theory indicates that drives summate, it would be expected that *Ss* who have high scores on both the *PPI* and *TAS* would perform less well on the maze than *Ss* who are low on these measures.

Since the *Ss* had to be run through the entire experimental procedure before appropriate *PPI*-measures could be obtained on them, it was not possible to select high- and low-anxious *Ss* on this measure prior to the experiment. This limitation resulted in a relatively restricted number of *Ss* who showed high scores on both the *PPI* and *TAS*, and *Ss* who showed low scores on both measures, since these two measures are independent of each other. As a result, 5 *Ss* fell in the top 20 of both the *PPI* and the *TAS* distributions, while 4 *Ss* fell in the bottom 20 of these two measures. The *t*-tests between these two groups on total error-scores and trials to criterion yielded values that were not significant.

Discussion. The results of this experiment do not indicate a relationship between anxiety (as measured either by the *TAS* or the *PPI*) and performance in maze-learning. If only the *TAS* is considered, these findings are in accord with those of Axelrod. The question now becomes one of why neither Axelrod nor the present experimenters were able to duplicate the findings of Farber and Spence. One possibility that always exists is that minor modifications in experimental procedures were responsible, such as the use of opaque glasses (by Axelrod) or a blindfold (by us) rather than the cloth screen used by Farber and Spence, or the slight differences in *TAS*-scores among the *Ss* of the three studies. While such differences do offer a possible explanation for the discrepancy, a more likely explanation would seem to be suggested by the differences in means and scatter of the number of errors made, and trials required to reach criterion in the three experiments (See Table III). It is apparent from Table III that our mean errors and trials to criterion and those of Axelrod are in general considerably higher than those of Farber and Spence, the only exception being that our high-anxious *Ss* made slightly fewer errors than did those of Farber and Spence. Similar relations held for standard deviations. The *t*-tests were applied to determine significance of differences, using the modified *t*-test for heterogeneity of variance where appropriate.[11] Considering the low-anxious groups first, we found that both we and Axelrod obtained significantly higher variances for errors than Farber and Spence (both $ps < 0.01$). When the means for errors of the three experiments were tested, it was found that Axelrod's *Ss* were significantly higher than those of Farber and Spence (p. < 0.01), while the difference between our mean and that of Farber and Spence fell just short of significance at the 5% level. Similar results were obtained for number of trials to reach criterion. That is, for trials to criterion, both we and Axelrod had significantly higher variances than Farber and Spence (both $ps < 0.01$); and Axelrod's mean is significantly higher than that of Farber and Spence ($p < 0.01$), while the difference between our mean and that of Farber and Spence fell just short of significance at the 5% level. None of the differences between Axelrod's *Ss* and ours was significant.

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[10] J. C. Beam, Serial learning and conditioning under real-life stress, *J. abnorm. soc. Psychol.*, 51, 1955, 543-551; A. D. Calvin, F. J. McGuigan, S. Tyrell, and M. Soyars, Manifest anxiety and the Palmer Perspiration Index, *J. consult. Psychol.*, 20, 1956, 356.

TABLE III

MEANS AND *SD*s OF PERFORMANCE ON STYLUS-MAZE OF HIGH- AND LOW-ANXIETY GROUPS (*TAS*) IN THREE STUDIES

	Anxiety group	Errors		Trials	
		mean	*SD*	mean	*SD*
Farber-Spence	high	19.15	10.61	8.28	2.99
	low	13.70	8.05	6.88	3.06
Axelrod et al.	high	34.8	16.3	13.6	7.5
	low	29.3	14.4	12.7	7.5
This Study	high	16.7	11.57	9.8	5.7
	low	27.7	20.56	13.2	8.37

Let us now compare the high-anxious groups. Here it was found that Axelrod's *Ss* had a significantly higher variance for error-scores than did those of Farber and Spence (p < 0.01), while Axelrod's mean was significantly higher than that of Farber and Spence and ours ($p < 0.01$). Considering the number of trials to criterion for the high-anxious groups, both Axelrod's *Ss* and ours had significantly higher variances than Farber and Spence's *Ss* (both *p*s <0.01). Our mean for trials to criterion was significantly higher than that of Farber and Spence ($p < 0.01$). Differences that have not been mentioned were not significant.

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[11] A. L. Edwards, *Experimental Design in Psychological Research*, 1950, 167-169.

It would thus appear that, in general, Axelrod's and our scores on both criteria were significantly higher and more variable than those obtained by Farber and Spence. These differences in performance between Axelrod's and our *Ss*, on the one hand, and those of Farber and Spence, on the other, may well be because the experimental situation produced greater stress in Axelrod's and our *Ss* than in those of Farber and Spence. In support of this explanation, it should be noted that a large number of our *Ss* made such spontaneous comments as "Now you are going to find out how dumb I am," etc. It may well be that the *Ss* used by Spence are more accustomed to participating in experiments and thus were less disturbed than the *Ss* studied by Axelrod and us. If, in fact, the *Ss* of Axelrod (who were taken from English classes) and our *Ss* experienced a large amount of stress, then the effect of variations in *D* (*TAS*) may have been obscured. Such a possibility would thus explain the lack of relationship between anxiety-level and maze-performance.[12]

This explanation might similarly explain the lack of positive results with the *PPI* or with the *PPI*- and *TAS*-scores combined. It should be noted, that while Axelrod *et al.* recognize the possibility that their *Ss* were more threatened than those of Farber and Spence, they do not consider that this factor could explain the discrepant results, for they state, "The deductions from Hullian theory would, if anything, lead us to expect even more marked differences between *MAS*-groups [*TAS*] under present experimental conditions than were reported by Farber and Spence."[13]

Farber and Spence do not agree with this interpretation, for in considering the reasons for negative results in another study in which *Ss* were highly threatened, they stated "it might be assumed that all *Ss* were highly anxious throughout the experiment . . . [and therefore] differences in manifest anxiety would be expected to have little or no effect."[14]

What appears to be called for, then is a replication of this experiment in which the amount of situational stress is systematically varied. Where situational stress is small, it would be expected that the scores in maze-learning would be similar to those obtained by Farber and Spence, and the relationship between anxiety-level and maze-performance obtained by Farber and Spence should be confirmed. On the other hand, where the amount of situational stress is great, the findings of Axelrod *et al.* and those of the present study should be confirmed.

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[12] Another possibility is suggested by L. J. Kamin and Olga Fedorchak, (The Taylor scale, hunger, and verbal learning, *Canad. J. Psychol.*, 11, 1957, 212-218). They were interested in the effect of anxiety and hunger on paired-associates learning and, while their results were negative as far as S–R theory was concerned, they did report some significantly different behavior between men and women. Sex-differences in anxiety-behavior have also been noted by Spence *et al.* and by other experimenters, hence the fact that all of our *Ss* were women may have been a factor in our results. The fact that Axelrod *et al.*, ran separate analyses for men and women and failed to support Farber and Spence's findings with either sex argues, however, against the sex-variable's being a crucial determinant in this case.

[13] Axelrod, Cowen, and Heilizer, *op. cit.*, 136.

[14] Farber and Spence, Effects of anxiety, stress and task variables on reaction time, *J. Pers.*, 25, 1956, 16.

THE EFFECT UPON INCIDENTAL LEARNING OF VARYING THE INFORMATION ABOUT THE IRRELEVANT MATERIAL

By WILMA A. WINNICK and WALTER L. WASSERMAN, Queens College

When incidental learning has been demonstrated experimentally, it has often been presumed to be due to a set to learn which has transferred from previous learning-situations in which instructions to learn have been made explicit[1] or to a set which generalizes from the specific instructions related to the relevant material.[2] Such a set to learn incidental material as well as instructed material probably varies greatly with different *Ss*. The set may be one of which *S* is aware; noticing the irrelevant material, *S* may from time to time direct his attention toward it, possibly as a diversion from the instructed task; or even naïve *Ss*, knowing that they are participating in an experiment, may be on the alert for a 'trick' and be aware of the possibility that they may be tested for knowledge of the irrelevant material. In fact, reports from *Ss* participating in incidental learning experiments indicate awareness varying from no notion at all that such learning might be tested to certainty that some such 'trick' is in the offing.

Uncontrolled variation in the strength of the set to learn incidental material might be a serious source of error in experiments on incidental learning. Jenkins, in an analysis of his *Ss'* introspective reports, showed that variations in the set to learn on the part of incidental learners will greatly affect the amount of incidental learning which takes place.[3] In his experiment, the *Ss* in the incidental learning group were told to read aloud words for other *Ss* to learn, then were themselves tested on the material. This is an experimental set-up which is probably highly conducive to the occurrence of a great deal of learning, since no other learning activity occurs simultaneously as the *S* reads out the material.

In many experiments in this field, the learning in question is incidental to the learning of other material that *S* is instructed to accomplish. Although *S* may be aware of the irrelevant material, he has been clearly directed to learn other material. Under such circumstances, can there be a variation in the amount of incidental learning produced by different sets?

Problem. The present experiment has attempted to vary the strength of the set to learn irrelevant material by varying the information provided *S* about this material. All *Ss* were instructed to learn one type of material, but they were presented with differing kinds of information about the purpose of the irrelevant material.

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* Received for publication July 7, 1958. The assistance of Lawrence Pervin and Gerald Weinberger in this study is gratefully acknowledged.

[1] G. H. Brown, Factors influencing incidental learning, *J. exp. Psychol.*, 47, 1954, 163-169.

[2] Leo Postman and V. L. Senders, Incidental learning and generality of set, *ibid.*, 36, 1946, 153-165.

[3] J. G. Jenkins, Instruction as a factor in 'incidental' learning, this JOURNAL, 45, 1933, 471-477.

In a sense, this might be equivalent to the possible hypotheses that *Ss* might themselves adopt about the purpose of the irrelevant material. For example: S_A might barely notice that this material is there, invoking no judgment about it; S_B might caution himself not to be diverted by the material; or S_C might jump to the conclusion that this material will turn out to be important and must be learned. We are interested in discovering whether these three different attitudes result in significantly different amounts of incidental learning.

We are interested, moreover, in how these attitudes about the irrelevant material affect the speed of learning the relevant material. Does S_C, for example, take time out from relevant learning to learn irrelevant material on which he thinks he might be tested? Is S_B, because he has decided that the irrelevant material has nothing to do with the experiment, then able to accomplish the learning of relevant material more efficiently? How does learning of S_A, who is without an hypothesis, compare with that of the other two *Ss*? This aspect of the experiment may be conceived as the reverse of the problem studied by Bahrick.[4] He tested the hypothesis that the amount of learning irrelevant to a set induced by the experimenter (*E*) is inversely related to the strength of the incentive used to determine that set. Our problem may be stated as raising the question whether the strength of the set to learn irrelevant material (or *S*'s incentive to learn it), if it influences such learning, will also affect the efficiency of the instructed learning.

Apparatus and materials. The materials to be learned were serially exposed in a memory-drum with a 2-sec. rate of exposure and an interval of 15 sec. between successive series. The materials consisted of 14 colored forms drawn to occupy approximately ¼ sq. in. The materials were derived from those used by Bahrick.[5] The forms were exposed by the memory-drum in the following sequence: yellow triangle, black crescent, green cross, red circle, pink crescent, black rectangle, brown square, blue circle, green diamond, red rectangle, blue triangle, yellow square, brown diamond, pink cross.

Subjects. Fifty-four *Ss* participated in the experiment, being chosen from introductory classes in psychology. All served individually and were naïve as to the purpose of the experiment.

Procedure. Three equated groups, given differential treatment with respect to instructions, were chosen on the basis of performance on a pretest. This test consisted of a series of 14 nonsense-syllables, averaging 25.5% in associative value, exposed serially in the memory-drum. This situation served also to acquaint the *Ss* with the anticipation-method of learning, which was explained in detail.

For the experiment proper, the three groups were given identical materials to learn in the memory-drum, but groups differed with respect to instructions. The *Ss* were given the following instructions:

Group A: The memory-drum will now expose a series of forms. You are to memorize the names of these forms by the anticipation-method, until you are able to anticipate correctly the names of all of the forms in the list.

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[4] H. P. Bahrick, Incidental learning under two incentive conditions, *J. exp. Psychol.*, 47, 1954, 170-172.

[5] Bahrick, *op. cit.*, 171.

Group B: The memory-drum will now expose a series of forms. You are to memorize the names of these forms by the anticipation-method, until you are able to anticipate correctly all of the forms. You will notice that the forms are of different colors. Do not let this divert you. Just concentrate on memorizing the forms.

Group C: The memory-drum will now expose a series of forms. You are to memorize the names of these forms by the anticipation-method, until you are able to anticipate correctly all of the forms. You will notice that the forms are of different colors, and you will be tested on the colors, but just attempt to memorize the names of the forms.

Immediately following the learning-series, all the *S*s were given the test of recognition for colors. They were presented with a sheet of paper containing a column of plain forms in the order learned; across the top were all of the colors. The *S*s were instructed to indicate which of the colors belonged with each form as follows:

On this chart you will see in the column on the left the 14 forms in the order in which you learned them. Now tell me which of the colors (in the row across the top) you recall as belonging with each of the forms.

Results and discussion. Table I shows, for the three equated groups, the number of trials they required to learn the instructed materials and scores on the recognitive test for incidental learning. From these data, it is obvious that the information provided the three groups had differential effects upon the amount of incidental learning taking place. The scores of all groups are above the chance score of 2 correct out of 14. Group A, which was given no information about the role of the incidental material, had the middle-most score, a mean of 7.50 colors correctly recognized. The *S*s in Group B, having been told not to be diverted by the colors, seem to have responded accordingly, with a mean of only 4.66 colors recognized. The highest score of 8.94 was achieved by Group C, the *S*s of which had been told that they would be tested on colors but should concentrate on forms. The analysis of variance performed on the recognitive data shown in Table II yielded a highly significant value. Subsequent *t*-tests showed the differences between Groups A and B and Groups B and C to be significant at the 1% level of confidence and the difference between Groups A and C to be significant at the 5% level.

TABLE I

NUMBER OF TRIALS REQUIRED BY THE DIFFERENT GROUPS TO LEARN THE NAMES OF THE FORMS AND SCORES ON THE RECOGNITIVE TEST FOR COLOR

Group	*N*	No. of trials to learn forms	Recognitive test-score
A	18	13.11	7.50
B	18	14.00	4.66
C	18	12.89	8.94

That these differences in recognitive scores are achieved without any effect upon the learning of the instructed material is shown by the learning-data of Table I. The prediction that the *S*s who achieve high degrees of incidental learning do so at the expense of the instructed learning and that, conversely, those who do little incidental learning are enabled thereby to accomplish instructed learning more efficiently is not borne out by these results. Rather, Group B, with the lowest recognitive score for the irrelevant material took the greatest number of trials to learn the instructed material. Group C, which had the highest score, took the fewest trials to learn instructed material. These results suggest the possibility that the set induced with respect to irrelevant material may generalize to the learning of relevant material, or in the opposite direction from that demonstrated by Postman and Senders;[6] that is, the group alerted to the possibility of a test for irrelevant material learned the relevant items more efficiently due to the generalizing effect of this set. This possibility was not substantiated by statistical examination of the differences in learning scores for the three groups. The analysis of variance among the learning scores for the three groups yielded an *F*-value falling short of significance at any accepted level of confidence.

TABLE II

Summary of Analysis of Variance of Learning and Recognitive Scores for the Three Equated Groups

Source of variation	Learning			Recognitive		
	df.	*MS*	*F*	*df.*	*MS*	*F*
Between groups	2	10.5		2	87.7	
Within groups	51	6.0		51	6.4	
Total	53		1.7*	53		13.7†

* Not significant. † $p < 0.01$.

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[6] Postman and Senders, *op. cit.,* 159.

PERFORMANCE-DECREMENT AT AN OBSERVER-PACED TASK

By WILLIAM E. KAPPAUF, University of Illinois, and M. CARR PAYNE, Georgia Institute of Technology

The present study of observer-paced search falls in the general area of inquiry concerned with performance at observing or watching tasks which continue for long periods. The experiment was carried out in a group situation. The *Ss* worked in test-booklets where they searched successively for different numbers for an uninterrupted period of 75 min. Each *S* set his own pace under the instruction to proceed as rapidly as possible. His rate of finding numbers probably varied with many factors, among them his searching pace and his vigilance or alertness as he scanned the page of numbers. The object of the study was to determine whether the rate of finding numbers would change significantly during long periods of research—a result of some importance for understanding performance at tasks which are paced by the worker.

Method: Test-materials. The test-booklets were made up of similarly appearing pages, each containing 90 pairs of 2-digit numbers. These number pairs were arranged in six triple-spaced columns with 15 entries in each. Every 2-digit number from 10 to 99 appeared both as the first member of some pair and as the second member of some other pair. The order of the first numbers was random and varied from page to page. *S*'s task was to search for a given first number, find it, mark it, note the second number of that pair, begin searching for it among the set of first numbers, and so on. Thus, each number which was found led to a new number for which to search. This particular task was developed for this study by Bakan while a member of the Laboratory staff and has since been used by him in a study of time-estimation.[1]

Subjects. The *Ss* were 60 men drawn from the 47th and 57th Training Squadrons at Chanute Air Force Base in the fall of 1953. They reported for testing in groups of 15.

Procedure and design. The experimental session began with a period of instruction, two supervised practice-periods of 15 min. each, and a short rest-period. The *Ss* then worked continuously in their booklets for 75 min. During this time, they worked on 10 different pages. Working time on any one page was controlled by *E* who, at prearranged intervals, instructed all the *Ss* to turn to the next page and search for the number 10. The page-turning schedule allowed 15 min. for each pair of pages.

The page-times which were used were as follows: 10 and 5 min. for the pages of the first pair, 8 and 7 min. for the pages of the second pair, 6 and 9 for the

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[1] Paul Bakan, Effect of set and work speed on time estimation, *Percept. Mot. Skills*, 5, 1955, 147-148.

third pair, 4 and 11 for the fourth, and 9 and 6 for the fifth. Time-per-page was so limited that *S*'s task would not become appreciably easier because of his growing familiarity with a given page. Further, the use of a schedule of varied times per page made it quite improbable that *S* could make valid assessments of his rate of accomplishment from page to page.

The 10 test-pages consisted of five specific pairs of pages which, as pairs, were ordered to form five types of booklet according to the sequences of a 5 × 5 Latin square. The *Ss* in each test-session were divided randomly into groups of three, and each of these groups was assigned to one of the five types of test-booklet. Thus, over the 60 *Ss* of the experiment, there were in all 12 replications of the square.

TABLE I

Analysis of Variance

	Source of variation	Sum of squares	*df.*	Mean square	*F*
Among Ss	Orders of pages	2,786.5	4	696.6	1.15
	Groups of Ss	2,404.7	3	801.6	1.32
	Orders × groups	10,508.1	12	875.7	1.45
	Ss within order-groups	24,207.4	40	605.2	
	Total between Ss	39,906.7	59		
Within Ss	15-min. search periods	1,390.6	4	347.7	6.44*
	Pairs of pages	757.7	4	189.4	3.51*
	Groups × periods	440.1	12	36.7	
	Groups × pairs of pages	468.3	12	39.0	
	Latin square residual	1,018.0	12	84.8	1.57
	Latin square heterogeneity	1,510.4	36	42.0	
	Residual within Ss	8,629.3	160	53.9	
	Total within Ss	14,214.4	240		
Total for experiment		54,121.1	299		

* $p<0.01$.

Results. Performance was scored as the number of numbers found in each 15-min. period. Examination of the score-distributions indicated that the data were suited to analysis of variance. A summary of that analysis is presented in Table I.

Only two effects were statistically significant. One of these was the effect associated with 15-min. work-periods which the experiment had been designed to explore. The other was the effect associated with pairs of pages, an effect which presumably implies a greater degree of clustering of the search numbers on some pages than on others. The values of *F* presented in the table for these effects involve the use of the mean square 'residual within *Ss*' as error-terms. Had a pooled mean square been computed, based upon the residual within *Ss*, the Latin square residual and the variation representing square heterogeneity, the *F*-values would have been almost identical with those tabled.

The nature of the performance-change over the 75-min. session was a decrement, as shown by the following values which represent the mean number of numbers found in the successive 15-min. portions of the search: 50.1; 48.1; 45.2; 45.5; and 44.2. The decrement, although not very marked, was present in the records of each group of 15 *Ss* considered separately.

Discussion. Studies of vigilance concern stimulus-detection in situations where the events to be detected occur relatively infrequently and where, from *S*'s point of view, there is uncertainty with respect to the time of their occurrence. *E* programs the sequence of events and *S* is instructed to watch or listen alertly and to respond in a stated manner to indicate his detections. Two forms of task may be recognized, differing as regards the duration of the watched-for event. Broadbent distinguished these by calling them *paced* and *unpaced* tasks.[2] Paced tasks are tasks like those of Mackworth,[3] Jenkins,[4] and Kappauf and Powe,[5] in which the events are brief and transient, and the number of successful detections is taken as the performance-measure. Unpaced tasks are tasks like those used by Broadbent,[6] and by Loeb, Jeantheau and Weaver,[7] in which the stimulus-event is not transient but persists until *S* responds to it, and where response-latency is the measure of performance. Results of work with these two types of tasks have established two relations with reasonable clarity. In paced situations, performance-decrement is marked when low rates of signal or stimulus-presentation are used, but becomes smaller when the rate is high.[8] In unpaced situations, performance-decrement is considerably less marked than might be expected from data for similar paced situations: depending on the specific situation and the particular method of evaluating *S*'s latencies, the decrement at unpaced tasks may be negligible or slow to appear.[9] Broadbent, in fact, has been inclined to accept the view that decrement does not occur with unpaced tasks.[10]

The task used in the present experiment would probably not fall within the definitions of vigilance-tasks adopted by most workers. Stimulus-presentation rates, which here were a function of *S*'s searching-pace as well as the order of numbers on the page, where high, as indicated by the average detection-rate of about 200 numbers per hr. Furthermore, while there was uncertainty as to the time of 'appearance' of the number being sought, *S* had somewhat greater knowledge of the stimulus-program than is available in most vigilance-tasks. He knew that the number being looked for was indeed on the page and hence that the interval between detections should never be very long.

The task was, nevertheless, one involving alertness and as such should manifest performance-changes in time which are related to those found for vigilance-tasks. The task had two important characteristics: it was an unpaced task and it was a task where the rate of detection was high. On both these counts, data from vigilance-experiments would suggest that the decrement would be a relatively small one—and so it was. The fact, however, that the decrement obtained was a change of some 10% and that it was statistically significant, cautions one against the generalization which Broadbent has advanced that performance-decrement is not to be expected with observer-paced tasks.

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[2] D. E. Broadbent, Noise, paced performance and vigilance, *Brit. J. Psychol.*, 44, 1953, 295-303.

[3] N. H. Mackworth, Researches on the measurement of human performance, *Med. Res. Council Spec. Report Series*, No. 268, 1950, 1-156.

[4] H. M. Jenkins, Performance at a visual monitoring task as a function of the rate at which signals occur, *Tech. Report No. 47, Mass. Inst. of Tech., Lincoln Lab.*, 1953, 1-25.

[5] W. E. Kappauf, and W. E. Powe, Performance decrement at an audio-visual checking task, *J. exp. Psychol.*, 57, 1959, 49-56.

[6] Broadbent, The twenty dials test under quiet conditions, *Med. Res. Council, Appl. Psychol.*, Unit Report 130/50, 1950, 1-12; Some effects of noise on visual performance, *Quart. J. exp. Psychol.*, 6, 1954, 1-5.

[7] Michael Loeb, G. Jeantheau, and L. A. Weaver, A field study of a vigilance task, *Army Med. Res. Lab., Fort Knox*, Rept. No. 230, 1956, 1-9.

[8] H. M. Jenkins, *op. cit.*, 1-25; Kappauf and Powe, *op. cit.*, 49-56.

[9] Broadbent, *op. cit.*, *Brit. J. Psychol.*, 295-303; *Med. Res. Council, appl. Psychol. Unit Rept.*, 1-12; Loeb, Jeantheau, and Weaver, *op. cit.*, 1-9.

[10] Broadbent, Neglect of surroundings in relation to fatique decrements in output, in W. F. Floyd, and A. T. Welford (eds.), *Symposium on Fatigue*, 1953, 173-178.

PITCH AND THE VERTICAL LOCALIZATION OF SOUND

By PHILIP E. PEDLEY and ROBERT S. HARPER, Knox College

Tones high in pitch were observed, by Pratt, to have a higher spatial localization than tones low in pitch.[1] In his experiment, however, he was not able to account for the particular mechanism that operated to produce this phenomenon. He did state, "If they [his results] receive further verification, auditory theory must look for the physiological correlate which underlies the spatial difference in pitch."[2] In considering this problem, Pratt asked, "May it not be that the observers were not really making judgments of spatial localization, but were really assigning to clearly recognized differences in pitch the spatial characters which inevitably attach to the terms universally employed to designate these pitch differences?"[3] In other words, instead of judging localization, the observers were simply assigning to high or low pitched tones the spatial character implied by the words *high* and *low*. Pratt believed, however, that this was not the case, but rather that the localization was immediately experienced. As he said, "I can only reply that on the basis of the introspective conviction of the observers and myself I think emphatically that this is not the case."[4]

Later, during World War II, a device known as "Flybar" (Flying by Aural Reference) was introduced.[5] This instrument was proposed as a navigational aid in the blind-flying of aircraft. It consisted of apparatus for presenting tones of varying characteristics independently to the two ears. From changes in these characteristics, the pilot was able to tell whether his plane was following the proper flight path. Several characteristics of the tones were changed—absolute intensity, intensity-differences between the ears, and pitch-differences between the ears. Each type of change was related to some aspect of the attitude of the plane. For example, pitch might reflect the height of the plane: if the plane was climbing, the pitch of the pulsating tone would rise; if the plane was falling, the pitch would drop.

The success of a test of "Flybar" supported Pratt's findings to the extent that high pitched tones were localized higher in space than low pitched tones. This finding may be accounted for by either of two possibilities. (1) Each audible frequency, either by a learned or an innate process, has a particular height associated with it. This would imply a mechanism that discriminated on an *absolute* basis. (2) Tones heard as high or low in pitch, *relative* to some subjective standard, have a particular height associated with their perception. This would imply a mechanism that discriminated on a relative basis.

An experiment was designed to test the relationship between pitch and vertical localization under the two hypotheses stated. This was done by presenting a group of low pitched tones, a group of intermediate pitched tones, and a group of high

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* Received for publication April 21, 1958.

[1] C. C. Pratt, The spatial character of high and low tones, *J. exp. Psychol.*, 13, 1930, 278-285.

[2] Pratt, *op. cit.*, 284.

[3] *Op. cit.*, 283f.

[4] *Ibid.*, 284.

[5] Alphonse Chapanis, W. R. Garner, and C. T. Morgan, *Applied Experimental Psychology*, 1949, 250-256.

pitched tones, with two of the tones common to all groups. The highest two tones of the low group were the same as the center tones of the intermediate group and the lowest tones of the high group. If the highest tones in each of the three groups are localized at the same height, up high, and the low tones equally low, then the effective discriminative mechanism would be a relative one. On the other hand, if the highest tones in the low group are judged equal in height to the lowest tone in the high group, the effective mechanism would be an absolute one.

Procedure. The apparatus consisted of an audio-frequency oscillator capable of generating pure tones at the frequencies shown in Table I. The output was fed to two loud speakers connected in phase and mounted behind the top and bottom of a black cloth screen. The screen was marked off at 1-ft. intervals with numbers from one to seven, the number '1' being 1 ft. from the floor, and number '7,' 7 ft. from the floor. The speakers were mounted behind Positions 1 and 7.

TABLE I

FREQUENCY OF TONES IN THREE GROUPS

(Numbers in parentheses are the order of presentation within each group)

Group A	Group B	Group C
(4) 100	(2) 240	(6) 900
(7) 150	(7) 370	(2) 1400
(5) 240	(6) 580	(5) 2250
(1) 370	(3) 900	(7) 3400
(3) 580	(5) 1400	(1) 5200
(2) 900	(1) 2250	(3) 8000
(6) 1400	(4) 3400	(4) 12500

Twelve students served as *S*s. Each *S* sat in a chair facing the screen 10 ft. away. His head was level with Position 4. The *S*s were told they were to take part in an experiment to determine the accuracy with which tones of different frequencies could be localized. They were informed that there were seven speakers, one behind each of the seven numbers, and a group of seven tones of different pitches would be presented randomly through these speakers. They were to listen to each tone and then record the number posted in front of the speaker from which it came.

Each *S* made one judgment of each tone in the group. Each tone was presented for 15 sec., with a 5-sec. interval between tones in each group. He then had a 5-min. rest-period and repeated the procedure for the second and third groups.

Tones were presented through the two speakers in three groups of seven tones each. The frequencies of the tones in each group are shown in Table I. These tones, spaced equally on a logarithmic scale, were randomly selected within each group. The order of presentation of the three groups was systematically varied to control for sequential effects. The 900∼ and 1400∼ tones were common to each of the three groups, being the highest in the low group, the middle pair in the intermediate group, and the lowest in the high group.

Results. The mean heights at which the critical tones were judged, in each of the three groups, are shown in Fig. 1.

The two critical tones were localized lower in position when they were of low pitch relative to the rest of the tones in the group than when they were of higher pitch relative to the other tones of the group. The higher the relative pitch of the tone, the higher was its relative height.

On the basis of the hypothesis of an absolute discrimination, one would expect that, regardless of the position of the tone within the group, it would be localized at the same height in all groups. The most probable localization would be at either Positions 3 or 4 for the 900~ tone, and either Positions 4 or 5 for the 1400~ tone. Thus, with one observation of each tone in each group by 12 *Ss*, the 900~ tone would be judged at Position 3 or 4 twelve times. The relationship of the observed frequencies to these theoretical frequencies was tested by the Chi-squared test. With *df.* = 2, and $X^2 = 22.7$, the hypothesis implying an absolute mechanism was rejected at $P < 0.01$.

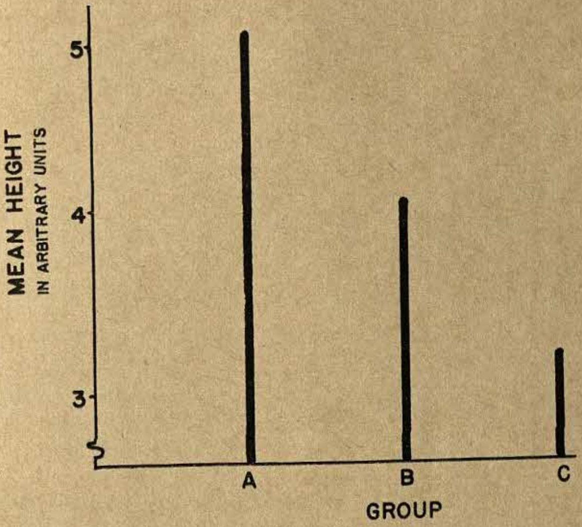

FIG. 1. THE MEAN HEIGHT AT WHICH THE 900~ AND 1400~ TONES WERE JUDGED TO BE IN EACH GROUP

In Group A the critical tones were the highest pitched tones; in Group B they were the intermediate pitched tones, and in Group C they were the lowest pitched tones.

This experiment provides further validation of Pratt's findings that *Ss* judged tones of different pitches as originating at different heights. In terms of mean height of judgments, the results follow a pattern that would be predicted by the action of a relative mechanism. Thus, the critical tones were localized higher in space when they were presented within a group of low pitched tones than when they were presented within a group of intermediate or high pitched tones.

Summary. The *Ss* were presented with groups of 7 tones of different pitch and judged the height from which the tones originated. When tones were of high pitch, relative to other tones in the group, they were judged to originate higher in space than when they were of lower pitch in relation to other tones in the group. The mechanism responsible for this judgment was concluded to be a relative rather than an absolute mechanism.

THE RETENTION OF 'GOOD' AND 'BAD' FIGURES

By DONALD F. SOLTZ and MICHAEL WERTHEIMER, University of Colorado

The gestalt principles of form-perception have been the subject of many investigations. Some attempts have also been made to apply these principles to memory, with the expectation that "modification within the memory-trace for visual forms must be in the direction of reducing the stresses present in the original perception."[1] Wulf, after exposing a number of simple forms briefly and asking his *Ss* to reproduce these forms at various time-intervals, found that the reproductions differed from the original forms in the direction of either "sharpening" or "leveling."[2] While many of Wulf's figures were asymmetrical, Allport, using more symmetrical figures along with asymmetrical ones, found that *Ss*' reproductions showed a tendency to leave the symmetrical figures unaltered and to change the asymmetrical ones.[3] Hubbell reports similar results, using a different method.[4] Perkins also found what he considered definite evidence for change in the direction of balance and symmetry.[5]

There have, however, been a number of studies which have produced results not entirely consistent with these findings.[6] The material in the literature seems to be almost evenly balanced between results which are consistent with the gestalt view and results which can be considered to contradict it. In an attempt to help clarify the situation, the present study was designed to discover whether part of the problem may not be due to differences in the kinds of figures used. From the gestalt point of view, one would expect 'bad' figures to suffer alteration in the memory-trace, while 'good' figures would be less likely to suffer such change. Hence one could predict that 'good' *Gestalten* should be more easily remembered than 'bad' ones. For the purposes of this study, following Hubbell, open asymmetrical figures were considered 'bad,' while closed symmetrical figures were considered 'good.'

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* Received for publication August 22, 1958. The assistance of Mr. Leon R. Mittman in this study is gratefully acknowledged.

[1] C. E. Osgood, *Method and Theory in Experimental Psychology*, 1953, 588.

[2] Friedrich Wulf, Beiträge zur Psychologie der Gestalt: VI. Über die Veränderung von Vorstellungen (Gedächtnis und Gestalt), *Psychol. Forsch.*, 1, 1922, 333-373.

[3] G. W. Allport, Change and decay in the visual memory image, *Brit. J. Psychol.*, 21, 1930, 133-148.

[4] M. B. Hubbell, Configurational properties considered 'good' by naïve subjects, this JOURNAL, 53, 1940, 46-69.

[5] F. T. Perkins, Symmetry in visual recall, this JOURNAL, 44, 1932, 473-490.

[6] J. J. Gibson, The reproduction of visually perceived forms, *J. exp. Psychol.*, 12, 1929, 1-39; N. G. Hanawalt, Memory trace for figures in recall and recognition, *Arch. Psychol.*, 31, 1937 (No. 216), 1-89; F. W. Irwin and M. A. Seidenfeld, The application of the method of comparison to the problem of memory change, *J. exp. Psychol.*, 21, 1937, 363-381; M. A. Seidenfeld, Time as a factor in the recognition of visually perceived figures, this JOURNAL, 51, 1938, 64-82; D. O. Hebb and E. N. Foord, Errors of visual recognition and the nature of the trace, *J. exp. Psychol.*, 35, 1945, 335-348.

Method and procedure. Four 'good' and four 'bad' figures, taken from Hubbell,[7] were presented to 60 undergraduate women at the University of Colorado. Women were employed because Hubbell had quantified her figures on women. The eight figures were presented simultaneously on a single sheet of white paper (8½ × 11 in.), for a period of 15 sec. The order of the figures was randomized on the sheets for different *Ss*, to avoid any bias which might result from the constant position of a given figure. There were four different exposure-sheets containing the same eight figures in different positions, and 15 *Ss* were assigned to each sheet. They were told to look at each figure separately, and to try to remember as much as they could about each one, since they were to be tested later for memory of the figures. Immediately after exposure, 30 of the *Ss* were presented with sheets containing 80 varied figures and including the 8 figures they had just been shown. For purposes of calibration, the 72 new figures had previously been judged by five women undergraduates in terms of the similarity of each of them to the 8 exposed figures. The original plan had called for the use of 10 of Hubbell's exposure-figures and 100 test-figures, but a number of figures were eliminated to equate the number of recognition-figures similar to the 'good' and 'bad' figures on the exposure-sheets. The number of figures on the recognition-sheet similar to the eight exposure-figures was equalized on the basis of these judgments. This was done to eliminate the bias which might have resulted from the presence of more figures similiar to the four 'bad' original figures than to the four 'good' figures, or vice versa.

The second group of *Ss* were tested for recognition two weeks after the original exposure, but only 28 of them were available. The method was the same as that used in the test for immediate recognition. All the *Ss* were allowed a maximum of 3 min. with the recognition-sheets.

Results. Recognition of the figures immediately after exposure to them was almost perfect. Only 3 of the 30 *Ss* failed in perfect recognition—these three failing to recognize one 'bad' figure apiece. Although the trend here is in the predicted direction, the extremely good performance of the *Ss* was such as to eliminate any statistically significant differences in the recognition of 'good' and 'bad' figures which might have emerged had recognition not been so uniformly excellent.

In the group tested 2 weeks later, 21 *Ss* recognized at least one more 'good' than 'bad' figure, 3 *Ss* recognized more 'bad' than 'good' figures, and 4 recognized an equal number. Using the binomial expansion, the probability of obtaining such a degree of superior recognition of the 'good' figures by chance is considerably less than 0.01. A significant difference ($p < 0.01$) also occurred between the results for immediate recognition as compared with recognition after two weeks.

Discussion. The results clearly support the hypothesis that 'good' figures are easier to recognize than 'bad' ones. This difference, however, seems to emerge only after sufficient time has passed to reduce the accuracy of recognition to a low level. This finding seems contradictory to that of Irwin and Seidenfeld, who found that changes occurred shortly after exposure but did not increase with further passage of time.[8] The contradiction may be only apparent, however, both because the tasks used in the two studies differed, and because the problem in the present study may have been too simple, since it yielded such extremely accurate recognition on immediate tests.

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[7] Hubbell, *op. cit.*, 47.

[8] Irwin and Seidenfeld, *op. cit.*

In addition to considering the findings as consistent with the gestalt view, they can also be readily interpreted in the context of information-theory.[9] It takes less information to specify a closed, symmetrical figure than it does to specify an open, asymmetrical one. From this point of view it is not at all surprising that memory for 'good' figures is superior to memory for 'bad,' since a greater amount of information must be retained for accurate recognition of the 'bad' ones. This analysis would make it unnecessary to postulate more extensive changes in the memory-traces of the 'bad' figures than in those of the 'good.' Which of these two factors—if, indeed, they are different factors—is responsible for the substantial difference obtained in this study cannot be ascertained within the present design.

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[9] J. E. Hochberg and Edward McAlister, A quantitative approach to figural 'goodness,' *J. exp. Psychol.*, 46, 1953, 361-364.

TRANSPOSITIONAL RESPONSES OF RHESUS MONKEYS TO STIMULUS-OBJECTS OF INTERMEDIATE SIZE

By George V. Gentry, John E. Overall, and W. Lynn Brown,
University of Texas

Whereas Spence has demonstrated that a theory of discriminative learning of the 'absolute' type can predict continued choice of the 'larger' of two stimulus-objects during transpositional testing, he concludes that choice of the object of intermediate size in a three-stimulus problem should provide a critical test of that theory.[1] He trained six chimpanzees to choose the intermediate of three stimulus-objects differing only in size. During transpositional testing, stimulus-values were so shifted that the intermediate and largest objects during training became, respectively, the smallest and intermediate objects during the test. Thus, the *Ss* were given the choice between an object of intermediate-size which had not been rewarded during training and an object not of intermediate size which had been rewarded during training. The chimpanzees were strong in their preference for the object which had been positive during training, apparently disregarding its non-intermediate relation during the test.

Gonzales, Gentry, and Bitterman refined the paradigm of transpositional testing to maximize sensitivity to 'relational' tendencies which might be present in choice of the stimulus-object of intermediate size.[2] Four chimpanzees were trained to choose the intermediate of three objects differing only in size. During transpositional testing, the stimulus-sizes were so shifted that the object of intermediate size in each test-set and one not intermediate in size were equidistant along the size-continuum from the 'absolute' value of the positive object. In terms of an 'absolute' theory of discrimination, responses of the *Ss* should have been equally distributed between the test-objects which were equally distant from the 'positive' object. Actually, the four chimpanzees demonstrated a significant preference for the test-object which appeared intermediate in size in the test-set.

Apparently, the chimpanzee, when allowed to choose between the two solutions, responds to 'absolute' stimulus-objects rather than to relations between objects. When, however, 'absolute' stimulus-objects determine no distinct preference, responses suggest that the chimpanzee has learned something about relationships between the objects. Because transposition of the 'intermediate-size' choice appears critical for theories of discriminative learning, the present experiment was undertaken: (1) to verify the 'relational learning' observed by Gonzales, Gentry, and Bitterman; and (2) to extend the demonstration of 'relational' choice to the rhesus monkey— a species somewhat further removed from man than the chimpanzee.

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* Received for publication September 11, 1958.

[1] F. W. Spence, The differential response in animals to stimuli varying within a single dimension, *Psychol. Rev.*, 44, 430-444, 1937; The basis of solution by chimpanzees of the intermediate size problem, *J. exp. Psychol.*, 31, 1942, 257-271.

[2] R. C. Gonzales, G. V. Gentry, and M. E. Bitterman. Relational discrimination of intermediate size in the chimpanzee, *J. comp. physiol. Psychol.*, 47, 1954, 385-388.

Method: Subjects. The present report, which is concerned solely with the problem of discriminative learning, discusses the performance of eight normal, non-irradiated control animals belonging to a larger group maintained at this laboratory for investigation of the effects of chronic, whole-body irradiation. These *Ss* were about 5 yr. of age at the start of the experiment and represented the most highly sophisticated monkeys available at this laboratory, having had more than 3 yr. of experience in a variety of psychological experiments. The monkeys were housed indoors in air-conditioned quarters and worked five days a week. In addition to raisins, apples, peanuts, and corn received as incentives during testing, they were fed daily in their home cages at the close of each work day.

Apparatus. A string-pulling apparatus was employed throughout the experiment. The stimulus-objects presented on each trial were mounted on tracks and equipped with strings by which they could be drawn within reach by the *S*. The objects were cubical wooden boxes, each of which was open at the top and painted black both inside and out. The boxes were designated in order of size with the numbers 1 to 9. The faces of the smallest box was 9 sq.in. in area, and the size of each successive box in the series was increased by a factor of 1.15.

Procedure. Each *S* was first trained to a criterion of 16 out of 18 correct responses to choose the box of intermediate size from among Boxes 1, 5, 9. Eighteen trials were given each day and the correction-method was employed throughout the experiment. The spatial arrangement of the three boxes was randomly varied. Following training on Boxes 1, 5, 9, the discrimination was sharpened by training to the same criterion on Boxes 3, 5, 7.

The test for transposition involved two new sets of boxes: Boxes 2, 4, 6 and 4, 6, 8. In each case, Boxes 4 and 6, presented on each test-trial, differed in size from the positive box (Box 5) by equal ratios (1.15). Transpositional testing was accomplished in four days. Each day, 12 trials involved continued training on Boxes 3, 5, 7 with the box (Box 5) of intermediate size being rewarded. Interspersed among the training-trials each day were six test-trials—three with Boxes 2, 4, 6 and three with Boxes 4, 6, 8. On test-trials any choice was rewarded. In 4 days of testing, each test-set was presented 12 times, twice in each of the 6 possible spatial arrangements.

Results and discussion. Table I presents the distribution of responses to the boxes of each test-set for the eight normal sophisticated rhesus monkeys. On the 24 test-trials, the mean number of responses to boxes intermediate in size was 17.8. Since no theory of discriminative learning would predict choice of extreme stimulus-values, Boxes 2 and 8, a chance distribution of responses should result in approximately 12 out of 24 choices of the box of intermediate size. The preference for the intermediate-size box demonstrated by the *Ss* of this experiment is greater than chance at the 1/10% level of significance.

Choice of the box intermediate in size, disregarding absolute stimulus-values, should result in an equal distribution of responses between Boxes 4 and 6. The mean number of choices of these boxes were 11.9 and 11.1, respectively. These values do not differ from chance to any significant degree.

Above chance-response on transpositional test-trials to the box intermediate in size indicates that rhesus monkeys do learn stimulus-relationships during discriminative training. During training on the discriminative problem, the hypothetical gradients of excitation and inhibition which are established should be symmetrical about Box 5. To the extent that Boxes 4 and 6 are functionally equidistant from Box 5 along the size-continuum, the strengths of response-tendencies based upon 'absolute' stimulus-properties should approach equality. When response tendencies based upon 'absolute' stimulus-values are equated, rhesus monkeys demonstrate significant 'relational learning.' Thus, the results of the present experiment confirm results obtained by Gonzales, Gentry, and Bitterman with chimpanzees and extend the findings to a lower primate form.

TABLE I

NUMBER OF TIMES THE VARIOUS BOXES WERE CHOSEN DURING THE TEST-TRIALS

S	Boxes:				No. times the intermediate box:	
	2	4	6	8	chosen	not chosen
1	2	9	1		17	7
		4	8	0		
2	0	10	2		20	4
		2	10	0		
3	0	10	2		18	6
		4	8	0		
4	1	8	3		16	8
		3	8	1		
5	2	8	2		17	7
		3	9	0		
6	0	7	5		15	9
		4	8	0		
7	2	10	0		20	4
		2	10	0		
8	0	9	3		19	5
		2	10	0		
Total	7	95	89	1	142	50

APPARATUS

A MULTI-FIELD ELECTRONIC TACHISTOSCOPE

By Harold S. Koletsky, Syosset, New York, and Paul A. Kolers, Wright Air Development Center

A number of tachistoscopes have been described within the past few years which use electronic components and gas-discharge tubes as the controlling elements in a Dodge-type structure.[1] These units, however, have generally been restricted to two-channel operation: adapting field and stimulus-field.[2] The present note describes an electronic apparatus which permits precise control of duration, intensity, and sequence of many viewing fields. By extending the principle of the Dodge tachistoscope to several viewing fields, it becomes possible to explore visual functions over a far wider range than heretofore.

Circuitry. The basic timing-circuit is a mono-stable multivibrator. This circuit controls a power amplifier which, in turn, operates the cold cathode gas-discharge tubes used as light sources. The multivibrator (5687) and power tube (6S4A) circuits of a single channel—controlling a single viewing field—are shown in Fig. 1. It was found physically convenient to build three such circuits into one three-channel unit with integral power supply. A block diagram of such a three-channel unit appears as Fig. 2.

Circuit-operation is that of a typical multivibrator and is best explained by describing a single channel. In its stable condition the right half of the multivibrator (T-2) conducts with the grids of the left half (T-1) and of the 6S4 held below their cut-off points. At a given moment, a large negative pulse comes onto the grid of T-2, which reduces the plate current of that tube. The resulting positive pulse on the plate of T-2 is placed on the grids of T-1 and of the 6S4, causing the latter two tubes to conduct. This action further reduces the voltage on the grid of T-2 by regeneratively cutting off that tube and putting T-1 full on. Conduction in T-1 then continues through approximately half the time-constant established by the RC network (R and C of Fig. 1). At the end of that time the potential on the grid of T-2 has been restored to the cut-off value, which causes that tube to begin to conduct regeneratively, driving T-1 and the 6S4 off. The large negative pulse on the plate of T-2, associated with the flopping of that tube, becomes a negative trigger to the right grid of the multivibrator next in sequence. With T-2 conducting, the circuit is again in its stable condition and remains so until another negative pulse is applied to the grid of T-2.

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[1] Raymond Dodge, An improved exposure apparatus, *Psychol. Bull.* 4, 1907, 10-13.

[2] Lawrence Karlin, (The New York University tachistoscope, this Journal, 68, 1955, 462-466) describes such a device and discusses the drawbacks of some earlier versions.

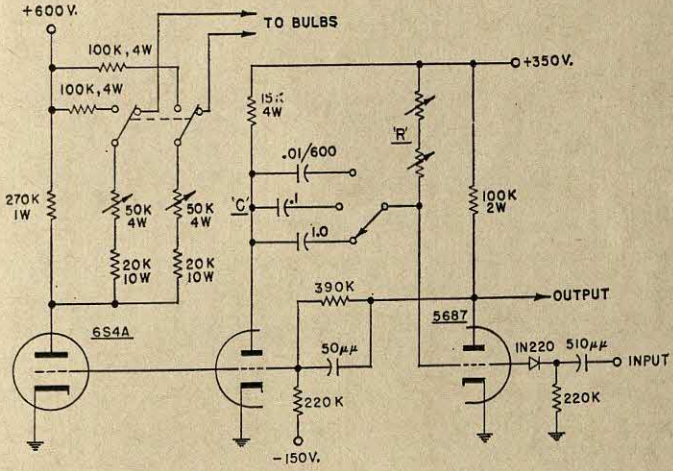

FIG. 1. SCHEMATIC DIAGRAM OF ONE CHANNEL

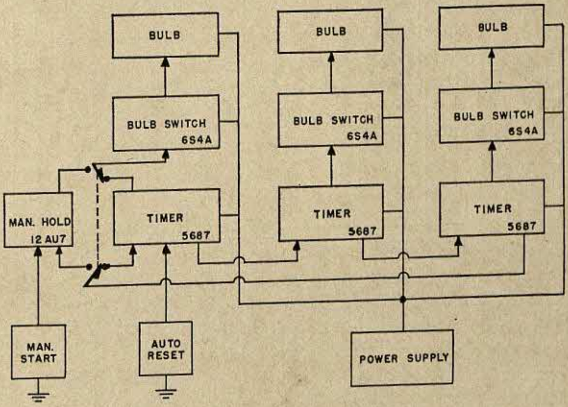

FIG. 2. BLOCK DIAGRAM OF THREE CHANNELS WITH INTEGRAL POWER-SUPPLY

Since the grid of the 6S4 is coupled directly to the grid of T-1, both tubes will conduct or cut off at the same time. The 6S4 thus serves to amplify the pulse output from the multivibrator, and so to control the light bulbs.

To operate a sequence of lights, the output of one channel is coupled to the input of an identical second channel. As many such channels as desired may be cascaded in this manner. As previously noted, Fig. 2 shows a block diagram of three such channels. Once begun, cycling of the channels continues in the manner described through the squence of as many channels as have previously been arranged and until stopped manually.

In addition to automatic cycling as just described, the manually operated holding circuit indicated in Fig. 2 permits one-cycle operation. This circuit consists of a 12AU7 used as a bi-stable multivibrator. When T-1 of this tube conducts, the bulb in Field 1 is held OFF. When T-2 conducts, this bulb is held ON indefinitely. Thus, when this circuit is operative, the 5687 of Channel 1 is by-passed and Field 1 is held ON (bulb lit) until Channel 1 is released manually. After Channel 1 is released, the device runs through one sequence of channels and returns to Channel 1 again ON.

The light bulbs controlled by these channels are of the cold cathode gas-discharge type. One side of the bulb is always held at 600 v. When a channel is in its stable condition (the 6S4 cut off), 600 v. appears also at the second side of the bulbs controlled by that channel, which holds the bulbs OFF. When the 6S4 conducts, the large voltage drop on its plate is reflected on the second side of the bulb. The resulting voltage across the bulb causes the latter to light.

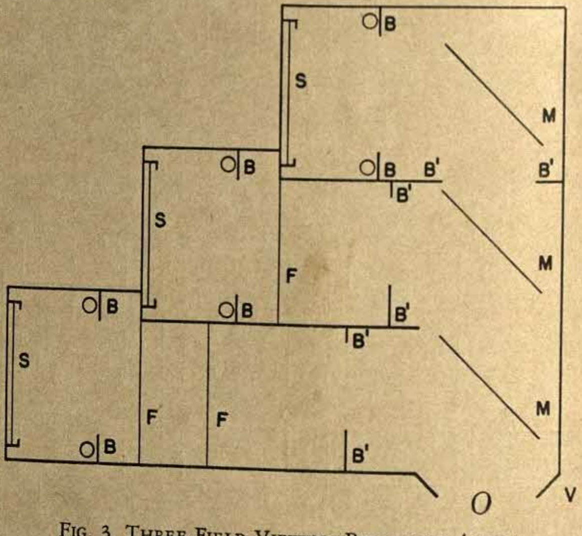

FIG. 3. THREE-FIELD VIEWING-BOX FROM ABOVE

S = stimulus-card; B = fixed baffles; B′ = adjustable baffles; M = semi-reflecting mirror; F = filter; O = cold cathode light bulb; V = viewing hood; *O* = observer.

With the present device, two bulbs are controlled by each channel. The intensities of the bulbs may be varied independently over at least 25% of their maximal output, a range which more than compensates for individual differences in their fabrication. This range of variation permits precise matching of intensities. In addition, a DPDT switch allows the bulbs operated by a channel to be by-passed completely; hence, any of the channels may be used for dark as well as light intervals.

The values of the timed intervals of a channel when the device is on automatic cycling are established by the RC circuits. While theoretically these components may take any value, it was found convenient to have a range from 1 m.sec. to 3 sec. in each channel. This range is achieved by means of three timing capacitors and two sets of fixed resistors (C and R of Fig. 1) in each channel, and may be changed at will by the insertion of larger or smaller capacitors, provision for which exists. In addition to the above values, any viewing field may be illuminated for any period greater than 3 sec. by means of a manually operated switch which places an infinitely large resistance in the RC network. Repeatability of durations of illumination during cycling is better than 1%; precision components in the RC circuit make the timing increments linear. Rise and decay times of the pulses into the gas-discharge tubes are of the order of 200 μsec. The B+ and B− supplies to the timers are regulated to within 1% for a 30 v. change in the line.

Viewing box. Fig. 3 shows a top view of a three-field viewing box. The mirrors and filters indicated are titanium-dioxide coated units whose transmission-reflection characteristics are of the order of 60 : 40 ± 5%. When an eye and a light-source are on opposite sides of the glass, the eye receives the transmitted light; when the two are on the same side of the glass, the eye receives the reflected light. A head rest which may be attached to this unit has provision for a biting-board and for other fixed density filters. The intensity of the light in each field may be varied by the insertion of fixed and variable density filters in the optical path of that field. The viewing box illustrated maintains optical distances constant over the three viewing fields. Its essentially modular construction permits the easy addition of fields as required. It is also possible to position the light bulbs nearer the back wall of a field and trans-illuminate stimulus-materials placed closer to *O*.

APPARATUS NOTE

NEW METHOD OF RESPIRATORY RECORDING

In work with psychotic children we found pressure-sensitive devices such as the pneumograph to be impractical for measuring respiration. The rubber tube around the chest is awkward to keep in place and is easily damaged. It tends to frighten some children and, moreover, it picks up too many nonrespiratory bodily movements.

An iron-constantan thermocouple provides a simple and efficient way of circumventing these difficulties. The fine wire is affixed to rigid headgear such as a plastic toy helmet and the twisted end situated before the child's nose and mouth. The other end of the wire is connected directly to the input of a low-level DC-amplifier.

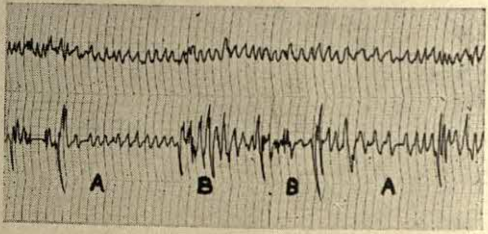

FIG. 1. SAMPLING RESPIRATORY TRACING
The upper tracing was obtained by the traditional pneumographic method; the lower tracing, by the new method.

A record of respiration so obtained and one made by the traditional pneumographic method are shown in Fig. 1. The upper curve was obtained with a Phipps and Bird Pneumotachograph Attachment connected to a Grass Model CP1 transducer and a Grass Model 5P2 High Impedance Pre-amplifier; the lower curve was made simultaneously with an iron-constantan wire (No. T6-36-AT, Thermo Electric Co., Inc., Saddlebrook, N.J.) and a Grass Model 5P1 Low Level Preamplifier. The preamplifiers were mounted in a Grass Model 5 six-channel polygraph. The two records indicate that the thermocouple yields a cleaner curve during periods of general activity (Points B). During periods of quiet (Points A), the two curves are very similar as to rate. It is, of course, understood that this method is appropriate only when the precise amplitude of pen-displacement is not an important variable.

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NOTES AND DISCUSSIONS

A NEW SIZE-DISTANCE ILLUSION

This illusion is produced by viewing monocularly through a tube which has a small hole at the far end. For convenience, one may also make a tube out of one's hand, controlling the aperture at the far end by moving the little finger. Point the tube toward a single object (a vase, a doorknob, someone's head) at any distance greater than 6 ft., controlling the aperture that only the object, and none of its surroundings, is visible. Then remove the tube and, with the other eye still closed, look at the same object in unobstructed view. The increase in apparent size is striking.

That the phenomenon is not simply retinal is shown by the fact that the illusion is experienced even if one sights through the tube with the right eye, keeping the left eye closed, and then closes the right eye to view with the left. Nor does it appear to depend to any extent on a change in the usual cues for distance. This for two reasons: first, it occurs more obviously with monocular than with binocular vision; and secondly, it will work at distances as great as five miles, well beyond the limits within which distance cues are operative. It is not affected by the nature of the target-object, although interestingly enough the effect is even more marked when one chooses as the target an object which is itself a two-dimensional projection, such as the contents of a slide on a screen. Most *S*s, including those as young as 5 yr., spontaneously report a change of apparent size; about 1 in 10 report a change in apparent distance; rarely, and in the writer's experience only among psychologists, the phenomenon is not reported at all, or is denied on questioning. Finally, continued experience with the illusion seems to increase rather than decrease the effect.

Observation of the characteristics of the perceived object under the two conditions suggests that the apparent change is by no means a simple one. The increase in size, as one characteristic of the target-object, is usually so striking as to obscure immediate discovery of the other changes. The object seen through the aperture, which acts as a kind of reduction screen, appears smaller, clearer, and more distant. It is cut off from its surroundings and seems flat, less rounded or three-dimensional than objects seen in binocular view. Koffka,[1] referring to similar paradoxical phenomena such as the "tunnel illusion," the "opera glass illusion," and the so-called Koster phenomenon, attributes them to anisotropy of space, *i.e.* to the fact that not all direction in space are phenomenally equal. In addition, there is the opposite illusion in which something seen nearby in a dense fog appears to loom up, seeming larger and closer than it actually is. Considering this entire group of related visual phenomena, it may be suggested that under the special conditions which give rise to these illusions we are able to compare examples of two general classes of perceived objects. The first class contains objects which are seen as small, clear, isolated from their context, and at some distance, as though they were in a tunnel. The second contains objects seen as large, close, less clear, and embedded in their settings. Myopic persons who in order to see better narrow the visual field by squinting, take advantage of these distinctions; and indeed, to some extent the difference between monocular and binocular vision also accords with them. The new illusion described here, as an easily observable situation in which objects of both types are present for comparison, may therefore have more than passing interest to students of visual perception.

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[1] Kurt Koffka, *Principles of Gestalt Psychology*, 1935, 277-278.

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THE TASTE OF WATER

The lack of a solution that is tasteless to all observers (*O*s) is one of the most serious handicaps to the investigation of taste—particularly in experiments on the psychophysics of taste. Distilled water, and water which the *O*s habitually drink and to which they are accustomed, have for years been used as neutral, tasteless solutions. These solutions are not, however, tasteless to all *O*s; a sizeable percentage of the *O*s reported that these so-called neutral solutions arouse various qualities of taste. Most of the evidence that these solutions are not tasteless to all *O*s is imbedded far back in the literature, but the findings are confirmed by recent studies.

In an experiment undertaken to determine the effects of a threshold-concentration of one quality of taste upon the thresholds of the other qualities, Anderson was unable to obtain, from one of two *O*s, a threshold for bitter (an aqueous solution of quinine hydrochloride) when he used the drinking water at Ann Arbor, Michigan, as the dilutent.[1] Reports of bitter were not only received from this *O* at all concentrations of the stimulus-solutions but even from the water itself. Checks were made with various combinations of drinking and distilled water and in all of them bitter was predominant though judgments of neutrality (or plain water) were acceptable and even encouraged by the instructions.

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[1] R. J. Anderson, Taste threshold in stimulus-mixtures. Unpublished doctoral dissertation, University of Michigan, *Microfilm Abstr.* 10, 1950 (No. 1941), 46 f.

Similar results were obtained by the present author from one of three *O*s using the common drinking water of Gainesville, Florida. The results of a single *O* would have been disregarded if strong supporting evidence had not been found in the literature. In establishing thresholds of a group of *O*s, Von Skramlik was unable to obtain thresholds for bitter (quinine hydrocloride) from 2 out of 10 *O*s when distilled water was used as the dilutent.[2] For these two *O*s, distilled water was reported as tasting bitter.

Using local rather than general stimulation, Kiesow obtained similar results.[3] Using five *O*s, he found that distilled water was reported as bitter at the base of the tongue by all of them. Two of the five *O*s in addition reported that it was bitter at the tip and the sides of the tongue; one that it was neutral (*i.e.* tasteless) at these areas; and two that, though bitter at the base, it was sweet at the tip and sour at the sides. Of the 20 reports regarding the taste of distilled water, 11 were bitter, 4 sour, 3 neutral, and 2 sweet. Kiesow's findings were not the first of this kind to be reported. He states in his study that "Die gleiche Erscheinung ist auch 1870 schon von Camerer beobachtet worden."[4]

As these reports indicate, water (distilled or the usual drinking water) is not tasteless. The numbers of *O*s tested are small; but the conclusion is clear: water is bitter for some *O*s and it cannot be used with them as a dilutent in the psychophysical investigations of that quality. All *O*s should be tested regarding their taste of water before they are used in a psychophysical experiment on taste.

University of Florida RICHARD J. ANDERSON

THE FIRST DISTANCE-CONTROLLED TELEPATHIC TEST

The first distance-controlled telepathic test was reported by Herodotus in his *History* written about 450 B.C. Approximately 100 yr. earlier, Croesus, King of Lydia, was considering whether to go to war against the rising power of Persia. Croesus, not knowing which of the various oracles to trust, decided first to test their abilities before asking the crucial question about going to war. Messengers were sent to seven oracles with the instruction that "they were to keep count of the days from the time of their leaving Sardis, and, reckoning from that date, on the hundredth day they were to consult the oracles, and to inquire of them what Croesus the son of Alyattes, King of Lydia, was doing at the moment. The answers given them were to be taken down in writing, and brought back to him.[1] Only the Delphic oracle reported correctly that he was engaged in the unkinglike task of cooking a tortoise and a hare in a brazen pot. Such then, with time and task controlled, was the first distance telepathic test giving one hit and six misses. Just to complete the 2500-yr.-old story: Croesus, reassured about the prowess of the Delphic oracle sent rich gifts to it, and, at the same time, inquired about the advisability of going to war with Persia. He was assured that, if he did so, a great empire would be destroyed. How was he to know that it was to be his own?

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[2] Emil von Skramlik, Mischungsgleichungen im Gebiate des Geschmackssinnes, *Z. Sinnesphysiol.*, 53, 1921, 46 f.

[3] Friedrich Kiesow, Beitrage zur physiologischen Psychologie des Geschmackssinnes, *Philos. Stud.*, 10, 1894, 329-368; 523-561, esp. 362, 523.

[4] Kiesow, *op. cit.*, 365.

Northwestern University ROBERT I. WATSON

FIFTY-FIFTH ANNUAL MEETING OF THE SOCIETY OF EXPERIMENTAL PSYCHOLOGISTS

The fifty-fifth annual meeting of the Society of Experimental Psychologists was held at The Johns Hopkins University on April 17 and 18, 1959. W. R. Garner, Chairman of the Society for the year, presided at the business meeting and at sessions for presentation of scientific papers.

The following members attended the meeting: Bray, Brogden, Chapanis, Egan, Estes, Garner, Geldard, E. J. Gibson, J. J. Gibson, Grant, Helson, Humphreys, Hurvich, Irwin, Kennedy, Landis, Licklider, Miles, Morgan, Mueller, Neff, Newman, Pfaffman, Ratliff, Riggs, Schlosberg, K. U. Smith, Solomon, Stevens, Volkmann, and Wickens.

Walter Rosenblith, Leo Postman, and Eliot Stellar were elected to membership in the Society, bringing the total membership to 58 members and 23 fellows.

Members presented reports of research in progress at scientific sessions held the afternoon of the 17th and the morning of the 18th.

The Warren Medal for 1959 was awarded to Harry Helson "for his development of the concept of adaptation-level and his demonstration of its power to encompass many empirical findings, to reconcile conflicting results, and to point the way to new investigations."

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[1] Herodotus, *History*, Rawlinson trans., Book 1, Section 47.

The Society passed the following resolution:

> The Society of Experimental Psychologists, anticipating the flood of students scheduled shortly to descend upon the colleges and universities of the Nation, urges administrations of institutions of higher learning to make every effort to preserve high standards in the teaching of psychology. The history of psychology attests the necessity of basing the social and humanitarian aspects of the subject on a solid foundation of experimentally oriented facts and principles. We recognize that, in a period of unusual demands on the machinery of higher education, there may develop strong temptations to ignore the more basic areas of the science to the detriment of its ultimate fountainhead, the laboratory. In the present day, all subdivisions of psychology embrace the experimental method and depend on laboratory techniques for their nourishment. Whereas we recognize some of the problems lying just ahead and expect to share in their attempted solutions, we urge unstinted support for psychology as a laboratory science.

The Society accepted the invitation of the University of Wisconsin to meet in Madison in 1960. W. J. Brogden was elected Chairman for 1959-1960.

University of Chicago W. D. NEFF

FIFTY-FIRST ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY

The Southern Society for Philosophy and Psychology held its fifty-first annual meeting at the Sheraton-Jefferson Hotel in St. Louis, Missouri, on March 26–29, 1959. The Departments of Philosophy and Psychology of Washington University were hosts.

Thirty-five papers were presented in philosophy, and 29 in psychology. The special award papers were presented by William Thomas Blackstone (University of Florida), on "Objective emotivism," and by Alfred E. Kuenzli (University of Southern Illinois), entitled "Psychological sources of ethical objectivity." These are selected annually from among those papers presented by members who have not yet received their Ph.D. degrees or who have received them within the past five years. At the annual joint session, papers were presented by John R. Reid (University of Maryland), and Saul Rozenzweig (Washington University), on "Psychotherapy with and without values." Comments on these papers were made by Franklin J. Shaw (University of Alabama), and James K. Feibleman (Tulane University). The presidential address, given by Lewis E. Hahn (Washington University), was entitled "Philosophy as a comprehensive vision."

At the annual business meeting 29 philosophers and psychologists were elected to membership, and 11 to associate membership. The following officers were elected for 1959–1960. Wilse B. Webb (University of Florida), President; Rubin Gotesky (University of Georgia), President-elect; Dan R. Kenshalo (Florida State University), Secretary. Edgar Henderson (Florida State University) is in his second year of a three-year term as Treasurer. Harold Lee (Tulane University), and Stanford Ericksen (Vanderbilt University), were elected as Council Members from Philosophy and Psychology, respectively. Leroy Loemker (Emory University) was appointed by the Council to fulfill the unexpired term of Rubin Gotesky who is now a Council Member by virtue of his office as president-elect.

A short memorial was observed for late members C. A. S. Dwight, Ernest Meyers, and Henry Wunderlich.

The membership accepted the joint invitation of Florida State University and the University of Florida to hold their next meeting in Biloxi, Mississippi on April 14-16.

Florida State University DAN R. KENSHALO

THIRTY-NINTH ANNUAL MEETING OF THE WESTERN PSYCHOLOGICAL ASSOCIATION

The Western Psychological Association held its thirty-ninth annual meeting at San Diego, California, April 16-19, 1959. Registration for the meeting was 961, with approximately 100 additional guests attending. The committee in charge of both program and arrangements consisted of Charles M. Harsh, Oscar J. Kaplan, Wallace V. Lockwood, William H. Stegeman, and Ivan N. McCollom (Chairman).

The program included 156 papers, 19 symposia, and a workshop for state association officers. Scheduled meetings of related groups included Psi Chi, the California State Psychological Association, and the Group Psychotherapy Association of Southern California. At the annual dinner, Quinn McNemar delivered the presidential address, "At random: sense and nonsense." An invited address presented by Karl H. Pribram was entitled, "A nervous system for psychologists—1959 model, built from the inside out."

The President for the coming year is Donald B. Lindsley. Francis H. Palmer continues as Secretary-Treasurer. David Krech is President-elect for 1960-61. The 1960 meeting will be held in April at San José, California.

San Diego State College IVAN N. MCCOLLOM

THIRTY-FIRST ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The Midwestern Psychological Association met at the Hotel Morrison, Chicago, on May 7, 8, and 9, 1959. Host institutions were the Illinois Institute of Technology and Northwestern University. George S. Speer, Illinois Institute of Technology, served as chairman of the Local Arrangements Committee.

A total of 2011 persons registered at the meetings. The number of newly elected and reinstated members during the year was 317, bringing the current membership to 2328.

The program, planned by a committee under the chairmanship of Alexander M. Buchwald, Indiana University, consisted of 112 volunteered papers (presented in 20 sessions), 11 symposia, 4 seminars, an open meeting concerned with current developments in VA psychology programs, and a meeting of Psi Chi which included volunteered papers and an invited address by J. R. Kantor.

The 20 sessions at which papers were presented were concerned with: personality; test behavior; reinforcement; psychopharmacology; human development; animal development and early experience; measurement and scaling; somatic responses; concept formation; studies of amount of reward; comparative psychology; learning and conditioning; social psychology; industrial psychology; physiological psychology; probability learning and decision making; reinforcing factors in verbal conditioning; cerebral pathology; sensory and perceptual processes; and human learning and conditioning. The symposia and seminars were concerned with: perceptual-motor performance; recent approaches to the question of subliminal perception; testing differences within a multiple treatment design; the D^2 statistic in studies of interpersonal perception; psychopharmacology—an experimental approach to clinical problems; cognitive deficit in schizophrenia; interpersonal behavior—a learning-theoretic approach; conceptual and methodological developments in parent-child research; the validity of experimental designs oriented toward acceptance of the null hypothesis; contemporary approaches to traffic behavior; identifying and providing for the intellectually gifted child during the elementary school years; creativity; the medical orientation of the psychologist in a medical setting; human factors in the design of weapon systems; and the modification of leadership attitudes through management development courses.

The presidential address, "Trends and troubles of experimental psychology," was delivered by Arthur W. Melton, University of Michigan.

Ross Stagner, Wayne State University, was elected president for 1959-60, and Donald R. Meyer, Ohio State University, was elected to the Council for 1959-62. The 1960 meeting will be held April 28, 29, and 30 at the Deshler Hilton Hotel, Columbus, Ohio.

State University of Iowa I. E. FARBER

THIRTIETH ANNUAL MEETING OF THE EASTERN PSYCHOLOGICAL ASSOCIATION

The Eastern Psychological Association met April 3 and 4, 1959, at the Chalfonte-Haddon Hall in Atlantic City, New Jersey. Of the 1650 persons registered at the meetings, 884 were members of the Association, 487 were guests, and 288 were new members who joined the Association at the meetings. The present active membership of the Association totals 3283.

A committee headed by John B. Miner was responsible for local arrangements and the program was planned by a committee under the chairmanship of Julius Wishner. The program consisted of 218 papers (presented in 30 sessions), 6 symposia, 5 special meetings, 3 invited addresses, 3 films and a film-making demonstration. The 30 paper sessions were concerned with the following topics: influence processes, brain mechanisms, cognitive processes, perception (2 sessions) evaluation and measurement, aversive learning, comparative, personality and social, conditioning, brain damage, psychopharmacology (2 sessions), self-concept, engineering psychology, human learning, audition, child, decision processes, quantitative methods, brain-simulation, sensory intermittence, clinical (2 sessions), animal learning, social processes, verbal behavior, animal behavior, motor performance, and sensory processes.

The symposia were titled: "The role of the physician in modern society;" "Problems of research in clinical psychology;" "Current developments in Rorschach technique;" "Research foci in the psychological study of the mentally retarded;" "Theoretical issues in exploratory and related behaviors;" and "The continuum hypothesis in neurosis and psychosis." In addition to special meetings entitled "Administration, psychology, and public mental health programs," and "Training for the practice of group-therapy," the program included a meeting of Psi Chi, a meeting of state association officers sponsored by the APA Board of Professional Affairs, and a meeting concerned with graduate training in clinical and counseling psychology. Representatives of Eastman Kodak and the Pennsylvania State University demonstrated the production and use of inexpensive teaching films.

The following invited addresses were given; 'Toward an indigenous methodology—reflections prompted by certain trends of psychology: A study of a science," by Sigmund Koch; "A microanalysis of human problem-solving behavior," by Herbert A. Simon and Allen Newell; and "Toward a comparative psychology of learning: Some experiments with fish and other animals," by M. E. Bitterman.

Carl Pfaffmann presented the annual presidential address, "The pleasures of sensation." During the business meeting it was announced that the following new officers had been elected: President, James J. Gibson; Directors, Stuart W. Cook and Charles N. Cofer.

The 1960 meetings of the Association will be held at the Statler Hilton in New York City, April 15 and 16. The 1961 meetings will be held in Philadelphia.

CARL H. RUSH

New York, N.Y.

SIXTH ANNUAL MEETING OF THE SOUTHWESTERN PSYCHOLOGICAL ASSOCIATION CONFERENCE

The Southwestern Psychological Association met in joint session with the Kansas Psychological Association in Topeka, Kansas, April 16–18, 1959. The program contained both papers and symposia.

Papers were read on a wide variety of subjects, including perception, motivation, clinical problems, educational problems, learning, personality and social, and physiological psychology. The symposia topics ranged from discussion of areas such as school psychology and mental retardation, through items like repression, problems in psychotherapy, methodological approaches in study of animal behavior, human factors in engineering, mental health in industry, to more unusual subjects such as, group dynamics in organizational settings, creative aspects of interdisciplinary team research and finally to psychopathology of murder and forensic psychology.

Special events included a Psi Chi luncheon and tours of the VA and Topeka State Hospitals. The Presidential Address, "Inkblot perception and personality," presented by Wayne H. Holtzman, was well received.

New officers elected were: Ruth Hubbard, President, Waco VA Hospital; President-Elect, Fillmore Sanford, University of Texas; Treasurer, Ernest Barratt, Texas Christian University. The Secretary, Beatrix Cobb, Texas Technological College, continues in the second of a two-year term.

BEATRIX COBB

Texas Technological College

FIFTH ANNUAL MEETING OF THE SOUTHEASTERN PSYCHOLOGICAL ASSOCIATION

The Southeastern Psychological Association held its fifth annual meeting April 22–25, in St. Augustine, Florida, at the Ponce de Leon Hotel. This was a joint meeting with the Florida Psychological Association and with the Florida Association of School Psychologists. The program consisted of 9 symposia, 74 papers, one workshop, and 3 special meetings. There were 528 persons registered for the meetings. The presidential address, entitled "Seeing in the dark," was presented by Winthrop N. Kellogg. Officers for 1959–1960 are: President, M. C. Langhorne; President-Elect, J. F. Dashiell; Secretary-Treasurer, Susan W. Gray; Members-at-Large of the Executive Committee, Eliot H. Rodnick, Richard W. Husband, and Stanford C. Ericksen. The 1960 meeting will be held in Atlanta, Georgia, at the Biltmore Hotel, March 30–April 2.

George Peabody College for Teachers — SUSAN W. GRAY

THE 1959 MEETING OF THE AMERICAN PHILOSOPHICAL SOCIETY

The Annual General Meeting of the American Philosophical Society was held in the Hall of the Society in Philadelphia on April 23-25, 1959. The sessions were devoted to a commemoration of the centennial of the publication of the *Origin of Species* by Charles Darwin. At 6 meetings there were 15 papers read or addresses delivered about evolution, its pre-Darwinian status, the significance of Darwin's contribution, the current status of evolutionary theory, and the various effects of the doctrine on civilized thought and conduct. At the annual dinner Sir Charles Galton Darwin, the physicist grandson of the evolutionist, gave an anecdotal address about his grandfather, the voyage of the Beagle, and the *Origin of Species.*

The first Karl Spencer Lashley Award "in recognition of useful and significant work in the field of neurobiology" was made to Rafael Lorente de Nó. The award was presented by William H. Taliaferro, and Mrs. Lashley was asked to stand to accept the approbation of the audience.

Of the 23 persons elected to resident membership those most closely related to psychology were Philip Bard, Robert K. Merton and Curt P. Richter. Henry Allen Moe was elected President of the Society, and William J. Robins, who retired as President, was elected to succeed Luther P. Eisenhart as Executive Secretary.

Walter R. Miles and Edwin G. Boring were the only psychologists present at the meetings.

Harvard University

EDWIN G. BORING

THE 1959 ANNUAL MEETING OF THE NATIONAL ACADEMY OF SCIENCES

The National Academy of Sciences held its annual meeting in Washington on April 27-29, 1959. There were symposia on the deep sea and on the atmosphere. Five honorific awards were made, including the Kimber Genetics Award to T. M. Sonneborn of Indiana University. Among a dozen scientific exhibits were the Bell Telephone Laboratories' electronic recognizer of hand-written numbers and the low-temperature techniques for preparing biological specimens, developed by the Massachusetts General Hospital and the Massachusetts Institute of Technology. These latter techniques make possible the fixation of the dark-adapted retina without bleaching when photographed.

Carl Pfaffmann was elected a member of the Academy. Thus the Section of Psychology gained one member after having lost Lashley and Nissen during the year. Of the 22 Section members there were 8 present: F. A. Beach, E. G. Boring, Leonard Carmichael, H. F. Harlow, Heinrich Klüver, W. R. Miles, Neal Miller, and C. P. Richter.

Harvard University

EDWIN G. BORING

Robert Morris Ogden: 1877-1959

Robert Morris Ogden, Professor Emeritus of Psychology at Cornell University, died in his eighty-second year, during the early morning of March 2, 1959, in the Tompkins County Memorial Hospital at Ithaca, New York. He was born in Binghamton, New York, on July 6, 1877, the son of James Sherman and Beulah Maria (Carter) Ogden. His death, which followed an illness of several months, was due to a carcinoma of the pancreas.

Following his elementary and secondary education in the public schools of Binghamton, he entered the Engineering College of Cornell University to pursue his interest in construction and design. He soon discovered, however, that his real interest lay in the field of the liberal arts and, after taking his first course in psychology under Professor E. B. Titchener, that it was in psychology in particular. He accordingly transferred to the College of Arts and Sciences in his sophomore year and was graduated with a B.S. degree in 1901 with a major in psychology, a minor in esthetics, and a research publication to his credit.[1]

Preferring catholicity in his training and not wishing to take two degrees at the same university, Ogden decided to go to Europe for his doctorate. Encouraged by Titchener, he went to Germany in the fall of 1901 to the University of Würzburg to study with Oswald Külpe—with whom Titchener had established a close and lasting friendship during the latter's student days in Wundt's laboratory at Leipzig. Külpe had recently established a new psychological laboratory at Würzburg which was, next to Wundt's laboratory at Leipzig, recognized as the best in Europe. It seemed to be and, as events turned out, it was the best place for Titchener to send this outstanding student, for Ogden arrived there just as the studies by Mayer and Orth and by Marbe, which were basic to the Würzburg school of imageless thought, were published.[2] The full significance of these studies was not, however, recognized at that time and Ogden was given the problem, for his doctoral dissertation, of discovering how the rate of reading meaningful and meaningless materials aloud influenced learning and retention.[3] Though not the busy, bustling laboratory that it later became,[4] loyalties were acquired which led him to return to Würzburg and to take sides when the controversy regarding imageless thought developed.

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[1] R. M. Ogden, A method of mapping retinal circulation by projection, this JOURNAL, 12, 1901, 281-291.

[2] A. Mayer and J. Orth, Zur qualitativen Untersuchung der Association, *Z. Psychol.*, 26, 1901, 1-13; Karle Marbe, *Experimentell-psychologische Untersuchungen über das Urteil: Eine Einleitung in die Logik*, 1901, 1-131.

[3] Ogden, Untersuchunden über den Einfluss der Geschwindigkeit des lauten Lesens auf das Erlernen und Behalten von sinnlosen und sinnvollen Stoffen, *Arch. ges. Psychol.*, 2, 1903, 93-189.

[4] Ogden (Oswald Külpe and the Würzburg School, this JOURNAL, 64, 1951, 11)

Ogden received his Ph.D. degree at Würzburg in 1903. Since academic positions, the only source of employment open to psychologists at that time, were few and far between when he returned to America, he gladly accepted an assistantship in psychology at the University of Missouri with Max F. Meyer. There he spent two happy and profitable years during which, besides gaining experience in teaching, he published two papers,[5] began the translation with Meyer of Adolph Hildebrand's *The Problem of Form in Painting and Sculpture,*[6] and, most important of all, gained his wife.

In the fall of 1905, he accepted the call to the University of Tennessee as an Assistant Professor in the Department of Philosophy and Psychology. His rise there was rapid; in 1907 he was promoted to an associate professorship and in 1909 to a professorship. He remained at Tennessee until the fall of 1914, when he went to Kansas. During his years at Tennessee, he published fourteen articles, the translation that he had begun with Meyer, and an introductory textbook in psychology. He also attended and presented a paper—on the relation between esthetic content and affective experience—at the Sixth International Congress of Psychology which was held in 1909 at Geneva, Switzerland.[7]

While on this trip to Europe, Ogden visited the Würzburg laboratory and participated as an observer in some of the experiments on thought then being conducted there. Since Bühler's introduction, in 1907, of 'imageless thought' as a new element of consciousness, the Würzburg School had come to full bloom. Though previously a bit doubtful about the claims of this School, Ogden reports that, during the course of his observations as a *Versuchsperson,* he "made acquaintance with 'thought' as an undeniable 'presentation' without a sensory, imaginal, or affective 'content' to support it in consciousness."[8] He was thereafter an ardent protagonist of the Würzburg School.

Convinced by the evidence of his own observations, Ogden continued to experiment on imageless thought when he returned to his own laboratory

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writes of this period as follows: "Although I was a student in Würzburg at the time 'thought psychology' was in the brewing stage, and knew, personally, the pioneers, Mayer, Orth, Marbe, Ach, and Watt, I cannot say that the laboratory seemed at the time alive with new discoveries."

[5] Memory and the economy of learning, *Psychol. Bull.*, 1, 1904, 177-184; and The esthetic attitude, *J. Philos. Psychol., sci. Method*, 2, 1903, 408-413.

[6] *Op. cit.*, 1907, 1-141.

[7] Die Beziehung des ästhetischen Verhaltens zum Gefühlsleben, *Proceed. VI Congress Int. de Psychol.*, 1909.

[8] Ogden, this JOURNAL, *op. cit.*, 1951, 12.

at Tennessee. As his results corroborated those at Würzburg, he was prompted, first to write a summary and critique of the work that had been done in the field,[9] and then to write an introductory textbook embodying 'thought,' along with sensation, image, and affection, as a conscious element.[10]

Though the textbook was well planned and clearly written, it was not a success. It appeared at a time—in the spring of 1914, just before the beginning of World War I—when the world in general and psychology in particular were in great flux. Behaviorism and the testing movement were rampant in America and Gestalt theory claimed the little attention that psychologists in Europe could take from their war efforts. The question of 'imageless thought' had not been decided; it had merely been pushed off the stage by the new interests; and Ogden's book, which Külpe warned him was premature and advised him against publishing,[11] went with it. The book was, however, a worthy effort—the capstone of the school of 'imageless thought,' for it is "the only textbook written," as Ogden later observed, "from the angle of the Würzburg School."[12]

Due to the influence of James R. Angell, the leader of the functional school of psychology, who was attracted to him by his articles on 'imageless thought,' Ogden went to the University of Kansas in the fall of 1916 as Professor and Chairman of the Department of Psychology. He continued his researches at Kansas[13] and as a result was called in the fall of 1916, to his Alma Mater, Cornell University, as Professor and Chairman of the Department of Education in the College of Arts and Sciences. He remained at Cornell and lived in Ithaca the rest of his life. He retained the Chairmanship of the Department of Education until 1931, when it was joined administratively with the Department of Rural Education to form a School of Education, and his Professorship in Education until 1939, when the Department of Education in the College of Arts and Sciences was abandoned. He was then transferred to a professorship in the Department of Psychology.

During the spring semester of 1923, Ogden served as Visiting Lecturer in the Graduate School of Education of Harvard University. Following his return to Cornell, he was appointed Dean of the College of Arts and

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[9] Imageless thought: Résumé and critique, *Psychol. Bull.*, 8, 1911, 183-197.

[10] *An Introduction to General Psychology,* 1914, 1-270, esp. 88.

[11] Ogden, this JOURNAL, *op. cit.,* 1951, 12.

[12] *Idem,* 12.

[13] He wrote a short article on "Why education fails to educate," *School & Soc.*, 1, 1915, 312-315, and two long articles which appeared in 1917 in the Titchener and Creighton Commemorative Volumes, respectively.

Sciences, a position he retained for twenty-two years—until his retirement of June 1945.

Ogden had had considerable experience as an administrator before his appointment to the Deanship. He was Supervisor of the Summer Schools of the South, Knoxville, Tennessee, from 1911–1914; Secretary-Treasurer of the American Psychological Association from 1913–1916, and a member of the Council of that Association from 1918–1920; and Chairman of the Cornell Summer Sessions from 1919–1923.

Despite his administrative duties, Ogden maintained his scholarly interest and continued to write and to publish. His numerous papers on sound during the late 'teens and early twenties were subsumed in his book, *Hearing,* which was published in 1924. In the same year, his translation of Kurt Koffka's *Die Grundlagen der psychischen Entwicklung,* appeared under the title *The Growth of the Mind.* This was followed, in 1926, by his book on *Psychology and Education;* in 1928, by his translation of the second and revised edition of Koffka's book; in 1932 (with the collaboration of F. S. Freeman), by the revised edition of *Psychology and Education;* and in 1938, by his book on *The Psychology of Art.* In all, he published four books, one going through a second edition, and three translations. In addition to his books, he produced numerous reviews and papers on systematic subjects, confining them chiefly to art, esthetics, insight, learning, and Gestalt psychology.

Ogden was the first and principal proponent of Gestalt psychology in America. He not only wrote and translated in behalf of the Gestalt movement, but he also, as Dean of the College and Chairman of the Department of Education, brought Gestalt psychologists to his faculty as visiting professors. Koffka was a visiting professor in his Department during the academic year 1924–1925; Köhler, for a series of lectures in 1926 and again, in the Department of Psychology, during the spring semester of 1928; and Lewin, in the School of Education, for two years, 1933–1935.

In addition to his other activities, Ogden was a Coöperating Editor of the *Psychological Bulletin* for twenty-one years, from 1909 to 1929; and of this JOURNAL for thirty-three years, from 1926 until his death. He was an excellent editor, as this writer can well attest from the help and assistance received from him over the years. Recognizing this ability, the President of Cornell gave him the task of editing the long-lost and recently discovered papers of Andrew D. White, the first president of the University.[14] Though retired, Ogden retained an active interest in psychology as the articles and numerous reviews published by him in recent years clearly show.

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[14] *The Diaries of Andrew D. White,* R. M. Ogden, editor, 1959.

In partial recognition of his services to psychology, Ogden was elected to the following positions: president of the Southern Society for Philosophy and Psychology, 1912–1913; vice-president and chairman of Section I (Psychology) of the American Association for the Advancement of Science, 1936; president of the Association of Colleges and Universities of the State of New York, 1938–1939; and president of Division 10 on Esthetics of the American Psychological Association, 1954.[15] He was also elected to membership in the following honorary societies: Phi Beta Kappa, Sigma Xi, Phi Kappa Phi, Phi Delta Kappa, and Psi Chi.

An enumeration of the positions, the articles and books, and the honors received by Ogden does not portray the man. The listing merely indicates his versatility, range of interest, industry, determination, and popularity. The persisting core of his character was gentleness and kindness—strange characteristics for a successful administrator, which he most certainly was. 'Bob' or 'Rob,' as he was affectionately known by his intimate friends and acquaintances, possessed a tremendous capacity for work, as the tale of his accomplishments attests, but with it the rare ability of concealing his industry by never appearing to be hurried or driven. He was a gentle man, always courteous, cordial, and even-tempered; never brusque, blunt, or short. He was free with his time and counsel whenever anyone, friend or stranger, colleague or student, came to him for advice.

Though he was less retiring and aloof than Külpe, he was much like his revered teacher. The resemblance in character is striking to those who knew them both. Like Külpe, Ogden possessed an esthetic personality, and like him, his interest in art (all forms—music, painting, sculpture, architecture, and design) was keen, enlightened, and persistent. It was this characteristic that drew him as a young student to the Engineering College at Cornell to study "construction and design" and impelled him to turn to psychology as that was then being presented by Titchener.

Despite his accomplishments and his position, 'Bob' was modest and unobtrusive. His place in the history of psychology would doubtlessly have been higher had he been more aggressive and self-assertive, but that would have put him out of character and he would not have been held so high in the hearts of his students and his fellow men. The choice between these alternatives was not hard for him to make.

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[15] His presidential address before the S.S.P.P. was on "The relation of psychology to philosophy and education," *Psychol. Rev.*, 20, 1913, 179-193; before Section I, A.A.A.S. was on "Naïve geometry in the psychology of art" (this JOURNAL, 49, 1937, 198-216); and before Division 10, APA, was on "Recollections of Aesthetics," which was, as far as the writer has been able to discover, unpublished.

He is survived by his wife, Nellie Dorsey Ogden, his son Jonathon, two daughters, Mrs. Stewart Hemingway and Mrs. Frederick S. Brown, and eleven grandchildren.

University of Texas — KARL M. DALLENBACH

Karl Friedrich Muenzinger: 1885-1958

After an extended and painful illness—cancer of the spine—Karl F. Muenzinger died at his home in Boulder, Colorado, on Sunday morning, November 23, 1958. He was a devoted teacher, a careful and thorough experimenter in animal behavior, and an enthusiastic champion of the proposition that the proper business of psychology is all of behavior—the magic and mystery of man's appreciation of art no less than the tendency of *mus norvegious albinus* to display 'vicarious trial and error' (*VTE*) behavior in a discriminative problem.

Born April 28, 1885, at Speyer, Germany, he attended primary and secondary schools in both Germany and Switzerland. In 1905 he came to the United States where he enrolled as an undergraduate at the University of Texas. His German heritage was throughout his life both a source of joy and of harassment and bitterness. While teaching at the University of Texas, when he was also working toward his Ph.D., he was forced at the beginning of World War I to resign from the faculty because of his German origin. After finally obtaining his degree from the University of Chicago in 1918, he again found that, as a German, all academic doors were shut to him, and for a number of years he worked as an industrial psychologist for the Lakeside Press in Chicago. Twenty-five years later his attachments to Germany again became a source of distress to him, for the ugly image of Germany that Hitler created caused Muenzinger genuine personal grief as he both knew and loved the image of Germany as portrayed by her poetry and her *Lieder*.

A breakdown in health (tuberculosis) brought Muenzinger to Colorado, and in 1923 he joined the faculty at the University of Colorado in Boulder. In 1925 he married Dr. Florence Weaver whose gentleness and devotion helped so much to make their Boulder life a pleasant thing to see—a culturally rich family life which they shared with their three children, and later with ten grandchildren. Appointed Assistant Professor in 1924, he was promoted to the associate professorship in 1928, and to the professorship in 1938. From 1946 to 1953 he served as chairman of the department, and then retired to become Professor Emeritus. He continued in active research and consultation until shortly before his death.

Despite an allergy to rats, and although forced to limit his activities on account of his arrested tuberculosis, Muenzinger contributed approximately forty research papers, an introductory text-book, and numerous abstracts and reviews. Most of his work lay in the fields of animal learning, motivation, and color vision. While his concern with the rat derived from his interest in learning theory and systematic psychology, his work is outstanding primarily for its empirical findings rather than for the theoretical consequences he drew. This limitation was not due to Muenzinger's ineptitude with theory, but to his admirable self-restraint and his respect and love for the discipline of science. He found pretty much what he wanted in the already available theories and systems which in the 1920s and 1930s were in good supply. He was enough 'science oriented'—rather than 'Muenzinger oriented'—to resist the too-common compulsion to erect a 'personal' theory. He was a great admirer of Edward Tolman and he frequently sought to integrate his own experimental results within Tolman's framework. Within this framework, Muenzinger's work is of significant importance. A good illustration of this can be seen in his work with *VTE*. The observation of *VTE* and much of its experimental development stem from Muenzinger's laboratories; the theoretical exploitation of *VTE* must be primarily ascribed to Tolman.

There was perhaps another facet to his work which inhibited his development of a personally worked out 'system.' He was too thorough in his research to engage in the hit-and-run style (however brilliant the occasional hits) of experimental work which characterizes many a system builder. Instead of being content to 'make his point' with one superficially adequate experiment and then to go on to fill another chink in an ever-expanding, imperialist program, Muenzinger stayed with a problem, worried it, re-examined it, replicated it, until he felt satisfied that the original question he had asked of his research was on the way to an answer. His series of studies of the effects of reward, emphasis, and punishment on learning provides an excellent illustration of this thoroughness.

His research work dealt with important questions. It was original. It was sound. And it was carefully carried out. For this reason, primarily, one can legitimately speak of Muenzinger as an experimenter in systematic psychology, and as such his studies have had considerable influence on the development of modern systematic psychology and have received general acclaim. His colleagues' appreciation of his experimental contributions is evidenced by his election to the Society of Experimental Psychologists in 1938. His own University recognized his experimental eminence by appointing him Faculty Research Lecturer in 1946.

As a teacher Muenzinger was more than conscientious. He loved teaching and he loved his students, showing a loyalty and continued interest in their development far beyond the call of duty. His students recognized this devotion and appreciated it, and very few men in psychology have left behind them more devoted friends among their students. Shortly after his death, the Karl Muenzinger Award was established through subscriptions by his students and friends. Every June, a prize is to be awarded to a graduating senior who has majored in psychology. No tribute more appropriate than this and more consonant with Muenzinger's concerns could have been devised.

His concern about behavior and misbehavior was not limited to the rat. He was a founding member of the Society for the Psychological Study of Social Issues and retained his interests in social issues, academic freedom, and the wider concerns of mankind throughout his life. A lover of music and literature, he constantly referred to Goethe, Schiller, and Thomas Mann in his psychological discussions and lectures. During his final illness he translated German poetry and wrote several poems of his own.

Muenzinger will be remembered by his many friends and colleagues as a dedicated scientist, an experimental psychologist, and a gentle and noble man.

University of California, Berkeley DAVID KRECH

BOOK REVIEWS

By George L. Kreezer, Washington University

Aging and Human Skill, By A. T. Welford, London, Oxford University Press, 1958. Pp. v, 289. $4.00.

This volume represents an extension and expansion of Dr. Welford's book *Skill and Age* published in 1951. In addition to bringing together the extensive experimental work of the Nuffield Research Unit into Problems of Aging at the Psychological Laboratory, Cambridge, conducted between 1946 and 1956, when the Unit was disbanded, Dr. Welford has organized the results into a theoretical framework. According to the author, all skills consist essentially of the building of an organized and coördinated activity in relation to an object or a situation and thus involve the whole chain of sensory, central, and motor mechanisms which underlie performance. Skills are learned, and are serial in the sense that within the overall pattern of skill many different processes or actions are ordered in a temporal sequence. From this analysis, Dr. Welford proceeds to experiments designed to determine which aspects of skill are important in the alterations of performance with age. One chapter is devoted to methods of studying age-changes, in which the author concludes that age-studies with a practical aim must deal with a representative sample of the population to whom the practical issues relate, whereas studies dealing with theoretical problems must demonstrate that the old and young subjects tested are comparable. It is clear that in our present state of knowledge we do not know how to define 'comparability' of the young and old subjects, except in a very loose sense, such as level of education, occupation, and family status. Variables such as intelligence are of questionable validity. Welford recognizes the desirability of making successive measurements on the same subject, but points out the practical difficulties of time required to obtain answers, motivation of the subjects, and practice effects of using the same tests repeatedly. It is unfortunate that after setting out criteria for studies of aging, Dr. Welford does not offer evidence about how well the subjects included in his studies met the criteria. In fact, the reader must consult the 1951 volume for any description of his subjects.

Since any response requires the organization of perceived data, the selection of the appropriate response and the motor effector system, Dr. Welford and his co-workers have devised many ingenious experiments to break down complex performances into their component parts. Numerous experiments lead to the conclusion that the main locus of slowing with age in sensori-motor performance lies not in the speed of movement but in the time taken by central processes initiating, shaping, and monitoring movement. It is theorized that with increasing age the ratio between random disturbances in the nervous system (noise level) and the effects of sensory stimulation diminishes so that older subjects take longer to integrate incoming signals, thus increasing their reaction-time. While this is an attractive theory, only tenuous physiological data exist at the present time to favor it.

Since age-decrements are greatest in serial processes, where perception and selection of responses must be carried on simultaneously with previously chosen responses, the author concludes that the mechanisms for short term storage of data suffers a primary impairment with age. This is a legitimate presumption which requires further study to identify the physiological mechanisms involved.

Many ingenious experiments bring out the important discovery that decrements in performance by older subjects produced by increasing complexity of the task are not simply additive. For example, the age decrement in performance of tasks involving symbolic and spatial translation are much greater than either condition alone. From a practical point of view, the removal of one source of complexity from among many might cause a disproportionate improvement of performance in the older subject. It is also clear that tasks requiring complex responses are influenced much less by age than tasks involving complex stimuli.

In studies on problem solving, the limitations for the older subjects lie in the gathering and holding in mind of the data on which insight depends. The small age-reductions in speed of learning and in adaptability observed are attributed to impairments in mechanisms for short term memory.

The studies on problem-solving, learning, and memory depend more on the intuitive judgments of the investigators than the studies on perception and psychomotor responses. With the current developments in learning theory, it is surprising that the results were not analyzed in terms of parameters of the learning equation.

Although the volume gives many illustrative charts and tables, it is unfortunate that the number and nature of the subjects tested is omitted. It must be assumed that statistical tests of significance were applied, since all results depend on the differences between the means of age-groups.

On the basis of psychological tests, Dr. Welford has shown the primary importance of central processes in determining age-changes in performance. The problem must be shared with the neurologist. One might ask to what extent is the activity of the reticular system of the brain stem involved in age-changes in performance.

This book represents a major contribution to gerontology that should be on the bookshelf of every serious student of aging.

N. W. SHOCK

Baltimore City Hospitals

A Psychological Study of Typography. By CYRIL BURT. Cambridge (England): Cambridge University Press, 1959. Pp. xix, 68. $3.00.

This report is concerned mainly with the legibility of print and esthetic preferences for type faces. The introduction by Stanley Morison discusses the typographical factors involved in legibility and the alleged importance of reader preferences for specific type faces. This is followed by the author's statement on printing as a medium of communication. The greater part of the report deals with studies of legibility by the author and others. The appendix describes the characteristics of the commmoner type faces and gives illustrations of 20 of them.

The principal techniques employed for studying legibility were speed and accuracy of reading. Perceptibility at a distance, recording of eye movements, and frequency of blinking were also employed but little use was made of them. Esthetic preferences were employed in an attempt to classify type faces. Children and adults were subjects on various experiments. Where stated, the number of subjects was 20 to 30 in a group.

The emphases in the conclusions included the following: (1) For children, the different type faces were significantly different in variance but not from one face to the next in the final ranking. Type faces in common use were equally legible for adults. (2) For normal children, little advantage was found for boldness of print. (3) Typographical standards in terms of type size, leading and line width were *computed* by year-steps for children of different ages. For adults, 10-point type was best when set solid (no line width given). (4) One- or two-points of leading were suggested for 8-, 9-, and 10-point type. (5) Line widths with 10-point type for adults can range from 20 to 33 ems, or 3½ to 5½ in. (6) Narrow margins are apt to produce visual fatigue. (7) It is recognized that the above typographical factors must be coördinated, but no hint as to how this is to be done is given. (8) The esthetic ranking of type faces is supposed to yield data which indicate the best type faces to use for printing scientific materials, poetry, etc.

The critical reader should have many reservations concerning the validity of the conclusions presented in this book. The following are samples of those noted by the reviewer: (1) Morison's introductory comments are biased in the direction of the traditional views held by printer and type designer. (2) Speed of oral reading (p. 5) tends to become an invalid measure of speed of perception in reading. (3) The statement that short testing time will give misleading data (p. 5) is in error and contrary to published research findings. (4) A large part of the discussion and conclusions fail to cite the supporting data. They are almost casual in tone. Perhaps this is because the author had no satisfactory data at hand. (5) Although the book claims to take into account literature in the field, the author has either failed to read or decided to ignore results given in scores of American studies. Consequently many of the conclusions are either in error or are entirely inadequate. A striking example is the statement that narrow margins produce visual fatigue and that the eye of a younger reader is apt to swing right off the page. (6) In much of the discussion the reader is not informed whether the data refer to children or adults. (7) The methodology in classifying type faces according to preferences is suspect. A minimum of flimsy data is given maximal amounts of statistical treatment. Apparently atmosphere value is confused with esthetic preferences. (8) Heavy emphasis is placed upon introspective reports which have little if any validity. (9) It is surprising to find this author implying causal relations from co-variation.

The reader who is unacquainted with the field of legibility should not accept statements in this book because the author has high prestige. Much of the included material is neither accurate nor authoritative. The discussion contains many abstruse arguments, flimsy theorizing, actual errors of statement, and conclusions without any substantial basis in experimental data. Unfortunately the book merely adds confusion to the field of scientific typography rather than clarifying points at issue.

University of Minnesota

Miles A. Tinker

Psychologie du temps. By Paul Fraisse. Paris, Presses Universitaires de France, 1957. Pp. 326. 1200 F.

Professor Fraisse has contributed a welcome book, particularly welcome because the subject of his book has been both troublesome and evasive in the history of psychology. He has reorganized, in a sense, the psychology of time so that his consideration of several subject matters can be more logical and much more cohesive than has been characteristic of the treatment of time in the psychologies of the past.

When the structuralists of psychology proclaimed time or protensity as a proper attribute of sensation in the 19th century, they showed due regard for the fact that sensations have duration and that the durative aspect must be taken into account. A little more than duration was involved when Mach wrote about time as a sensation in and of itself, transcending the sensations associated with the individual modalities. What more there was than duration became more obvious when some of the early act psychologists added *time-form* to *space-form*. Still the concern was primarily with time as a dimension in sensation and perception. Time is clearly relevant, however, to more aspects of human behavior than these two, and Fraisse has covered adequately the many ways in which time must be considered.

There are three main divisions in this book, each corresponding to a major aspect of time in psychology. The first has to do with conditioning to or adaptation to time and, in particular, externally imposed temporal structures and rhythmic structures. The second main division has to do with temporal perception as such: the perception of order, of duration, the discrimination between the durative and the instantaneous, and the discrimination of the simultaneous from the successive. This second part contains most of what we have come to know as the classical work on time perception. The third main division, called "the matrix of time," considers the present position of a person in time relative to his past and to his future. This novel setting for a consideration of a further aspect of time is used as a vehicle for the presentation of work on memory and of the prediction of future, and also the generation of the concept of time, particularly by the human species.

The author introduces each particular subject by declaring its relevance to everyday life and by foreshadowing the work to be considered through a philosophically-oriented set of assumptions. The chapter on the biological effects of external rhythms imposed by the rising and setting of the sun, etc., for example, begins with a naturalistic consideration of the ontogeny of certain behaviors in lower animals. There follows a review of observations from biological experiments as well as conclusions from philosophical works. If there is a general criticism to be made of the book, it can be made from the point of view of a behavioristic psychology that is perhaps more characteristic of the United States than of France. As the reader goes along gaining information about the several aspects of time, he is not warned nor apprised of the source of the information being presented. Sometimes it results from a philosopher's speculation, at other times from some experimental results, and at still others from the author's own notions, some of which have experimental support and others of which have not been thus tested. This may not be so much a criticism of this book or of Professor Fraisse as it is of the state of development of the psychology of time. There are some areas, which have not been investigated, where the only relevant information comes from the speculative philosophy of a former day. These are the very areas, however, that the psychological reader can discover as needing investigation and thus even those parts of Fraisse's treatment that do not contain experimental data will have great heuristic value.

For the specialist, perhaps the psychophysicist who is interested in temporal perception, this book is rich in source material that has been brought together under relatively new headings. For example, in older sources one may find temporal problems having to do with vision under *Vision*, while those that have to do with hearing or touch are to be found subsumed under those particular sense modalities. For Fraisse, the important dimensions for categorizing experimental results are to be found in his schema for treating time, and are not bound to sense modalities or other less important criteria.

In recent years one has noted an increase in the number of psychological investigations that are concerned with time or with change. The publication of Fraisse's book is very timely for this development because it is a very thorough coverage of the psychology of time and represents a bringing together of works that have not heretofore been considered directly relevant to the psychology of time.

Central Institute for the Deaf
St. Louis, Missouri

IRA J. HIRSH

Talent and Society. By DAVID C. MCCLELLAND, ALFRED L. BALDWIN, URIE BRONFENBRENNER, and FRED L. STRODTBECK. Princeton, Van Nostrand, 1958. Pp. 275. $3.75.

The constant search for the competence and the talent presumed necessary for leadership has been intensified by the force of recent world events. Some searchers hope for better ways to identify the potential leader or inventor or benefactor. Others like John Gardner, hope to increase the supply of talent by more than early identification—by broadening the opportunities for training and by better and sounder education. It has been known for some time and shown by Terman in fact, that more than what the intelligence test measures is involved in real talent. In the present volume talent is defined as the exercise of an ability in a social setting, involving some activity of social consequence, and the talents discussed are those not ordinarily classified as abilities, such as values, motives, social skills, and occupational status. Baldwin's discussion of what ability really means concentrates on cognitively guided behavior —behavior in which motivation and action are independent of each other and action and are, therefore, under volitional control. Behavior that is cognitively guided is important for intellectual attainment, but it is not important according to McClelland, for social success. Bronfenbrenner, with John Harding and Mary Gallwey, convinced that much of the non intellective component in success is related to a social empathy, concerns himself with measuring the sensitivity to the feelings and needs of others. The ability to predict how others will rate and be rated was studied in 72 Cornell undergraduates who come to dinner at the Statler Inn at Cornell and engaged in discussions.

Strodtbeck's study of over- and under-achievers among boys of Jewish and southern Italian extraction in New Haven schools goes beyond the analyses of sociological factors of expected relevance to the exposition of power relations within the family (father : mother : son) and shows how items from his V-scale (for values) can predict school achievement from such power relations.

A contribution to the criterion problem of defining success is made by John Kaltenback and David McClelland in their study of what is involved in being rated a success if you live in a town small enough so that there is a good chance you will be known. Community service comes first, then income or occupational status, and then education and occupational level. They report that individuals with high achievement motivation tend to engage in the sort of activities that will lead their fellows to judge them as successful.

This volume does not solve the problem of talent and society but it contributes some pertinent discussion, some challenging research leads and some provisional conclusions. One of the most intriguing of Bronfenbrenner's findings is that related to the different social sensitivities of men and women. Women who are perceptive toward their own sex showed a tendency to be insensitive toward men. How much anecdotal evidence was collected on this point before psychologists confirmed it!

St. Louis University WALTER L. WILKINS

The Guidance Function in Education. By PERCIVAL W. HUTSON. New York, Appleton-Century-Crofts, Inc., 1958. Pp. xvii, 680. $6.00.

A different approach to a familiar subject is always welcome and eagerly sought by a reader of a new book. The writer of this book covers the areas of guidance services in much the same manner that other writers in the field do when discussing the various phases of the guidance program. A quick glance at this book does not reveal anything 'new' concerning the broad general areas that are covered. On closer scrutiny a significant difference noted in this book is a greater reference to the works of researchers and writers of a number of years ago. The writer was for twenty-four years the reviewer of guidance articles for a national education journal and has included in his text much of what was written about guidance from the early 1930s. In a sense this reference to the 'old' is refreshing because it shows that not all that is known about guidance was recently discovered. Donald G. Paterson criticized his 'younger colleagues' several years ago for what seems to be a lack of "an intimate knowledge of the major contributions made by the pioneers." He added that the contributions of writers of the period prior to 1920 and those later in the 1920s and 1930s are oftentimes overlooked. The author of *The Guidance Function in Education* gives considerable attention to the 'pioneers'—much more than do many other writers of books on guidance.

More attention is given in this book to the social setting of guidance services than to the psychological implications. It is not possible to overlook psychology when speaking of guidance but the psychologist who is looking for a source which is strongly oriented to his field will not find this book as fruitful as some other books that have been written in recent years. This book will, however, give an individual a good overview of guidance functions from the historical, philosophical, and practical point of view. In the area of counseling, which is a guidance function quite close to the thinking of psychologists, there is a description of how it fits into the total guidance program of a school.

In a practical sense guidance services in schools today are still in their infancy and not enough resources are being provided to make available the kind of guidance services that most psychologists and guidance workers would want. Reading this book helps a person realize that guidance is a new aspect of the total educational program and that it takes time to attain acceptance by the more entrenched elements in the field of education.

Washington University O. T. RICHARDSON

Objective Approaches to Personality Assessment. Edited by BERNARD M. BASS and IRWIN A. BERG. Princeton, N.J., D. Van Nostrand, 1959. Pp. x, 224. $4.95.

A dozen authorities in the clinical-counseling field, who constituted the Louisiana State University Symposium on Objective Personality Assessment, discuss in this book some of the current trends in the field of personality assessment. The authors are: Robert Watson, Donald Super, Raymond Cattell, Louis McQuitty, Irwin Berg, Allen Edwards, Wayne Holtzman, Bernard Bass, William Hunt, Starke Hathaway, James G. Miller, and Harold Pepinsky. The topics range from Berg's "The Unimportance of Test-Item Content" to Holtzman's "Objective Scoring of Projective Techniques."

The book does not add much to a definition of the word 'objective,' nor are the characterizations of the advantages of objective techniques more than a rehash of often repeated statements. What the book does accomplish is to provide clear expositions of some new ideas about tests and testing that have been appearing in the periodical literature. Berg, for example, goes into considerable detail about his "deviation hypothesis" which he has teased journal readers with for the past two years, and he presents an array of arguments to support his faith in a straight empirical analysis of responses without regard to the content of the test items that elicited the responses. He believes that the maladjusted—or in vocational context, the malassigned—tend to give atypical or deviant responses, that they answer most any type of material differently from the modal responses of the normative 'keyed' group.

Holtzman gives a scholarly review of various attempts to devise a reliable and valid method for scoring so-called projective personality techniques. He describes his own research in which he has developed an ink-blot technique which seeks to combine many of the clinically desirable features of ink-blot testing with traditional (and in his case, sophisticated) psychometric features. He uses a large number of blots and restricts his respondents to one answer per blot and is currently putting in acceptable order a carefully defined and delineated scoring system.

The reader will also be impressed with Edwards' discussion of his efforts to rule out the biasing effects of social desirability of specific test items.

There seems to be no general methodology evolving from the symposium, nor any concensus as to what the new conceptualizations in the area will be like. The book is interesting, and each chapter is scholarly, but, despite Pepinsky's efforts to whip up a general summary, the book remains a set of independent papers.

University of Texas

AUSTIN E. GRIGG

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A SYSTEM OF COLOR-PREFERENCES

By J. P. GUILFORD, University of Southern California, and
PATRICIA C. SMITH, Cornell University

Some years ago the authors were engaged in a rather systematic and comprehensive study of preference for color, in which more than 300 different color specimens were used. Data from this source have been utilized for different purposes in publications since that time, but a unified report has not been made.[1]

Among other reasons for publication, the authors believe that the results, systematically and fully presented, can provide a kind of basis for norms with regard to the color-preferences of young, adult men and women in North American culture. The authors fully recognize the limitations of the data, which were obtained from individuals originating mostly from one state (Nebraska) in a limited time of about two years. The results cannot, therefore, be proposed as representative of the general population. The authors had hoped to extend the sampling to other parts of the country and to other times, but the opportunities for doing so have not arisen and seem unlikely to arise in the foreseeable future.

The results presented, then, may serve as starting points or as a frame of reference for future studies of color-preferences. One thing that can be said of the results is that they possess a very high degree of internal consistency, and hence of reliability, for the population that they do represent. In addition to whatever norm-value they possess, they demonstrate a high degree of predictability of preference-values from knowledge of the attributes of color, they bring out some general principles of color-preferences, and they incidentally indicate some systematic sex-differences.

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* Received for publication January 6, 1959.

[1] J. P. Guilford, A study in psychodynamics, *Psychometrika*, 4, 1939, 1023; There is system in color preferences, *J. opt. Soc. Amer.*, 30, 1940, 355-359; System in color preferences, *J. Soc. Motion Pic. Eng.*, 52, 1949, 197-210.

Background. Prior to this investigation, it had been recognized that studies of color-preferences had been severely limited and were generally not reproducible. The chief source of limitation was the very small number of color-specimens used. Interest was confined to perhaps a half-dozen colors, usually of relatively high saturation. There had been no attempt to sample more extensively from the whole range of discriminable colors. Experiments were not reproducible because there was no specification of color-stimuli in terms that would enable other investigators to use the same colors.

The first step in the direction of a larger sampling of colors and in the direction of color-specification was taken by the senior author.[2] Using 40 color-specimens, each calibrated for hue, brightness, and saturation, he found that the average affective value of a color could be predicted to a large extent from knowledge of its position on these three dimensions. Affective value was apparently a periodic function of hue and was positively correlated with both brightness and saturation. Not much more could be said regarding the functional relationships because of the inadequate number of color-samples. Hence, a new study involving a much more liberal sampling was initiated.

MATERIALS AND PROCEDURE

Color-samples. It is commonly known that the number of discriminable colors is enormous. Of all the systems that pretend to represent the whole range of color-variations, that of the *Munsell Book of Color* offers the best opportunities for quantitative specifications along the lines of the attributes familiar to psychologists.[3] The authors chose to use specifications of colors in terms of their phenomenology rather than in terms of stimulus-variables for two reasons. One is the assumption that it is the appearance of the color that is the determiner of affective reaction, although the psychophysical problem admittedly deserves attention. The other reason is that colors between the hues of violet and red in the color-circle are more readily specified in phenomenal terms.

The abridged edition of the *Munsell Book of Color* contains more than 600 different colors. It was the authors' objective to obtain a fully representative sampling of colors from this set.[4] Of the 20 pages, each representing a different hue, alternate pages were selected, except in the region of green and blue, where colors of intermediate hues were also chosen. The reason for this exception was that in the senior author's previous study it had been found that affective value changed as a function of hue in that region. On each chosen page of the *Book of Color* we selected specimens nearest to brightness-levels (B) 2, 4, 6, and 8 (Munsell value). All levels of saturation (Munsell chroma) were included. Most of the color-specimens that we could find among the available materials actually departed slightly from the Munsell levels just indicated, but they were nevertheless fairly representative of the color solid. From earlier results during the collection of data, it became apparent that a very intensive sampling was needed in the series of achromatic colors, hence 21 samples of zero saturation were used. With the 295 chromatic specimens, the total number came to 316.

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[2] Guilford, The affective value of color as a function of hue, tint, and chroma, *J. exp. Psychol.*, 17, 1934, 342-370.

[3] *Munsell Book of Color*, Baltimore: Munsell Color Company, 1929.

[4] Except for a very few colors that were purchased from the Munsell Color Company, the sources of color were the Milton-Bradley series, the All-Color Company series, and the Bauman color-series obtained from Paul A. Struck, 415 Lexington Ave., New York).

For use in the experiments on affective-judgment, the colored papers were cut into 2-in. squares and glued to the centers of 4-in. squares of heavy cardboard, the surface of which was gray with a Munsell value of 5. The Munsell specifications of all the color stimuli were determined by averaging the judgments of 20 *O*s who compared them with samples in the Munsell book. These observations were made under conditions of background and of illumination identical with those under which affective judgments were given later.

Affective judgments. The color-stimuli were exposed to the *O*s within an enclosure, the entire inner surface of which was lined with the same gray cardboard on which the color-samples appeared. The enclosure was constructed on a table, of dimensions 30 × 48 in., and was 24 in. high. One end had an opening at which the *O* sat, his head being adjusted in position by means of a headrest. At the center of the other end was an opening just less than 4 in. square, within which each color-stimulus was exposed by resting its card in a convenient slot.

The illumination within the enclosure consisted of the direct, diffused light from four 60-w. frosted bluish Mazda bulbs, which were placed two on either side of *O*'s opening in the enclosure and shielded from his view. Each color-stimulus was exposed for 5 sec. after a preliminary 'Ready' signal.

Instructions. The essential part of the instructions reads:

> You are to judge the pleasantness of the color presented, being careful to judge it as a color. Do not think of it in connection with any object in particular, but rather take it just as a sensation. Try to make the judgment before the color is taken away. Your report of pleasant (*P*) or unpleasant (*U*) is to be given in terms of the following scale (the two extremes, 0 and 10, will in all probability not be used in this series):
>
> 0 = most unpleasant imaginable
> 1 = extremely unpleasant
> 2 = very unpleasant
> 3 = moderately unpleasant
> 4 = mildly unpleasant
> 5 = indifferent; neither *P* nor *U*
> 6 = slightly pleasant
> 7 = moderately pleasant
> 8 = very pleasant
> 9 = extremely pleasant
> 10 = most pleasant imaginable
>
> (The two most extreme categories were included as anchors. They were used on very rare occasions.

O judged about 100 colors on each day of observations, with rest-pauses after sets of 25. The order of presentation was partly random and partly planned. The planned aspect consisted of avoiding both extreme and minimal changes in hue, brightness, or saturation, also contrasts in probable affective value insofar as this could be guessed in advance. Every *O* judged the colors two times, on different days, the order on the second occasion being the complete reversal of that during the first. We thus had the opportunity for determining the degree of personal stability of ratings as well as inter-rater correlations for the purpose of estimating reliability of judgments.

It was planned to have the judgments of affective value (*A-V*) of all the color-stimuli made by 20 men and 20 women from among students volunteering to serve.

Things did not, however, work out that way. Actually, the study was started with 211 color-stimulus, including eight achromatic colors.[5] After completing the preliminary experiments, it was realized that there were serious gaps in the data, which would make difficult the establishment of quantitative functional relationships. An additional set of 114 chromatic colors was therefore selected to fill the gaps and observations were obtained from new groups of *O*s.[6] Ten color-stimuli were common to the two sets, to provide some information as to comparability of the judges in the two series of observations. After 10 men had judged the second set of stimuli, it was realized that additional samples of achromatic colors at the lower region of the brightness-scale were also needed in order to explore that section more intensively, hence 13 new achromatic stimuli were added.

It should be stated that all *O*s were tested for normality of color-vision before being accepted.

RESULTS

Reliability of the judgments. Several estimates were made of reliability of ratings of *A-V*. Correlations between first- and second-trial ratings of single *O*s ranged from 0.62 to 0.81 for the men and from 0.53 to 0.84 for the women. Raising these values by applying the Spearman-Brown formula to estimate what reliabilities should be expected for averages of 20 *O*s yields coefficients of 0.97 to 0.99. Averages of ratings were obtained by computing medians because of some irregularities in the form of distributions. Correlating the median *A-V*s from two randomly chosen sets of *O*s and applying the Spearman-Brown formula, we find reliabilities of 0.94 for the men and 0.93 for the women. For the 10 color-stimuli common to the early and late observations, the correlations between the two sets of median values were 0.86 and 0.91, for the men and women, respectively. Indirect evidence that the results from the early and late observations were generally comparable comes from two sources. One is that the general levels of ratings in the two sets were similar and the other is that the two sets of data seem to fit the same derived functional relationships.

Affective values of achromatic colors. The relation of *A-V* to degree of brightness of color in the achromatic series is of considerable importance, primarily because this series represents the points of zero saturation for colors of all hues and all levels of brightness. The *A-V*s of the achromatic colors thus had an important bearing upon determination of the functions relating *A-V* to saturation and indirectly upon determination of other relationships as well, as will be clear when we describe the process of fitting curves to the data.

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[5] For assistance in obtaining these early observations we are very much indebted to Mrs. Ada P. Jorgensen. These data were obtained during the autumn and winter of 1936-37.

[6] These later observations were obtained by the junior author, who also collaborated in the computation of results. The ratings were obtained during the spring and fall of 1938.

In the prior study referred to, it was concluded that there is a positive relationship between *A-V* and brightness of colors in general.[7] In that study, however, it was not determined either that this applied to the achromatic series or that the relationship is a linear one. In the early results from this investigation, it was concluded that the relation of *A-V* to brightness of achromatic colors is curvilinear, with *A-V* increasing with negative acceleration toward the white end of the scale.[8] There was a slight indication of an upturn at the lower end of the scale, but with only eight stimulus-values involved, the idea of a reversal in trend could not be accepted.

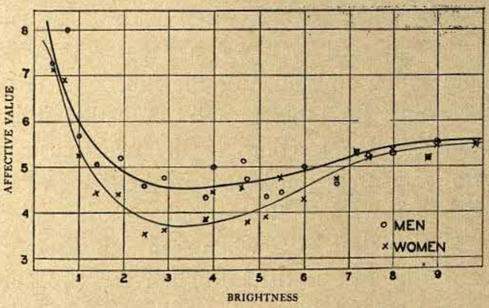

FIG. 1. AVERAGE AFFECTIVE VALUES RELATED TO BRIGHTNESS OF ACHROMATIC COLORS, AND THEIR CURVILINEAR REGRESSIONS

With a much greater sampling of achromatic stimuli, which involved 21 brightness-values, the shape of the regression is much more clearly determined, as shown in Fig. 1. The curves were drawn entirely from inspection of the points. The goodness of fit of the data to the curves is indicated by correlations of 0.87 for the men and 0.94 for the women between the 21 observed and expeced values.

First to be noted is the fact that most of the achromatic colors, from Munsell values 2 through 8, are rated slightly on the unpleasant side.[9]

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[7] Guilford, *op. cit.*, 1934, 360.

[8] Guilford, *op. cit.*, 1939, 13.

[9] In connection with Fig. 1, and all subsequent figures, it should be noted that the indifference-level is at 5.5. The reason is that a rating of 5 was arbitrarily assumed to occupy the range 5.0 through 5.99 on the numerical scale, with other rating categories defined accordingly.

It should be remembered that the ratings were obtained in a larger setting that involved the rating of numerous chromatic colors. This should be a desirable condition, for under these circumstances the adaptation-level established would probably be as unbiased as it could be. If *O*s had rated achromatic stimuli only, they might have given a larger proportion of judgments on the pleasant side.

The second noteworthy fact is that colors in the region of black were rated more pleasant than those in the region of white. This conclusion is largely determined by the ratings for two color-stimuli, which were somewhat unusual in one respect, *i.e.* texture. All other samples were of colored paper of similar texture. The blackest black was actually a sample of velvet cloth and the one next to it was a paper with a slightly velvety texture. One would expect an upturn of the functional relationship from the trends of the points for other samples. Whether it is, however, as extreme as indicated by the reactions to the two unusual specimens is hard to say. There is apparent consistency in the shape of the regressions in the region of dark grays and blacks. Perhaps, after all, differences in texture were not so very clear to *O* when seen at a distance of 4 ft.

The third noteworthy fact is that although the two curves, for men and for women, are similar in shape throughout, women have a general tendency toward lower ratings, a difference that is most pronounced in the region of lowest *A-V*s for both sexes. The smaller variance in the average ratings by men may mean that they were less sensitive in their affective reactions to changes in brightness. From their generally higher *A-V*s, one might also conclude that men are more tolerant of achromatic colors, but they showed a similar bias toward higher ratings of chromatic colors as well.

Affective value and hue. There has always been greatest interest in *A-V*s in relation to hue. In the current setting, the question concerning the functional relationship of *A-V* to hue must be restricted to instances in which brightness and saturation are held constant. We must recognize, however, that the function may change due to interactions of hue with the other two attributes of color. It is necessary to ask the question, therefore, for all combinations of brightness and saturation.

In Fig. 2 are shown the relationships that were concluded to exist between *A-V* and hue when saturation is held constant at a Munsell rating of 6 and when the *O*s are women. A functional relationship is presented for *B* 3 through 8 on the Munsell scale. The hues on the base line are spaced in accordance with the Munsell scaling. Points representing actual medians of ratings of *A-V* are not shown in Fig. 2, in order to present a cleaner and more simplified picture.

Some explanation is in order as to how the smoothed curves in Fig. 2 were obtained. Especially to be accounted for are the curves at B 3, 5, and 7, for which, as it was stated earlier, no color-samples had been selected for use in the investigation. The smoothing process was not confined to plots relating *A-V* to hue. There were also plots relating *A-V* to brightness, with hue and saturation held constant, and to saturation, with hue and brightness held constant. Examples of such smoothings may be seen in Figs. 3 and 4. The most important step, however, was to ensure consistency of relationship of *A-V* to all three attributes simultaneously, so that on any one plot a color having a given hue combined with a given brightness and a given saturation would have the same *A-V*. This meant considerable going back and forth from one plot to another and a number of iterations was needed. With the continuities thus achieved, it was possible to draw the interpolated functions seen in Fig. 2, as well as those in Figs. 3 and 4.

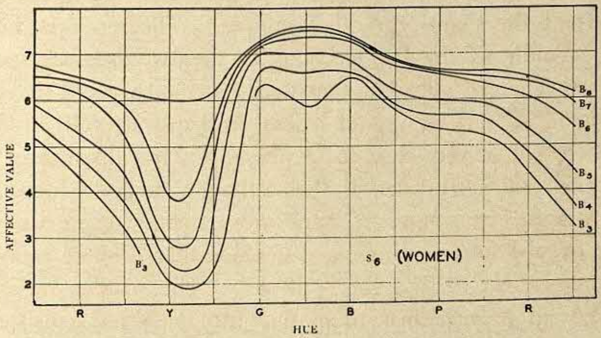

FIG. 2. CURVILINEAR REGRESSION-LINES RELATING AFFECTIVE VALUE TO HUE (Saturation constant at Munsell level S6; brightness at several selected levels.)

The curves in Fig. 2 are essentially similar in form to those obtained in the earlier investigation.[10] The maximal *A-V* comes in the region of green to blue, with a slight dip between those two hues, but only at lower levels of brightness. The minimal *A-V* comes on the green side of yellow and there is a slight secondary dip in the region of purple. The general levels of the curves are in direct proportion to the brightness-levels of the colors, with no inversions in this particular chart. There are a few inversions elsewhere among such curves.

It is of some interest to ask whether these relationships of *A-V* to hue can be described, as in the earlier investigation, in terms of periodic algebraic functions involving strong first and third components. Using some of our data, Stamm has found that the first and *second* harmonics account for most of the covariations with hue.[11] The difference between his finding and the earlier one is probably accounted for by the fact that the earlier study utilized Troland's *jnd*-scaling of hue, whereas Stamm used the Munsell scaling. The two scalings are not the same.

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[10] Guilford, *op. cit.*, 1934, 356.

Affective value in relation to brightness and saturation. Fig. 3 is shown as a sample of the smoothed relationships of *A-V* to brightness for men's judgments. At different levels of saturation (S), the regressions are very similar. They are usually monotonic, and the greater the saturation, the higher is the level of the curve.

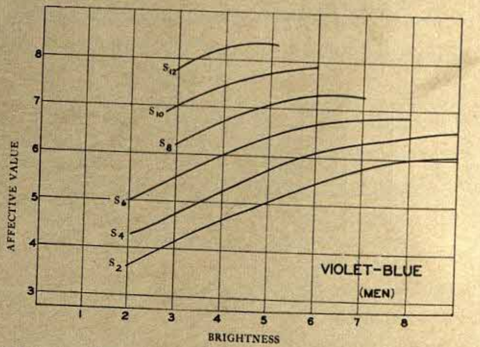

FIG. 3. CURVILINEAR REGRESSION-LINES RELATING AFFECTIVE VALUE TO BRIGHTNESS OF COLORS

(Hue constant at violet-blue; saturation levels varying from S2 to S 12.)

In general, there is a positive relationship between brightness and *A-V*, but in no case is it linear. For the so-called cool colors, like that represented in Fig. 3, the curves tend to be concave downward. They rise with negative acceleration and in some instances there is a slight drop at the upper levels. For the so-called warm colors, particularly in the region of yellow, there is positive acceleration. Taking these two kinds of regressions together, we may state the principle that colors tend to be liked most at levels of brightness at which they can be most saturated.

Fig. 4 shows an example of a family of curves for the relation of *A-V* to saturation for various levels of brightness (B), the hue being held constant. Most noteworthy is the fact that at the lower levels of brightness there is a reversal in *A-V* as a function of sautration. The principle is that at lower levels of brightness a saturation of zero is preferred to saturations of 2 or even of 4. The charts in Figs. 5-14 will show that the minimum *A-Vs* are usually at neither the lowest level of saturation nor at the lowest level of brightness and that this rule holds for both sexes and at most hues.

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[11] J. S. Stamm, Fourier analyses for curves of affective value of color as functions of hue, this JOURNAL, 68, 1955, 124-132.

Isohedonic charts. In Figs. 5-14 inclusive are shown what have become known as isohedonic charts, and isohedon being a line (or locus) of uniform *A-V*.[12] Each chart represents a hue at one page of the *Munsell Book of Color;* 10 (alternate) pages being represented. In each chart, the vertical axis represents the variable of brightness and the horizontal axis represents the variable of saturation. Any point in a plane represents a particular hue, brightness, and saturation. Obviously, not all points on a plane represent actual colors, since the color-solid is a double pyramid (or cone) and not a cubical affair. The curved lines (isohedons) in a chart extend roughly within the limits of the known, experienced colors.

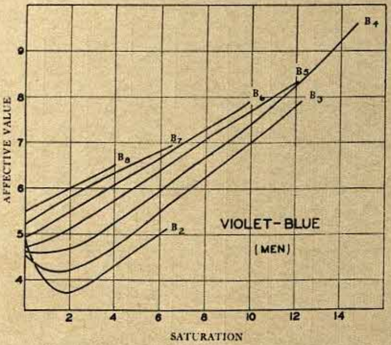

FIG. 4. CURVILINEAR REGRESSION-LINES RELATING AFFECTIVE VALUE TO SATURATION
(Hue constant at violet-blue and brightness at several levels varying from B2 to B8.)

The construction of an isohedonic chart proceeded roughly as follows. In preparation of one of the charts in Fig. 5, for example, the smoothed curves such as those in Figs. 2-4, wherever the hue red was in any way involved, were inspected, also the appropriate curve in Fig. 1. Examination of the plotted curves where red was concerned showed what combinations of brightness and saturation would yield predicted average *A-Vs* of exactly 4, 5, 6, etc., and of values midway between them.

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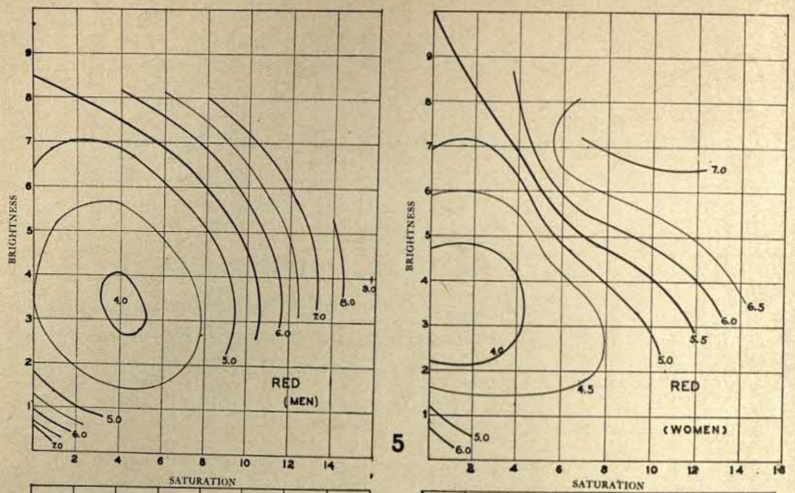
[12] Such charts were proposed by the senior author (*op. cit.,* 1939, 19.) and have also been applied to *A-V* as related to sounds (see Guilford, System in the relationship of affective value to frequency and intensity of auditory stimuli, this JOURNAL, 67, 1954, 691-695).



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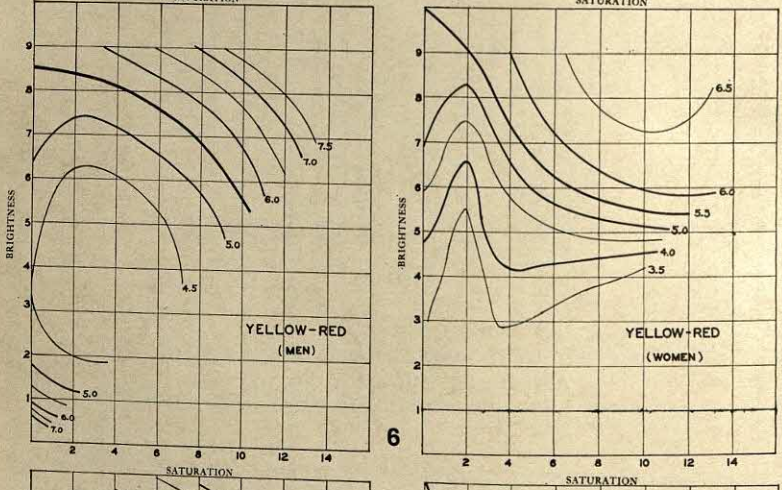

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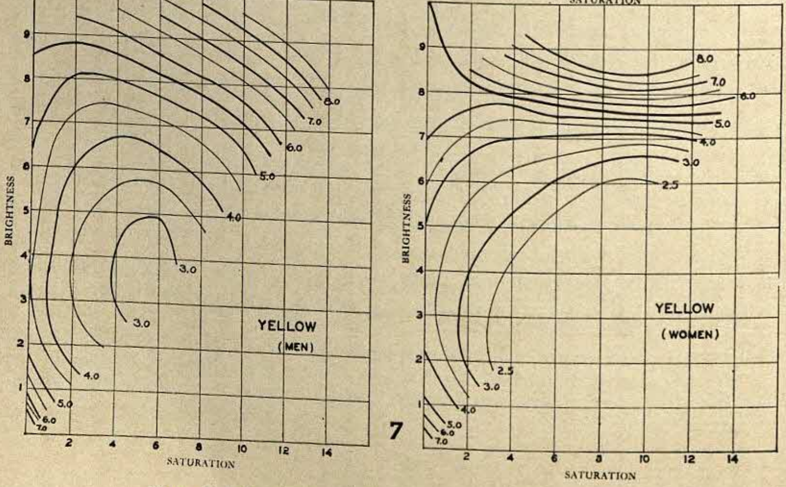

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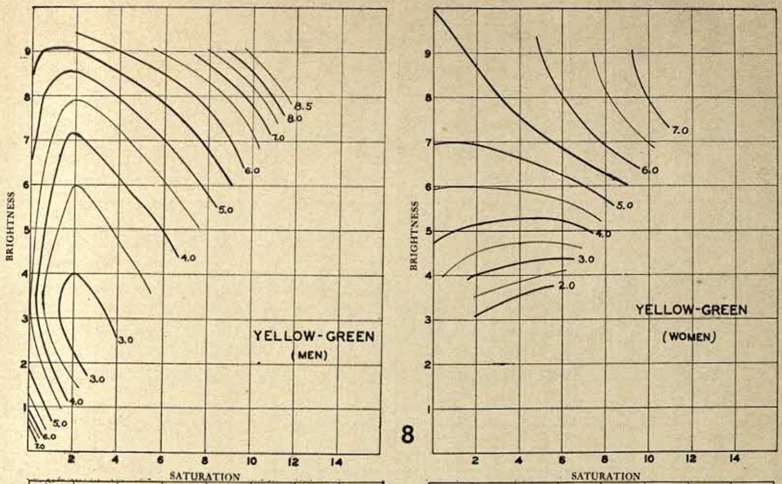
FIGS. 5-7. ISOHEDONIC C



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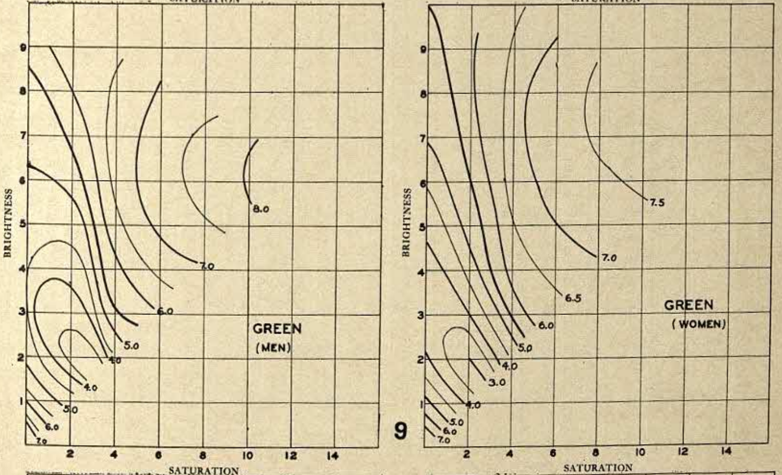

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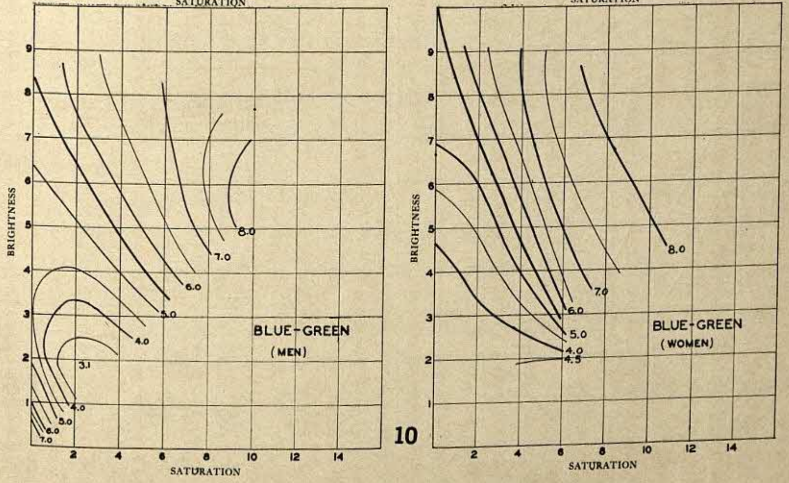

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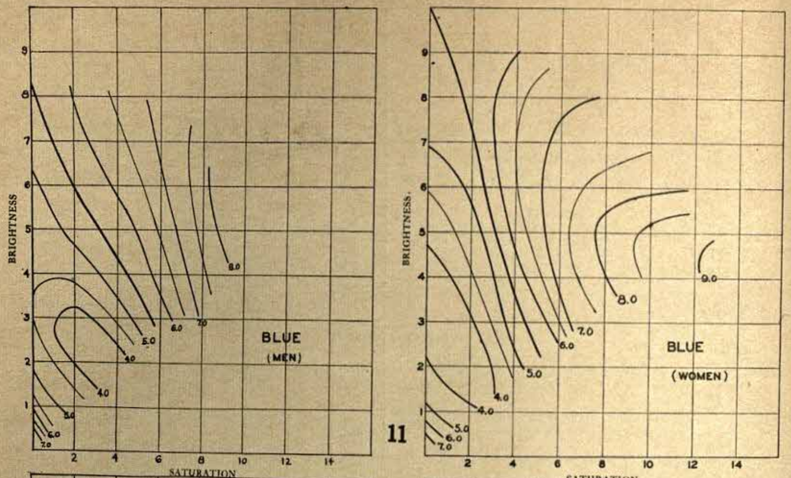
Figs. 8-10. Isohedonic Charts for YG, G, and BG



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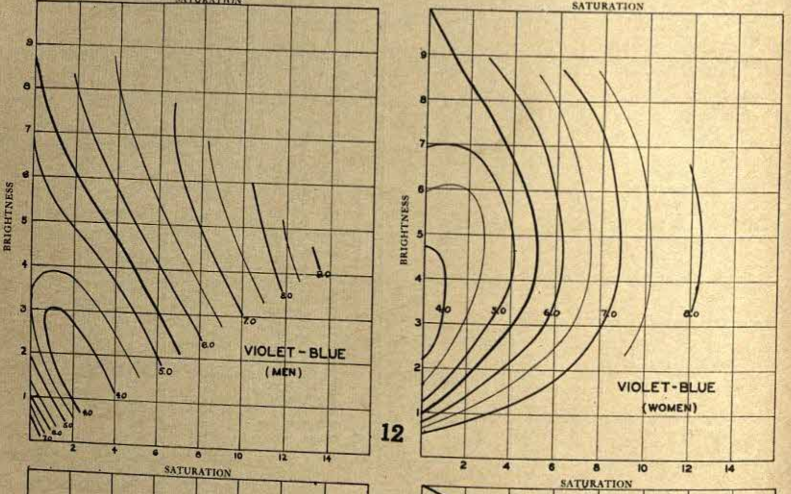

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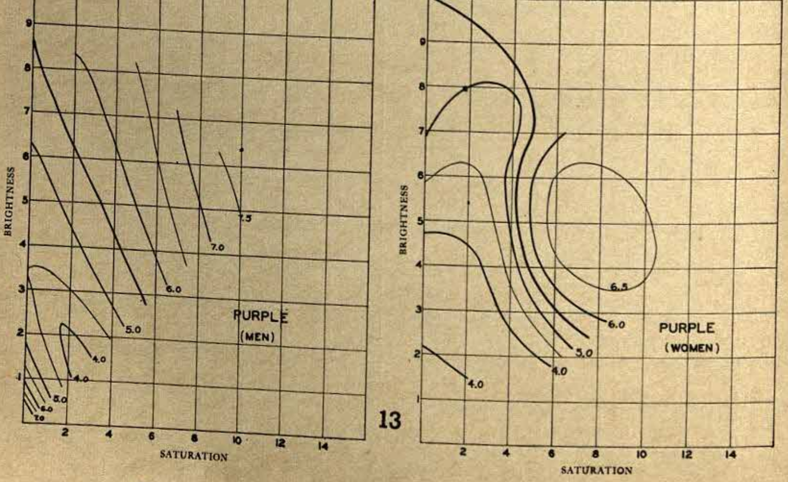

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FIGS. 11-13. ISOHEDONIC CHARTS FOR B, VB, AND P

The isohedonic lines were drawn accordingly. The line at an *A-V* of 5.5 has been drawn very heavy, since that value is at the point of indifference. For the men's chart in Fig. 5, colors above and to the right of this isohedon are expected to be rated pleasant to some degree. To the left and below this line the *A-V* decreases, down to the level of about 4, but beyond this the *A-V* again increasing and rising quite rapidly. For the corresponding women's chart, the general trends are similar, with minor variations.

Prediction of affective values. Perhaps the chief value of the isohedonic charts is the basis that they provide for the prediction of the most probable *A-V* from knowledge of the quantitative Munsell designations of hue, brightness, and saturation. For example, a red Munsell value (brightness–B) 7 and chroma (saturation–S) 10 would have an expected *A-V* of 6.8 for men and about 7.2 for women. A red of Munsell value B3 and chroma S12 would be expected to have an *A-V* of 6.2 for men and 5.5 for women.

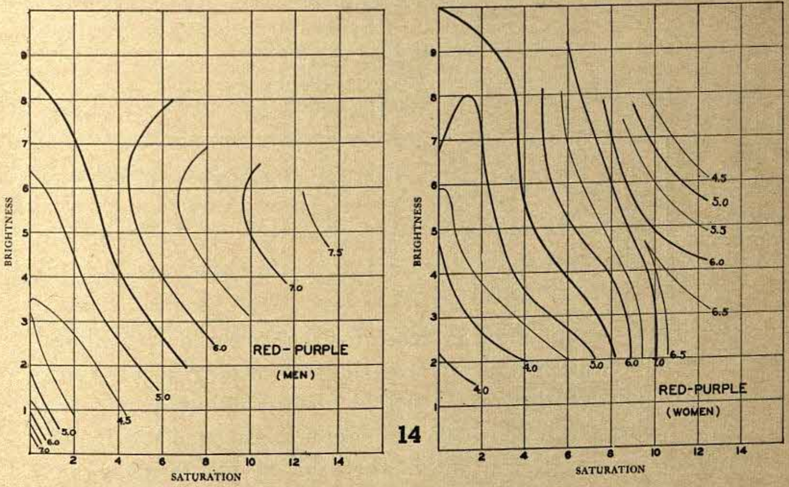

FIG. 14. ISOHEDONIC CHART FOR RP

The goodness of such predictions for the sample of *O*s from whom the judgments were obtained can be indicated by the correlations between expected and obtained *A-V*s. For each color-stimulus used in the study, an estimate of its expected *A-V* was found from the isohedonic charts. The correlations between obtained and exepcted *A-V*s are given in Table I, for each of the 10 hues separately and for each sex, as well as for all hues together. Somewhat smaller correlations should be expected in a cross-validation sample, of course.

From the given correlations, it will be seen that the best predictions can be made in the region from green to purple-blue and that the poorest predictions occur for purple, green-yellow, and yellow-red. In general, where predictions are better for men, they are also better for women. For all hues combined, the correlations between predicted and obtained *A-V*s are 0.93 for the men and 0.88 for the women. Coupled with this lower predictability for the women is the fact that their isohedonic charts are somewhat less regular and simple. Perhaps this is because women are more color-conscious and their tastes for color are more changeable and diverse. There is supporting evidence for the conclusion regarding lower stability and consistency of women's judgments of *A-V* of colors in a study that indicated that over a period of 14 yr., inter-year correlations averaged higher for men (0.80) than for women (0.68).[13] In the same study, however, agreement, within the same year, was about the same among women as among men—the average correlations being 0.88 for men and 0.90 for women.

TABLE I

Correlations among Observed and Predicted Affective Values for Hues and between Men and Women

(R=red, Y=yellow; G=green, B=blue; P=purple)

Ss	Hue										
	R	YR	Y	GY	G	BG	B	PB	P	RP	All hues
Men	.96	.83	.97	.86	.97	.93	.95	.94	.81	.87	.93
Women	.93	.80	.90	.79	.92	.92	.96	.91	.77	.90	.88
M vs. W	.88	.67	.94	.92	.93	.96	.95	.91	.70	.27	.84

Also shown in Table I are intercorrelations of obtained *A-V*s of men and women for the different hues and for them all combined. There was very poor agreement for colors with the hue of red-purple (RP), the correlation being 0.27. It is noteworthy that for this hue the women liked the most saturated colors less than they did most colors of moderate saturation. This is the only instance in which a reversal in *A-V* occurred in going from moderate to high saturation. This systematic shift in pattern was responsible for the low correlation between the sexes at the hue of red-purple. The next lowest inter-sex correlation was 0.67 for the hue yellow-red (YR). The correlations were above 0.90 in the hue region from yellow through purple-blue. The average inter-sex correlation was 0.84 (all colors included). Had the one hue of red-purple been excluded, the agreement might have been of the order of the intra-sex correlations.

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[13] W. E. Walton, R. B. Guilford, and J. P. Guilford, Color preferences of 1279 university students, this Journal, 45, 1933, 322-328.

Correlations do not, however, tell the whole story regarding sex-differences in ratings. In general, men's ratings average higher than women's. This is true for the earlier observations as well as the later observations, with different *O*s involved in the two cases. It is also true for both achromatic and chromatic colors. It is true at every hue except one. The over-all average difference is about a half unit on the rating scale. Although this difference is small, it represents a condition that cannot be ignored in predicting *A-V*s of colors.

DISCUSSION

Limitations in predicting affective value. It has been proposed that the isohedonic charts be used in predicting the *A-V*s of colors for average men and women. Within the sample of judges used in this study, and within the experimental conditions employed, it is evident that a very large proportion of the variance in the *A-V* can be accounted for on the basis of the known Munsell specifications of color. Use of such predictions in practical situations would, of course, be subject to many risks. It is not without reason, however, that we should expect some degree of relationship between such predictions and the satisfactions to be derived from colors in everyday experience. Improvement of predictions of such satisfactions would depend upon our having other pertinent information.

Greater generality of prediction could be achieved by further basic research that brought into the picture some variations of additional conditions. The size of the color-sample should be varied over a reasonable range, including many stimuli larger than were utilized in this study. The brightness-level of the background should be varied over a wide range. Surfaces other than that of paper could be utilized, also variations in texture, gloss, and shape. From a theoretical point of view, aperture colors could be used, also bulky colors. From a more practical point of view, each color could be applied to commercial objects such as gowns, neckties, houses, and motor cars. In all such studies involving the combination of color- and non-color variables, it will be important to watch for interaction-effects of one kind or another. For example, there may be strongly preferred (or unpreferred) combinations of colors and forms, the affective values of which cannot be well predicted from the affective values of their color and form components alone. Beyond all these areas of needed investigation there exists the whole area of affective reactions to color-combinations, a problem that has as yet been scarcely touched.[14]

SUMMARY

This report pertains to a systematic and relatively exhaustive study of the affective values of 316 different color specimens, each of which had been calibrated in terms of color-specifications of the *Munsell Book of Color*. The color-stimuli were in the form of 2-in. squares of paper on a neutral gray background. Each was rated by 20 men and 20 women students on two different occasions. Smoothed graphic adjustments of the data were made to curvilinear functional relationships, from which isohedonic charts were constructed showing the loci of equal preference-values applying to all colors in the color-solid.

Considerable consistency in ratings of affective values of colors was found from day to day for the same *O*s, from one set of *O*s to another within the same sex, and even between *O*s of different sex. Men rated colors generally a little higher than did women. Agreement as to relative affective values was better for colors of certain hues than for others, both within and between sexes. Preferences were highest in the region of green to blue and lowest in the region of yellow and yellow-green, when brightness and saturation are held constant. With few exceptions, affective value is positively related to brightness and to saturation, all relationships being curvilinear. Predictions of affective values from the specified hue, brightness, and saturation of colors were generally excellent within the sample of *O*s who rated the colors. Predictions of a more general scope will depend upon information concerning other populations, other variables of color-stimuli, other viewing-conditions, and particular uses of colors. Considerable gain in accuracy of prediction should be possible from further basic research of the kind reported here.

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[14] A study by E. C. Allen and J. P. Guilford (Factors determining the affective values of color combinations, this JOURNAL, 48, 1936, 643-648) is practically alone in the area of this problem.

THE DETECTION OF STATISTICALLY DEFINED PATTERNS IN A MATRIX OF DOTS

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Lincoln Laboratory, Massachusetts Institute of Technology

This paper deals with the ability of human observers to detect patterns in 'noisy,' visual displays. By 'noisy' is meant any aspect of a display that masks or competes with the signals or patterns being presented. An automobile driver, peering through a dirty windshield on a foggy night, is confronted by a 'noisy,' visual pattern. A television viewer in a fringe reception-area sees a 'noisy,' or 'snowy,' image. Radar scopes, X-ray films, and televised pictures are often obscured by 'noise.' The class of 'noisy,' visual displays is large, and 'noise' is, in general, difficult to specify. We chose to study a simple display—a matrix of dots on which 'noise' could be specified in terms of the probability of occurrence of a dot. Using this display, we have been able to carry out several parametric studies.

Experimental procedures. The displays used in all the experiments were based on a square matrix of dot-positions. A vertical bar pattern was specified by dividing the matrix into a number of vertical strips of equal width, and a horizontal bar pattern was similarly specified. The dot-positions in the odd-numbered bars were assigned a fixed probability p_1 of containing a dot on any particular display, and the dot-positions in the alternate bars were assigned a different probability p_2. To prepare a display, each dot-position was either filled or left vacant according to its assigned probability. The occurrence of a dot at any specific position in the matrix was independent of all other dot occurrences, so that each display was essentially a random sample from a set of independent bionomial distributions. The experimental displays were all variations of a standard display that had 128 × 128 dot-positions, and 8 bars of 16 rows (or columns) each. In some experiments the size of the matrix or the number of bars was varied. In all the experiments the probabilities p_1 and p_2 were varied to determine the curves of detection by an *O*.

The displays were prepared by means of a high-speed digital computer.[1] The computer was programmed to produce a random sequence of horizontal and vertical displays according to specified parameters. Fifty or more displays were prepared for each experimental condition, half with the horizontal pattern and half with the vertical pattern. The dots appeared on an oscilloscope attached to the computer and were photographed by an automatic 35 mm. camera. The computer generated and photo-

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* Received for publication November 11, 1958. The research reported in this paper was supported jointly by the Army, Navy, and Air Force under contract with the Massachusetts Institute of Technology.

[1] The Memory Test Computer (MTC) at the MIT Lincoln Laboratory.

graphed displays at a rate of about 10 per min. In an attempt to control brightness contrast, all filmstrips for a single experiment were made in a single, continuous session on the computer, and the film was developed at one time under uniform conditions. Brightness contrast was not precisely equated from one experiment to the next because of slight day-to-day variations in the oscilloscope of the computer and the developing of the film.

During the experiment the displays were projected onto a vertical screen 18 ft. from *O*. No fixation-point was provided, but the frame of the screen, about 2-ft. square, was clearly visible. Due to a slight maladjustment of the oscilloscope of the computer, the projected displays were not quite square; the standard measured 15.4 in. high × 16.9 in. wide. The diagonal dimension subtended a visual angle of 6.0°. A single dot subtended an angle of about 1.67′, and the angular distance between the centers of adjacent dots was about 2′. The *O*s reported that they could barely see a single isolated dot, and could not resolve individual adjacent dots. An area in which all dot positions were filled, however, did not appear uniform but had a very fine, regular texture.

The displays for a given pair of probabilities, p_1, p_2, were presented successively in a single, experimental session. Each display was projected for a fixed duration, usually ¼ sec., and *O* was required to report whether the pattern was horizontal or vertical. *O* was seated at a small table that held a response-box with a lever for each response. *O*'s response was recorded automatically and the next display was presented after a delay of about 2 sec. *O* was not informed of the correctness of her responses.

At the beginning of each experiment, *O* was shown a short practice-series of 5 or 10 displays at clearly supraliminal values, and additional displays at representative values of p_1 and p_2. This series served to familiarize the *O*s with the appearance of the type of displays to be used. It was necessary because changing the various parameters of the displays caused considerable change in the character of the stimuli, as seen in Figs. 1, 4, 6, and 8.

Five women, members of the Laboratory staff, served as *O*s in all of the experiments, except where noted.

Statistical procedures. The threshold was taken to be the difference between the probabilities p_1 and p_2 at which 75% of the responses were correct. To obtain the thresholds, the frequency of correct responses was averaged across *O*s; the traditional guessing correction was applied; the transformed data were plotted on normal probability paper as a function of $(p_1\text{-}p_2)$; and straight lines were fitted visually. To determine statistical significance, the methods of probit analysis were used.[2]

The ideal detector. When a display is formed by many dots, each of which occurs with some probability, it contains a large amount of information, in the Shannon sense, most of which is 'noise.' The signal or pattern can generally be described in many fewer bits. Thus it seems likely that the perceptual process must include some sort of statistical averaging process to reduce and recode the displayed information and to detect the signal, as Attneave has suggested.[3] Some possible scanning processes, such as local averaging and local differencing, have been suggested by Selfridge and

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[2] D. J. Finney, *Probit Analysis*, 1952, 88-99.

[3] Fred Attneave, Some informational aspects of visual perception, *Psychol. Rev.*, 61, 1954, 183-193.

Dinneen who programmed a digital computer to recognize patterns in dot-arrays.[4] These processes, however, turn out to be unsuited to displays with very low signal-to-noise ratio, such as those in the present experiments. Coltman, working in the context of X-ray quantum noise, used a statistical detection-model proposed by Rose to account for the detectability of horizontal bar-patterns generated by a Poisson process.[5] Rose's model is a special case of the general theory of signal detection, as described by Middleton, Peterson, Birdsall, and others.[6]

A model for an ideal statistical detector that is applicable to our displays can be derived from the general theory. Our experiments were designed in part to compare the behavior of human *O*s with this ideal model. We did not expect the human *O*s to be as sensitive as the ideal detector, but we thought that the curves relating the empirical thresholds to the various independent parameters of the display might have the same shape as the corresponding theoretical curves.

We consider the ideal detector to be a computing machine that adds no noise or fluctuations of its own to the inherent statistical variability in the displays. It is assumed that all the parameters and rules for forming the displays are available to the detector except the dot-probabilities. Thus the detector 'knows,' for both the vertical and horizontal patterns, exactly which positions are governed by each of the two binomial distributions. The machine operates by first counting the number of dots generated by each distribution under the hypothesis that the pattern is vertical, and noting the absolute difference in the counts. Then it counts and notes the equivalent difference for the horizontal hypothesis. Finally, the machine responds by reporting the hypothesis that led to the larger difference.

Using the normal distribution to approximate the binomial, it can be shown that the percentage of correct responses is $a^2 + (1\text{-}a)^2$, where a is the area under the unit normal curve to the left of Z, where $Z = [N(p_1 - p_2)]^{\frac{1}{2}}/[2(p_1q_1 + p_2q_2)]^{\frac{1}{2}}$ where N is the total number of dot positions, and $q = 1 - p$. This theoretical detection curve is nearly a linear function of $(p_1 - p_2)$ when transformed in the same way as the empirical data.

The threshold has been defined as the probability difference corresponding to 75% correct detections. Setting 0.75 equal to $a^2 + (1 - a)^2$, we find that a at threshold is 0.854,whence Z at threshold is 1.055. Setting this value of Z in the above equation and solving for $(p_1 - p_2)$, we find that the threshold is given by

$$(p_1 - p_2)_{75} = 2.11 \ (p_1q_1 + p_2q_2)^{\frac{1}{2}}/(N)^{\frac{1}{2}}.$$

For small values of $(p_1 - p_2)$, the expression $(p_1q_1 + p_2q_2)^{\frac{1}{2}}$ is very nearly equal to $(2pq)^{\frac{1}{2}}$, where $p = (p_1 + p_2)/2$, so we have, finally, that

$$(p_1 - p_2)_{75} = 2.98 \ (pq/N)^{\frac{1}{2}}.$$

Thus the threshold of the ideal detector is inversely proportional to $(N)^{\frac{1}{2}}$ and directly proportional to $(pq)^{\frac{1}{2}}$.

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[4] O. G. Selfridge, Pattern recognition and modern computers, *Proceedings of the Western Joint Computer Conference,* 1955, 91-93; G. P. Dinneen, Programming pattern recognition, *ibid.,* 94-100.

[5] J. W. Coltman, Scintillation limitations to resolving power in imaging devices, *J. opt. Soc. Amer.,* 44, 1954, 234-237; Albert Rose, The sensitivity performance of the human eye on an absolute scale, *ibid.,* 38, 1948, 196-208.

[6] D. Middleton, The statistical theory of detection: I. Optimum detection of signals in noise, *MIT Lincoln Laboratory, Technical Report 35,* 1953; W. W. Peterson and T. G. Birdsall, The theory of signal detectability, *Engineering Research Inst., Univ. of Michigan, Technical Report 13,* 1953.

EXPERIMENT I: DURATION OF EXPOSURE

The first experiment was designed to examine the effect of the duration of exposure on detection. We also wanted to determine the shape of the detection-curves and the extent of individual differences.

Procedure. Films of 128 eight-bar displays were prepared at each of five values of p_1: 0.563, 0.547, 0.531, 0.516, and 0.500 (36/64, 35/64, 34/64, 33/64 and 32/64 respectively), with $p_2 = 1 - p_1$. A typical display is shown in Fig. 1.

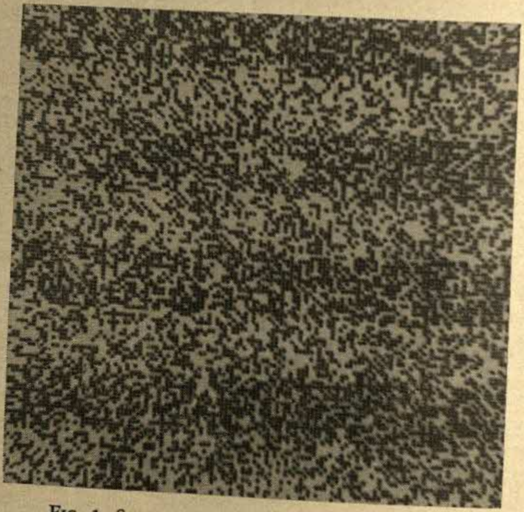

FIG. 1. SAMPLE HORIZONTAL BAR PATTERN
($p_1 = 0.60$, $p_2 = 0.40$; matrix size is 128 × 128; there are eight bars.)

The *O*s were tested at each of four durations of exposure: 1/16, ¼, 1, and 4 sec. The order of the first three durations was balanced as well as possible for the five *O*s, but within each duration, all *O*s viewed the series in the order of decreasing probability-difference, *i.e.* from $p_1 = 0.563$ down to $p_1 = 0.500$. The fourth duration—4 sec.—was given immediately after the other three.

Results. The data, averaged over *O*s, are shown in Fig. 2. The percentage of correct responses is slightly higher at ¼ than at 1⁄16 sec. and much higher at 1 sec. than at either 1⁄16 or ¼ sec. Lengthening the duration past 1 sec. did not, however, improve detection. Indeed, some of the *O*s reported that prolonged viewing served mainly to confuse them, so they finally adopted the strategy of glancing at the display and then looking away. For comparison, the hypothetical curve of the ideal statistical detector is also shown in Fig. 2. The empirical curves have the same general shape as the ideal curve, but the thresholds are much higher. The theoretical threshold is 0.012. The empirical thresholds were 0.084, 0.070, and 0.056 for exposures of $\frac{1}{16}$, $\frac{1}{4}$ and 1 sec., respectively.

Individual differences were slight. The effect of duration of exposure was about the same for all *O*s, so no great violence is done to the data by averaging over conditions for each *O*. The resulting curves, one for each *O*, are shown in Fig. 3. Even though they are very close together, it is possible to discern that the curves maintain the same rank order for the three highest probability differences. Thus, the individual differences may be consistent, even though the range of estimated thresholds is only 0.013.

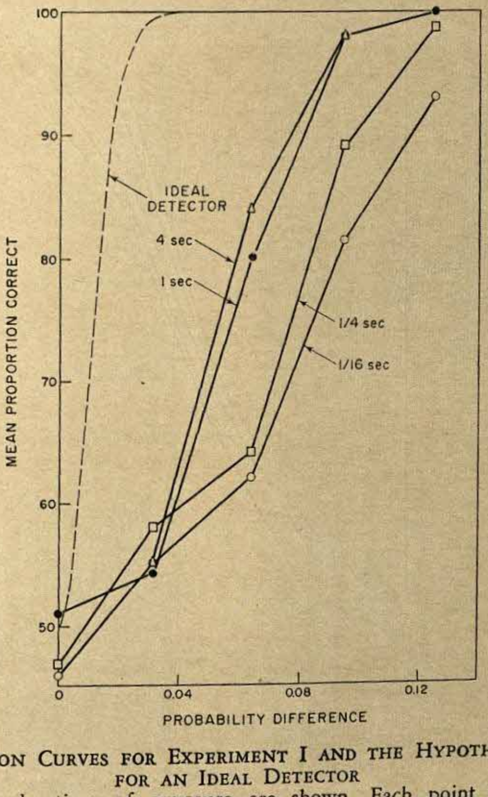

FIG. 2. DETECTION CURVES FOR EXPERIMENT I AND THE HYPOTHETICAL CURVE FOR AN IDEAL DETECTOR

(Data for four durations of exposure are shown. Each point represents 640 judgments: 5 *O*s × 128 trials.)

EXPERIMENT II: AVERAGE DOT-PROBABILITY

Experiment II was designed to study the effect of the average number of dots on the display.

Procedure. Five fixed values of p_1 were chosen as standards: 0.10, 0.30, 0.50, 0.70, and 0.90, and p_2 was increased from the standards in four steps of 0.02, yielding 20 pairs (p_1,p_2): (0.10,0.12), (0.10,0.14), (0.10,0.16), (0.10,0.18); (0.30,0.32), . . . (0.30,0.38); (0.50,0.52), . . . (0.50,0.58); (0.70,0.72), . . . (0.70,0.78); (0.90, 0.92), . . . (0.90,0.98). For standards 0.10 and 0.90, decreasing series were also made; (0.10,0.08), . . . (0.10,0.02), (0.90,0.88), . . . (0.90,0.82). One hundred displays were prepared for each of the 28 p_1,p_2 pairs. Fig. 4 shows some sample displays. The exposure was ¼ sec. The *O*s were tested at the five standards, with order of presentation determined by a latin square.

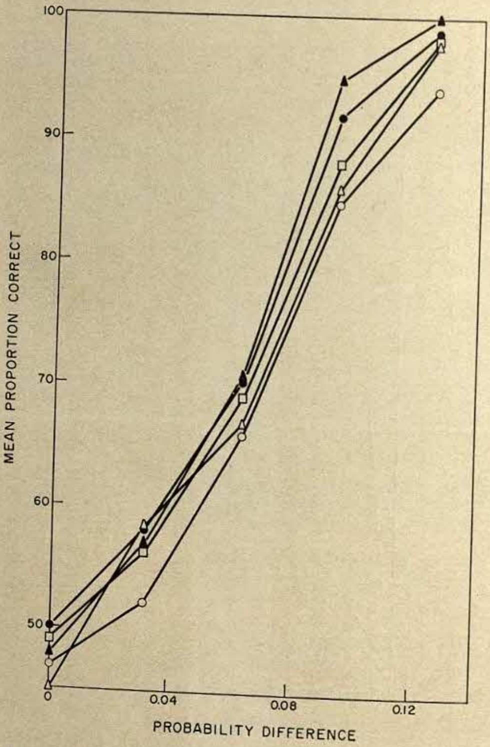

FIG. 3. DETECTION FOR 5 *O*s IN EXPERIMENT I
(Each point represents 384 judgments: 3 conditions × 128 trials.)

Results. The observed thresholds are shown in Fig. 5. Apparently detection is better at the extreme probability values.[7] Moreover, the shape

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[7] An unforeseen problem arose in choosing the abscissa for this graph. It had seemed reasonable to use the traditional standard-and-variable procedure in deter-

(Continued on page 510)

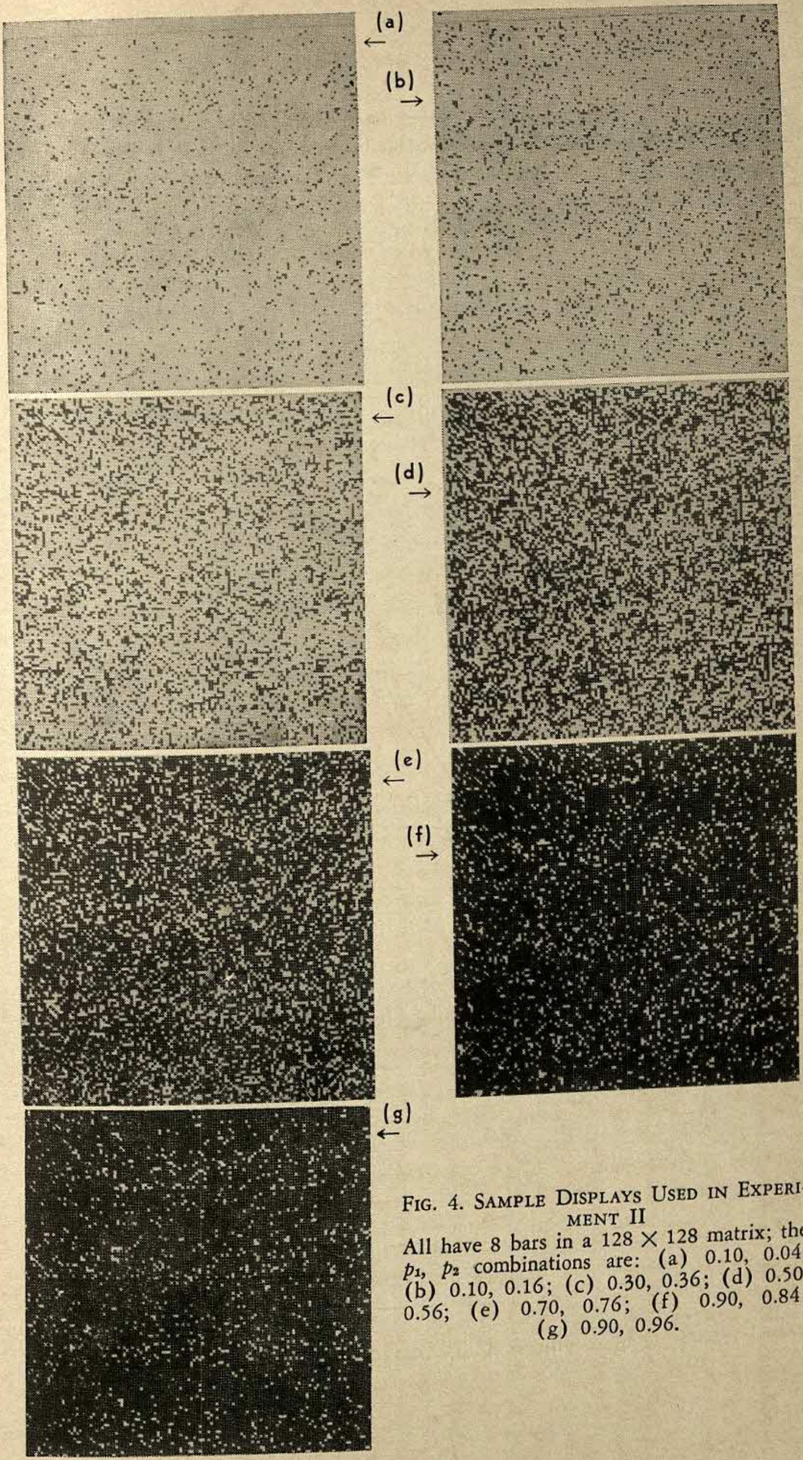

FIG. 4. SAMPLE DISPLAYS USED IN EXPERIMENT II

All have 8 bars in a 128 × 128 matrix; the p_1, p_2 combinations are: (a) 0.10, 0.04; (b) 0.10, 0.16; (c) 0.30, 0.36; (d) 0.50, 0.56; (e) 0.70, 0.76; (f) 0.90, 0.84; (g) 0.90, 0.96.

of the curve is very suggestive of the binomial *SD*. The ordinate of the fitted curve in Fig. 5 is proportional to $(pq)^{\frac{1}{2}}$, where p is the average probability at threshold. As noted above, the threshold of the ideal detector is likewise proportional to $(pq)^{\frac{1}{2}}$. The constants of proportionality are quite different, however. The fitted thresholds are larger than the ideal thresholds by a factor of 5.3. Thus the *O*s are less sensitive, but their performance is otherwise qualitatively similar to the ideal detector.

EXPERIMENT III: VISUAL ANGLE

Casual observations seemed to indicate that the visual angle subtended by the display was an important variable. It seemed reasonable to expect that as the visual angle became large the pattern would 'disintegrate' while as the visual angle became small, the dots would tend to merge, making the pattern into a set of gray bars. Consequently, the effect of visual angle was studied formally.

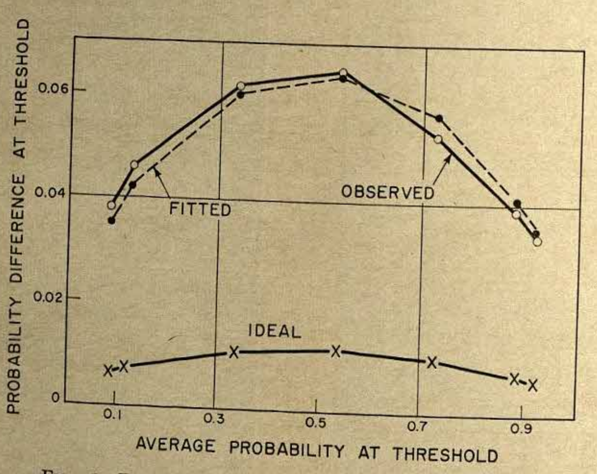

FIG. 5. DETECTION-THRESHOLDS FOR VARIOUS DOT PROBABILITIES IN EXPERIMENT II

The fitted and ideal detection thresholds are also shown.

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mining the detection-curves, but then how were the increasing and decreasing thresholds to be shown for the extreme probabilities? The problem arises wherever the magnitude of the threshold is changing rapidly with respect to the stimulus-magnitude. In the present case, p_1 and p_2 each affect half of the display, so the display should probably be characterized by the average number of dots, or the average probability. Unfortunately, this parameter was not held constant; the abscissa that we chose is the estimated average probability at the threshold: $1/2\,[p_1 + (p_2)_{75}]$.

Procedure. The visual angle of the displays in Experiment I was 6°, with *O* seated 18 ft. from the projection-screen. In the present experiment, a visual angle of 12° was obtained when *O* was seated 9 ft. from the screen. The *O*s were tested at a ¼-sec. exposure, with 128 trials at each of the five values of p_1 used in Experiment I; *i.e.* 0.563, 0.547, 0.531, 0.516, and 0.500, with $p_2 = 1 - p_1$.

Results. The threshold was found to be identical with that obtained under the equivalent condition in Experiment I; both were 0.074. This negative result prompted a further study of visual angle. The size of the display was so decreased, with its brightness held constant, that viewing distances of 6, 12, and 18 ft. corresponded to visual angles of 6°, 3°, and 2°, respectively. Three *O*s were tested in balanced order, at the ¼ sec. exposure duration. Only three values of p_1 were used—0.516, 0.531, 0.547—since these adequately spanned the expected thresholds. The resulting thresholds for the three visual angles are 0.072, 0.076, and 0.078, respectively. The differences are not statistically significant.

At 2° individual dots could not be resolved, while at 12° it was difficult to perceive the whole display in a single fixation. Nevertheless, within the range of 2° to 12°, visual angle had no significant effect on the detection thresholds of our *O*s.[8]

EXPERIMENT IV: GRAIN OF THE DISPLAY

The analysis of the ideal statistical detector suggests that detection thresholds might be inversely proportional to the square root of the number of dots in the display, which we shall call the grain of the display. In a square matrix, the grain is equal to the number of rows or columns. Two related experiments were done to study the effect of display grain.

Procedure. There are several ways in which the grain can be changed, depending on which of the many relevant parameters are held constant. Size of dot, separation of the dots, area of display, number of bars in the pattern, and number of lines of dots per bar are all relevant parameters and are to a considerable extent interdependent. It was decided to maintain a square display containing eight bars. The number of lines per bar was consequently proportional to the grain. It was further decided to vary dot-separation and display-area while maintaining a dot of fixed size.

Since the product of grain and dot-separation equals the square root of the display-area, dot-separation and display-area could not simultaneously be held constant, as the grain was varied. We decided to run the experiment in two parts. In Experiment IV A the area of the display was held constant, with the result that dot-separation was inversely proportional to grain (see Fig. 6 a, b, c, and d). In Experiment IV B dot-separation was held constant, with display-area being directly proportional to the square of the grain (see Fig. 6 e, f, g, and h). In Experiment IV A all displays were of the same size as the standard 128-line display, while in Experiment IV B the displays were smaller than the standard if they had fewer lines, and larger if they had more lines. Thus with grains of 32, 64, and 96, the displays with constant area (IV A) had a larger dot-separation than the corresponding displays with constant dot-separation (IV B), while the reverse is true for the 160-line display.

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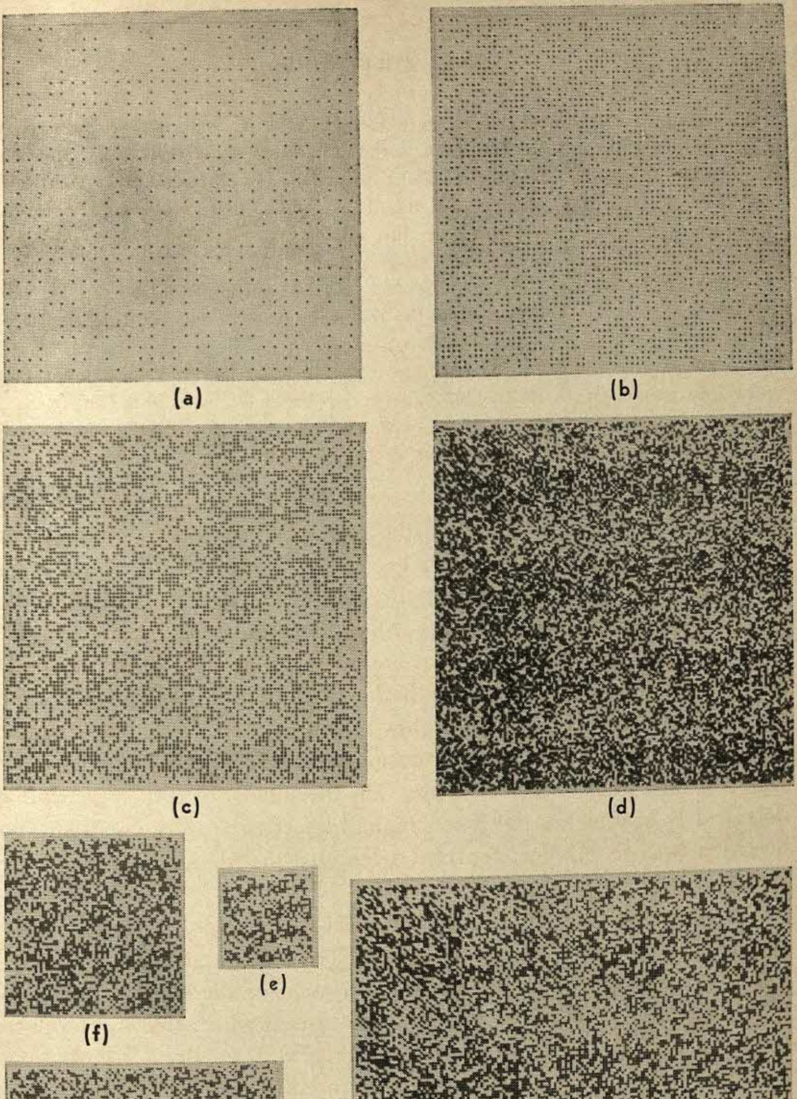
[8] This result is consistent with some recent unpublished data of Finney, obtained in a similar detection-task, in which the effect of visual angle is curvilinear with a very broad maximum. Significant decreases in performance are obtained with visual angles below 1° and above 15°.

The visual angle of the standard 128-bar display was 6°; the exposure was ¼ sec. The values of p_1 were 0.52, 0.54, 0.56, and 0.58 with $p_2 = 1 - p_1$ so that the average probability was always 0.50. In both experiments the 5 *O*s were tested in balanced order on 5 values of grain: 32, 64, 96, 128, and 160. Each *O* made 50 observations at each value of p_1 for each grain. Part B followed Part A in this experiment.

Results. The empirical thresholds for each grain are shown by the solid curves in Fig. 7. The ordinates of the corresponding fitted curves, shown as dotted lines, are proportional to $1/N^{\frac{1}{2}}$. It is apparent that the two ways of varying grain lead to different thresholds, and that in both cases the thresholds are only very roughly proportional to $1/N^{\frac{1}{2}}$. With constant dot-separation and varying display-area (Experiment IV B), the fit is good for low grains, but not for high. The thresholds obtained for the larger displays are not sufficiently low. On the other hand, with constant display area and varying dot-separation (Experiment IV A) the fit is good for high grains but not for low. The thresholds for displays with few dots widely spaced are much too high. In fact there was essentially no detection with the 32-line display. *O*s reported that the dots were scarcely visible.

The difference between the two empirical curves might seem to indicate an independent effect of dot-separation. The curves cross at a grain of 128, the standard display. From the description of the stimulus-materials given above it can be noted that in every case in which two displays had the same grain but differed in area, the display with smaller dot-separation, *i.e.* the denser display, yielded the lower threshold. Dot-separation would thus appear to be an important variable independent of the grain of the display.

The effect of dot-separation is not, however, independent of dot-size. In Experiment III we simultaneously varied dot-separation, dot-size, and display-area (of the proximal stimulus) while holding grain constant, and found no differences in thresholds. Perhaps the important parameter is the ratio of dot-size to dot-separation. This ratio was fixed in Experiment III, but was varied in Experiments IV A and B; the larger ratios—denser displays—giving lower thresholds.



(a) (b) (c) (d) (e) (f) (g) (h)

FIG. 6. HORIZONTAL BAR PATTERNS LIKE THOSE USED IN EXPERIMENT IV A AND B
There are 8 bars; $p_1 = 0.55$, $p_2 = 0.45$. A had a fixed display-size; grains shown are: (a) 32, (b) 64, (c) 96, (d) 160. B had fixed separation; grains shown are: (e) 32, (f) 64, (g) 96, (h) 160.

The threshold for the standard 128-line display is somewhat lower in Experiment IV than in Experiment III (0.060 vs. 0.074). This difference may be partly the result of differences in brightness contrast; the extent of contrast differences are believed small, but are unknown, as is the effect of contrast on detection in the present experimental situation. The difference in thresholds may also be partly the result of a general, long-term learning effect. In Experiment V, reported below, the corresponding threshold is still lower—0.045. Probably the *O*s were gradually learning how to react optimally in our experimental situation. Within each experiment, order of presentation was carefully balanced but comparisons of absolute values between experiments are somewhat fogged by this learning effect.

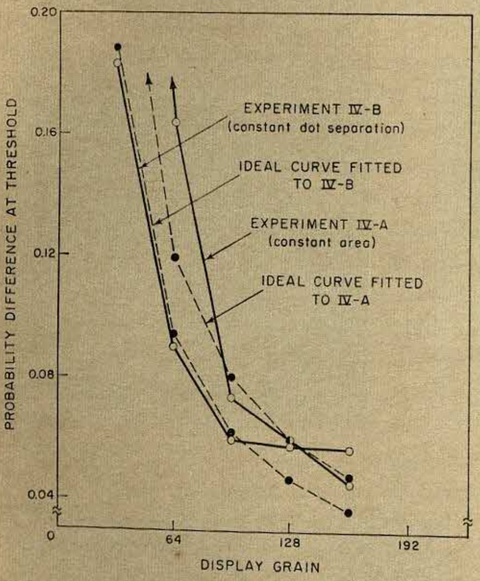


FIG. 7. DETECTION-THRESHOLDS IN EXPERIMENTS IV A AND B

(Dot-separation, in A, was varied with display-size held constant; In B, display-size was varied with dot-separation held constant. The best-fitting ideal detection-curves are shown for both conditions.)

EXPERIMENT V: NUMBER OF BARS

An 8-bar pattern was used throughout the preceding experiments in a variety of display-conditions. In Experiment V the number of bars in the pattern was varied. Patterns of 2, 4, 8, 16, and 32 bars were used. Samples are shown in Fig. 8. The standard 128 × 128 matrix was used, so the individual bars had 64, 32, 16, 8, or 4 lines, respectively.

Procedure. For each of the five patterns, our five well-trained *O*s judged 50 displays at each of four values of p_1—0.515, 0.530, 0.545, and 0.560—with $p_2 = 1 - p_1$. The exposure was ¼ sec.; the size of the display was 6°.

Results. The thresholds obtained are shown by the curve labelled 'experienced' in Fig. 9. All adjacent pairs of thresholds are significantly different except for the 8 vs. 16 pair. Clearly the number of bars had an important and non-linear effect on detection.

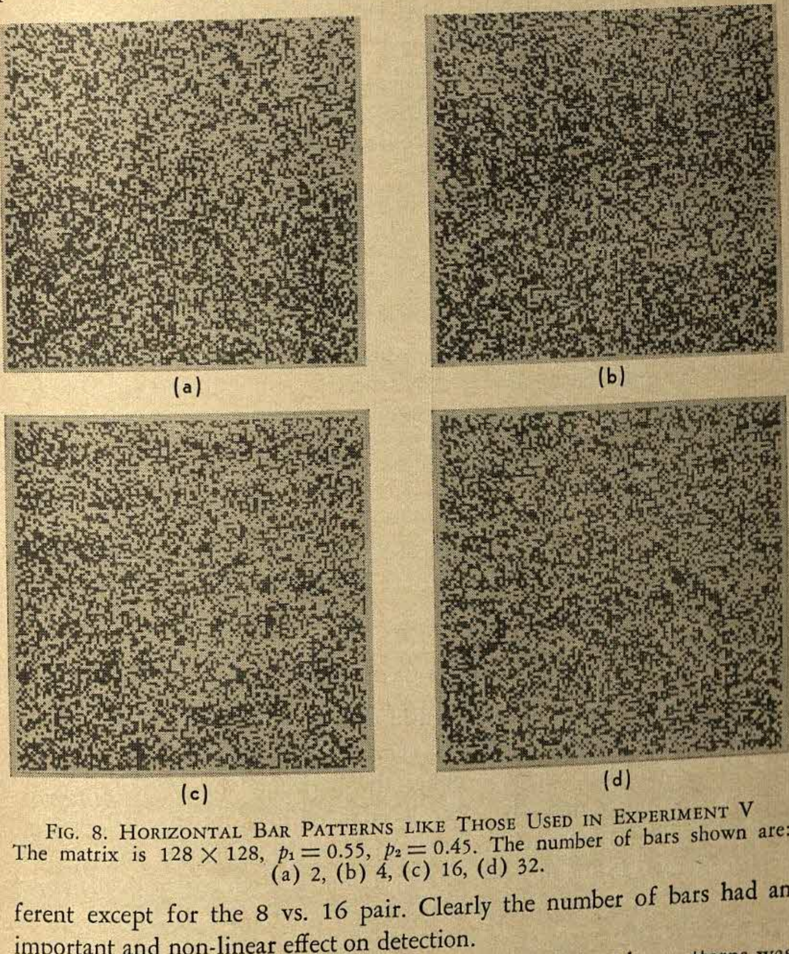

(a) (b) (c) (d)

FIG. 8. HORIZONTAL BAR PATTERNS LIKE THOSE USED IN EXPERIMENT V
The matrix is 128×128, $p_1 = 0.55$, $p_2 = 0.45$. The number of bars shown are: (a) 2, (b) 4, (c) 16, (d) 32.

It seemed possible that some of the advantage of the 8-bar patterns was the result of the *O*s' much greater experience with them. As noted above, evidence for a long-term learning effect exists. We were suspicious that this learning effect might reflect not only a general improvement in detecting dot-patterns, but also a specific improvement in detecting the standard

8-bar pattern. To check this misgiving, we repeated the experiment with five naïve *O*s. The results are shown in Fig. 9. The naïve group was less sensitive than the well-practiced group, but the differential effect of the number of bars was essentially the same for the two groups.

The results of this experiment are very different from the expected performance of the ideal detector. If the detector knows in advance—as our *O*s did—how many bars there will be, it is equally sensitive for any number of bars. On the other hand, if the detector behaves as if the pattern always has 8 bars, it will detect 8-bar patterns very well but will exhibit chance performance when confronted with 2-, 4-, 16-, or 32-bar patterns. Neither of these extremes describes the result obtained. Clearly a model for the behavior observed must be based on a more sophisticated statistical process.

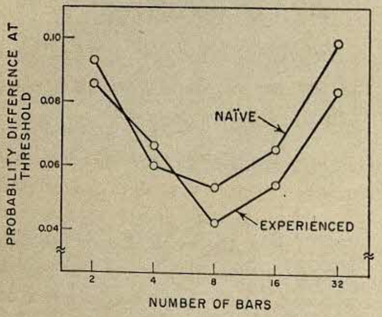


FIG. 9. DETECTION THRESHOLDS FOR EXPERIMENT V

EXPERIMENT VI: LOCATION OF CONTOUR

In the standard 8-bar pattern used in the previous experiments, the first 16 rows of the 128 × 128 matrix comprised the first bar, the next 16 rows the second bar, etc. For either the horizontal or vertical pattern, there was no uncertainty about the location of the contours of the bars. In Experiment VI uncertainty was introduced by randomly varying the number of rows in the first bar, and by adding a ninth bar, with the same p-value as the first, to fill out the square matrix. This is equivalent to removing the first g rows ($g < 16$) and placing them at the other side of the matrix. One might say that the 'phase' of the pattern was random.

Procedure. The *O*s, the five used throughout this study, were given 50 trials with displays in random phase at the four probability-values used in Experiment V.

Results. The resulting detection-curve was compared with the curve for the 8-bar patterns from Experiment V. The thresholds are 0.044 and 0.045, respectively, and are not significantly different. Since the brightness contrasts of the displays in Experiments V and VI matched very closely, and since it seems very unlikely that the long-term learning effect was an important factor in this comparison, it appears that the phase of the display has no effect on human detection. In contrast, phase has a strong effect on the ideal detector. The detector presented earlier has no way to accommodate for unknown phase. It would have to proceed as if the phase were known. It can be shown that this would more than double its threshold.[9]

EXPERIMENT VII: MULTIPLE EXPOSURES

In all of the experiments thus far reported, each judgment was based on a single display. In Experiment VII several displays were presented in rapid succession before *O* was asked for a judgment.

Procedure. Film strips were prepared containing several sets of 12 independently generated displays, all in the same orientation and all with the same display-parameters. Another film strip served as a control; it was identical to the first except that each set consisted of 12 repetitions of exactly the same display. The single display for each such set was chosen at random from the 12 in the corresponding set on the experimental film. The films were presented on a standard 16-mm. projector, at a rate of 16 frames per sec. Thus, each set of 12 frames had a duration of ¾ sec. After each set, the projector was stopped until *O* responded.

For both the experimental and control conditions, 48 sets of displays were prepared at each of two values of p_1, 0.515 and 0.531, with $p_2 = 1 - p_1$. The standard 128-line, 8-bar pattern was used. The five *O*s were tested three times with each film, yielding 144 judgments per *O* for each condition.

Results. The results averaged over *O*s are shown in Table I. For $p_1 =$ 0.515, the difference between experimental and control conditions is significant at the $\frac{1}{10}\%$ level.

We had hoped to be able to project the film at a rate fast enough so that individual dots in the pattern would fuse temporally, with the bars appearing as alternate shades of gray. At the rate used, however, fusion did not occur; rather, the dots appeared to scintillate. Nevertheless, *O*s were obviously able to make use of the additional information in the experimental condition. We have some preliminary evidence that at slower presentation rates, *e.g.* two per sec., the information is less readily assimilated, but further work must be done before more definite statements can be made about the effect of rate of presentation.

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[9] An ideal detector can be specified for the case of unknown phase. Its threshold would be the same for stimuli of known and unknown phase, since it would make no use of phase information. This threshold would be intermediate between the two thresholds of the known-phase detector confronted by displays of known and unknown phase respectively. The threshold of the unknown-phase detector varies directly with $(pq)^{\frac{1}{2}}$ and inversely with $N^{\frac{1}{2}}$. It also varies with the number of bars, but the effect is slight, and monotonic—not at all like the empirical curve of Fig. 9.

DISCUSSION

There is ample evidence that the detectability of bar-patterns in dot-matrices depends on the statistical properties of the pattern. To a first approximation the threshold is directly proportional to the standard deviation of the dot-density distributions. The threshold was seen to be directly proportional to $(pq)^{\frac{1}{2}}$ in Experiment II, and, roughly, inversely proportional to $N^{\frac{1}{2}}$ in Experiment IV. The threshold of the ideal statistical detector has the same properties as the thresholds obtained in Experiments II and IV, but is much lower. Thus, we may say that for a fixed pattern, and a fixed exposure, our *O*s behave like the ideal detector, except for sensitvity. Tanner and Swets obtained similar results in their comparison of *O*s with an optimal detector in a simple task of discriminating brightness, and Marill obtained an equivalent result in a simpler auditory task.[10] The difference in sensitivity has been attributed to an internal noise (variance) that is inherent in human detectors and that adds to the noice in the input signal. It is perhaps more suggestive to recall that ideal detectors are built to handle a very limited set of signals while humans are designed to function in a wide variety of circumstances. Some loss of sensitivity usually accompanies an increase in the bandwidth (variety of permissable signals) of a receiver.

TABLE I

MEAN PROPORTION OF CORRECT JUDGMENTS, EXPERIMENT VII

Condition	Probability-value (p_1; $p_2=1-p_1$)	
	.515	.531
Experimental	.72	1.00
Control	.62	.97

Experiments V and VI provide striking examples of the differences between the human *O* and the ideal detector. Unlike the ideal detector, the human appears to make no use of phase information, and to be very sensitive to the number of bars or contours on the display. The latter fact is easy to understand at a rough, descriptive level: the information about the pattern orientation seems to exist mainly at the contours, so there seems to be relatively little information when there are few contours, while with a great many bars, there are not enough lines per bar to 'establish' the contours. It is difficult, however, to devise a model that fits this description. Some sort of scanning process for locating contours seems to be indicated. (It should be noted that Selfridge's and Dinneen's elementary processes were not designed for threshold-patterns and are not applicable here.)[11]

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[10] W. P. Tanner, Jr., and J. A. Swets, A decision-making theory of visual detection, *Psychol. Rev.*, 61, 1954, 401-409; T. M. Marill, Detection theory and psychophysics, *MIT Research Laboratory of Electronics, Technical Report 319,* 1956.

The effect of dot-separation is not clear. A comparison of Experiments IV A and B shows that when all parameters are held constant except dot-separation, and consequently, display-area, the displays with the smaller dot-separation yielded lower thresholds. On the other hand, no significant changes in threshold were obtained in Experiment III, in which the dot-separation, dot-size, and display-area (in terms of the proximal stimulus) were varied jointly. As noted above, the effect might be expressed in terms of the ratio of dot-size to dot-separation. In Experiment III this ratio was constant, while in Experiment IV A it varied from 0.21 to 1.00, with smaller ratios giving higher thresholds. For ratios greater than unity, it is reasonable to suppose that larger ratios give higher thresholds, so that the optimal ratio is probably in the neighborhood of unity. Further work is necessary to clarify the interdependent roles of dot-separation and dot-size.

In a sense the task discussed in this paper is one of brightness discrimination, and yet the relation of our results to those obtained in brightness discrimination is not clear. Our thresholds are higher, but of the same order of magnitude as those obtained with uniform fields.[12] Yet the effects noted in our experiment are much different from those obtained with uniform fields. In our experiments, brightness is a linear function (with negative slope) of the number of dots per unit area; hence for fixed size and separation, brightness is linearly related to p, the average dot-probability. In Experiment II, the threshold was seen to be an inverted U-shaped function of p. For uniform fields of the same average brightness, the threshold decreases linearly, as p increases, but the decrease is slight. The statistical properties of our displays would seem to be more important than their average brightness.

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[11] Selfridge, *op. cit.*, 91-93; Dinneen, *op. cit.*, 94-100.

[12] Photometric measurements were made of several patches from each condition in Experiment II. These measurements can be described by the equation: $I = 1 + 1.5\ (1 - p)$ where I is the brightness of the patch in foot-lamberts, and p is the probability of a dot in the patch. For $p = 0.5$, $I = 1.75$. Standard data obtained with uniform fields (*e.g.* Fig. 30, in S. S. Stevens (ed.), *Handbook of Experimental Psychology*, 1951, 954) show that at this brightness, log $\Delta\ I/I$ at threshold is -1.7, whence the equivalent $\Delta p = 0.035$.

The experiments reported in this paper are exploratory; they raise more questions than they answer. The curious effect noted when the number of bars was varied suggests a number of further experiments. The effect of grain has not been clearly defined. The interactions among the major variables have not been studied, and some variables, such as brightness contrast, were not studied at all. A significant aspect of the study is its demonstration of the potential utility of computer-generated, statistically controlled dot-pattern as stimulus objects for studying pattern-detection.

SUMMARY

Seven experiments are reported on the detectability of statistically determined bar-patterns in dot-matrices. Exposure-durations below but not above 1 sec. had a differential effect. Individual differences were slight. For fixed matrix-size and varying dot-probabilities, thresholds of detection were proportional to the standard deviation of the binomial distributions governing the occurrences of the dots. For fixed dot-probability and varying grain (number of rows or columns in the square matrices), the thresholds were inversely related to the grain, but different forms of the relationship were obtained for two different ways of altering grain. Over a wide range, visual angle had no significant effect on detectability. The curve relating the threshold to the number of bars in the pattern was U-shaped. Patterns with random phase and fixed phase were equally detectable. A small but significant improvement in detectability resulted from a motion-picture display of several independent samples of a pattern. Differences between the performance of human *O*s and that of an ideal statistical detector are discussed.

ON DISCRIMINATING THE RATE OF VISUAL FLICKER AND AUDITORY FLUTTER

By J. W. GEBHARD and G. H. MOWBRAY, The Johns Hopkins University

Previous studies on difference-limens (*DL*) of visual flicker and auditory flutter have shown that *O* can accurately detect small variations in rate.[1] Within both of these modalities, the relationship between frequency (F) and the smallest discriminable increment in frequency (ΔF) is described by the flicker- and flutter-functions shown in Fig. 1. While the sensory mechanisms of the eye and the ear appear to handle rate-discrimination in different ways over the short range of frequencies where they can be compared, the accuracy with which this judgment is made is nearly the same. The relative *DL*s ($\Delta F/F$) for flicker vary from 0.005 to 0.024 in the frequency-range of 1 to 45 c.p.s. In the same range of frequencies for flutter, $\Delta F/F$ varies from 0.008 to 0.02.

The question now asked is: What happens when *O* is required to match the rates of flicker and flutter? It was supposed that this task would also be easy, resulting in some sort of sensory compromise between the two kinds of *DL*-functions shown in Fig. 1. Surprisingly, cross-sensory rate-matching proved most difficult, and attempts to reduce the size of the errors served only to demonstrate how well the mechanisms of rate-discrimination of the eyes and ears are insulated.

METHOD

Apparatus. The arrangement of the apparatus is shown in Fig. 2. Two independent variable-frequency pulse-generators supplied the square-wave oscillations for the standard and comparison stimulus-channels. One channel provided an electrical-input to a single Sylvania R1131C glow-modulator tube. The other channel operated an electronic gate that interrupted the thermal-noise from a generator. A three-position telephone switch without detents, operated by either *E* or *O*, allowed *O* to sample the flicker or the flutter without overlap. Additional arrangements made

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* Received for publication December 17, 1958. From the Applied Physics Laboratory, Silver Spring, Maryland. This report was prepared under Contract NOrd 7386 between the Bureau of Ordnance, U.S. Navy, and The Johns Hopkins University.

[1] G. H. Mowbray and J. W. Gebhard, Differential sensitivity of the eye to intermittent white light, *Science*, 121, 1955, 173-175; J. W. Gebhard, G. H. Mowbray, and C. L. Byham, Difference-limens for photic intermittence, *Quart. J. exp. Psychol.*, 7, 1955, 49-55; Sensitivity to changes in the interruption rate of white noise, *J. acoust. Soc. Amer.*, 28, 1956, 106-110.

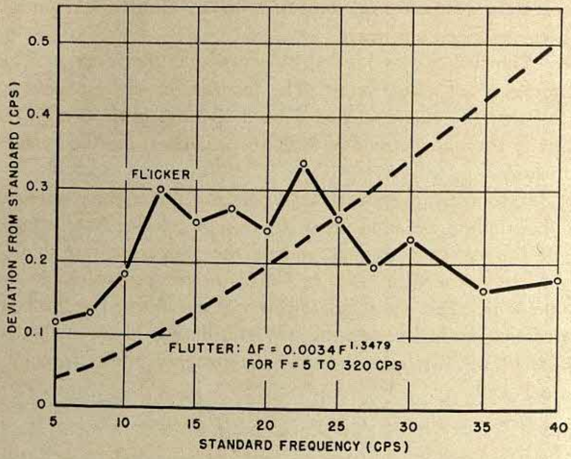

FIG. 1. RELATION BETWEEN PHOTIC FLICKER AND AUDITORY FLUTTER

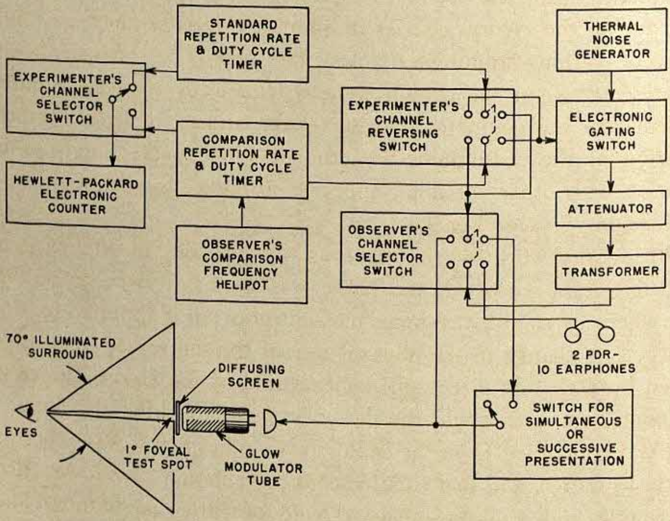
STANDARD REPETITION RATE & DUTY CYCLE TIMER
EXPERIMENTER'S CHANNEL SELECTOR SWITCH
COMPARISON REPETITION RATE & DUTY CYCLE TIMER
HEWLETT-PACKARD ELECTRONIC COUNTER
OBSERVER'S COMPARISON FREQUENCY HELIPOT
EXPERIMENTER'S CHANNEL REVERSING SWITCH
OBSERVER'S CHANNEL SELECTOR SWITCH
THERMAL NOISE GENERATOR
ELECTRONIC GATING SWITCH
ATTENUATOR
TRANSFORMER
2 PDR-10 EARPHONES
SWITCH FOR SIMULTANEOUS OR SUCCESSIVE PRESENTATION
70° ILLUMINATED SURROUND
EYES
DIFFUSING SCREEN
1° FOVEAL TEST SPOT
GLOW MODULATOR TUBE

FIG. 2. SCHEMATIC BLOCK DIAGRAM OF THE APPARATUS

it possible for the stimuli to be present continuously. The rate of intermittence was varied by a linear, multi-revolution potentiometer. The frequency and duty-cycle of each channel were continuously measured on an electronic counter. The circuits used have been described elsewhere.[2]

Procedure. The flicker was viewed binocularly at the fovea as a circle of white light subtending 1° of visual angle. The intensity of the test-spot could be varied by filters, but the 70° surround had a fixed luminance of 50 mL. The flutter was heard binaurally through Permoflux PDR-10 earphones, and the loudness was varied by an attenuator.

The *DL*s for comparing the rate of flicker and flutter were measured in the following by the method of adjustment. In this procedure, *E* set either a flicker- or flutter-rate on the standard channel, and set the comparison-rate above or below the standard by a random amount. *O* then varied the comparison-rate in the appropriate direction until it matched the standard. He was not allowed to bracket the standard. If he signaled that he had passed the standard, he was given another trial. The data are the means of the discrepancies from the standard rates, expressed in cycles per second (c.p.s.).

RESULTS

(1) Variables in cross-sensory matching. It was quickly discovered that the *DL*s for matching flicker and flutter were often 10 times larger than either the flicker-to-flicker or flutter-to-flutter matches reported earlier. Since training, by the use of extensive coaching, had no appreciable effect in reducing the errors, a series of short tests was conducted to assess the effect of various stimulus-variables. The duty-cycle, intensity, and order of presentation of the stimuli were systematically changed. Simultaneous presentation was compared with successive, and in the latter condition, the duration of the pulse-trains was studied. No variable, singly or in combination with other variables, appeared to reduce the thresholds to values approaching those in Fig. 1.

During the work on simultaneous presentation, an attempt was made to detect inter-sensory beats. None was found. It was noted, however, that when the sound was under the control of *O*, it appeared to 'drive' the light; *i.e.* a change in the physical rate of the flutter was immediately followed by a change in the phenomenal rate of the flicker. Under none of the conditions used could the flicker be made to 'drive' the flutter.

It was concluded from the exploratory studies that it was easier to match flicker to flutter, and that simultaneous presentation gave lower thresholds. Duty-cycle and stimulus-intensity were of little importance. Therefore, for the four short experiments that follow, a duty-cycle of 0.5 was adopted, the flutter was set at a sound pressure-level of 35 db. re 0.0002 microbar, and the flicker had a fused luminance of 100 mL.

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[2] R. G. Roush and E. T. Urbanski, Universal medical timer and pulse stimulator, *Electronics*, 26, 1953, 1954-1957.

(2) Simultaneous matching to fixed standard rates. Flutter was matched to flicker, and the reverse, by placing either rate on the standard channel and giving *O* control of the other. The stimuli could be turned on and off but, when on, both were continuously present. Two trained *O*s made five matches each at five frequencies: 5, 10, 20, 30, and 40 c.p.s., randomly assigned. Their combined results are shown in Fig. 3. The *DL*s are found to be larger when flutter is matched to flicker. This difference between the means of the two curves is statistically significant at the 5% level of confidence when evaluated by the *t*-test.

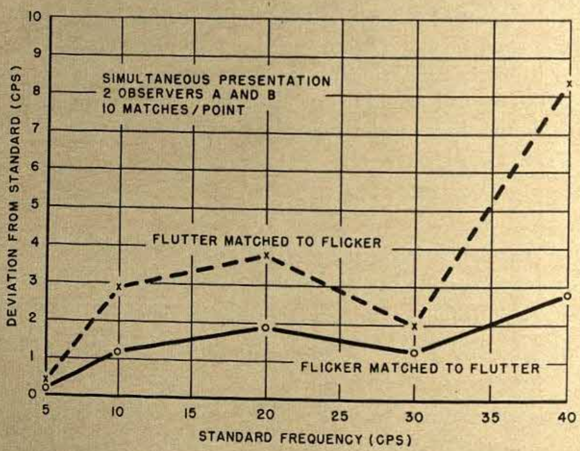

FIG. 3. THE DIFFERENCE-LIMENS FOR SIMULTANEOUS MATCHES TO FIXED STANDARD RATES

(3) Simultaneous matching to variable standard rates. It was thought that possibly the *O*s had learned the rates of the fixed stimuli, especially the auditory standards. Therefore, it was decided to vary the standard rate over the entire range from 5 to 40 c.p.s. Matches were made as before, except that the frequency of the standard was set randomly to values within six frequency-groups of six rates each: 5-10, 11-16, 17-22, 23-28, 29-34, and 35-40 c.p.s. The same two *O*s were used. The results are plotted at the center of each group as shown in Fig. 4. The thresholds are higher than those shown in Fig. 3, but the functions are similar, and the superiority of matching flicker to flutter is again shown. The mean dif-

ference between the measured points of the two curves is greater than zero at the 5% level of confidence when evaluated by the *t*-test.

(4) Successive matching to fixed standard rates. Two sets of measurements were taken to evaluate the effect of successive presentation of the stimuli. They differ in the number of frequencies and measurements, and in *O*s. In both experiments, *O* successively sampled the flicker and flutter in any desired manner by means of a selector-switch with an 'off' position. He controlled completely the order of the sampling, and the duration of the light, the sound, and the pause between them. Typically *O* looked at first one and then the other of the stimuli several times, decided whether the comparison was faster or slower, set in an appropriate adjustment, and resampled the stimuli. This sequence was repeated as many times as *O* wished, as there was no time-limit on making the matches. *O* also determined his rest-periods. The frequencies and *O*s were randomly assigned, and *E* set the comparison off the standard an equal number of times above and below the standard in a counterbalanced order.

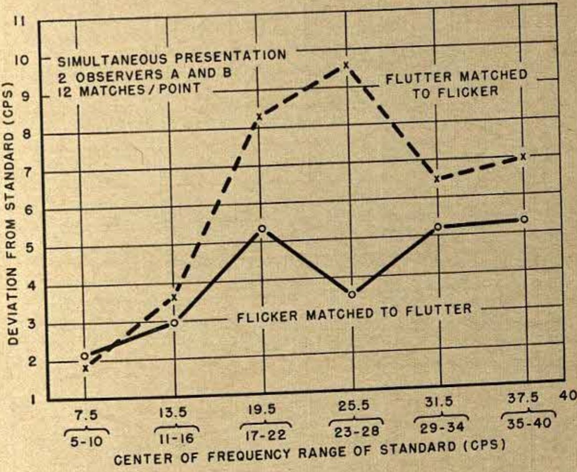

FIG. 4. THE DIFFERENCE-LIMENS FOR SIMULTANEOUS MATCHES TO VARIABLE STANDARD RATES

The first set of discrepancies from the standard rate is shown in Fig. 5 for the same *O*s that were used in the simultaneous comparisons. The means of 16 matches are shown at each of 13 frequencies. In the second set, shown in Fig. 6, one of the *O*s was the same, and the other, while practiced, was new. Twenty-four matches are plotted at each of eight fre-

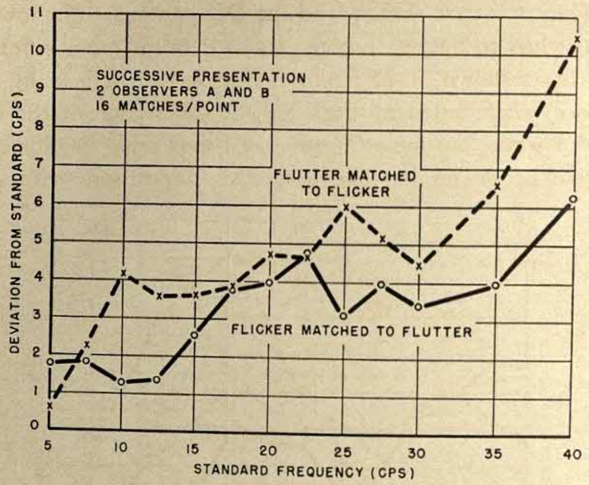

FIG. 5. THE DIFFERENCE-LIMENS FOR SUCCESSIVE MATCHES TO FIXED STANDARD RATES
(First set of measures.)

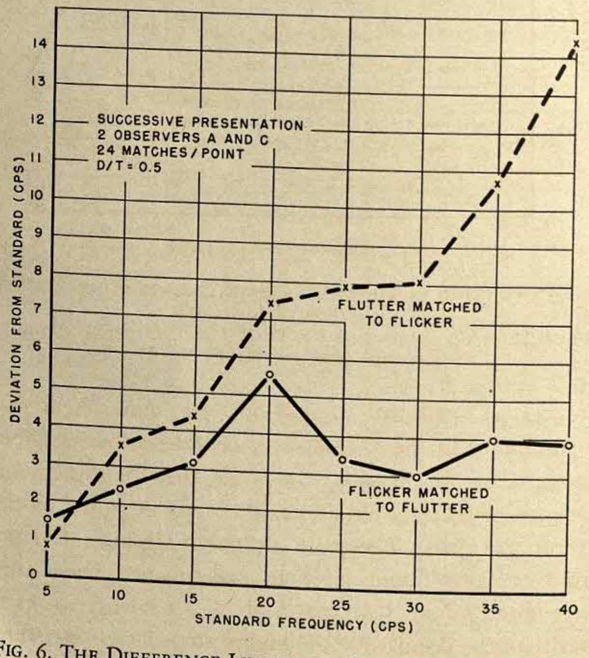

FIG. 6. THE DIFFERENCE-LIMENS FOR SUCCESSIVE MATCHES TO FIXED STANDARD RATES
(Second set of measures.)

quencies. The curves are similar, and the *DL*s continue to be lowest when flicker is matched to flutter. In both sets of data the mean differences between the curves shown in the figures are larger than zero at the 1% level of confidence when tested by the *t*-test. A comparison of the curves in Figs. 3 and 5 where the same *O*s were used shows that simultaneous comparisons give better results than successive comparisons. A test of the

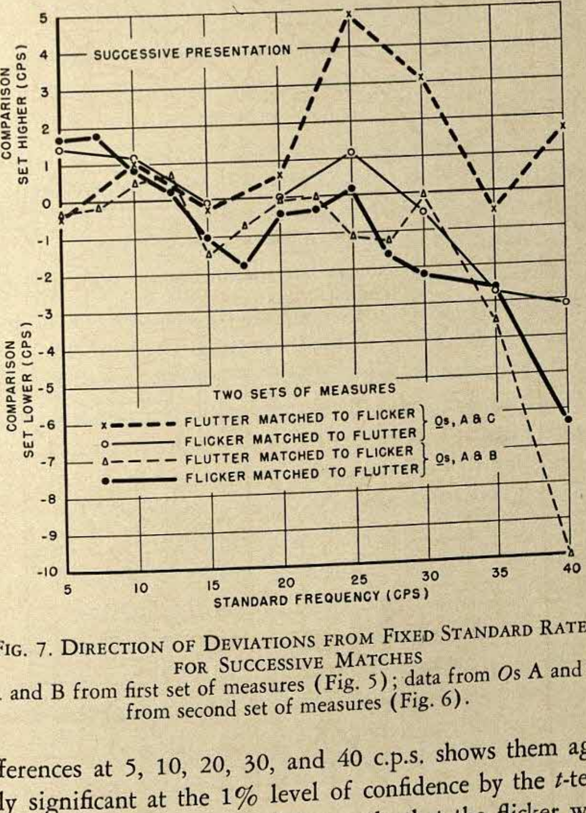

FIG. 7. DIRECTION OF DEVIATIONS FROM FIXED STANDARD RATES FOR SUCCESSIVE MATCHES
A and B from first set of measures (Fig. 5); data from *O*s A and C from second set of measures (Fig. 6).

mean differences at 5, 10, 20, 30, and 40 c.p.s. shows them again to be statistically significant at the 1% level of confidence by the *t*-test.

It was thought, at the outset of the work, that the flicker was usually judged to be slower than its physical rate; *i.e.* as measured by the flutter, a constant error was made. Therefore, the mean algebraic deviations from the standard were also found for both experiments. These are seen in Fig. 7, where three of the four curves show a tendency to set the comparison lower than the standard at the higher frequencies. We were unable, therefore, to measure the phenomenal rate of flicker in terms of the phenomenal rate of flutter in any consistent way.

DISCUSSION

The literature abounds with accounts of inter-sensory effects,[3] and some work, like that of Maass, suggests interesting interrelations between rate-processes in vision and audition.[4] Unfortunately, this work is non-quantitative, and is useful only to point out problems that should be redone with modern methods and equipment. The Russian work on influencing vision by auditory stimuli is also interesting, but lacks both convincing data and confirmation.[5]

In interpreting our work, we feel no doubt that inter-sensory rate-matching between the eye and the ear is much poorer than intra-sensory matching for either of these sensory processes. In the range of 5 to 40 c.p.s., the relative *DL*, $\Delta F/F$, for intra-sensory matches varies from 0.01 to 0.024. For inter-sensory rate-matches in the same frequency-range, $\Delta F/F$ varies from 0.03 to 0.42. Pollack has also found that "cross-modality matching (between the eye and the ear) is more variable and less accurate than intramodality matching."[6] This difference must be attributed to the inherent difficulty of the task, since neither training nor manipulation of the stimulus-variables had a marked effect on reducing the errors. Discrimination of rate appears to be a different process for the eye and the ear. This was shown first by the different functions seen in Fig. 1, and now by the inability of *O* to match flicker and flutter accurately. The degree of sensory isolation that exists between the eye and the ear for comparison of rate appears to be lacking between the ear and the skin. *DL*s obtained for the matching of rates separately presented to the ear and the skin fall in the same range as those found for the skin itself. This is perhaps due to the fact that the ear and skin handle rate discrimination in much the same way.[7]

For the finding that simultaneous presentation gave somewhat lower *DL*s than successive presentation, however, Pollack gives us no support. He found no clearly demonstrable differences between methods.

Of the two intermittent processes, flicker appears to be the more unstable. The first indication of this is based on the purely subjective observation that flutter influences the apparent rate of flicker, but not the reverse. We are not alone in having noted this. Pollack has also observed the apparent 'driving' effect, and, like us, he was unsuccessful in an attempt to measure it directly. More objective evidence of the instability of flicker in the presence of flutter is the finding that the flutter-to-flicker *DL*s are significantly higher at all frequencies. Pollack, too, found variability to be greater when *O* had the sound under his control. A related case of flutter affecting flicker has been presented by Ogilvie who reports that the *CFF* may be raised slightly by the simultaneous presentation of interrupted random noise.[8]

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[3] T. A. Ryan, Interrelations of the sensory systems in perception, *Psychol. Bull.*, 37, 1940, 659-698; G. M. Gilbert, Intersensory facilitation and inhibition, *J. gen. Psychol.*, 24, 1941, 381-407.

[4] Harald Maass, Ueber den Einfluss akustischer Rhythmen auf optische Bewegungsgestaltungen, *Arch. ges. Psychol.*, 100, 1938, 424-464.

[5] I. D. London, Research on sensory interaction in the Soviet Union, *Psychol. Bull.*, 51, 1951, 531-568, esp. 535 ff.

[6] Irwin Pollack, Personal communication.

[7] Mowbray and Gebhard, Sensitivity of the skin to changes in rate of intermittent mechanical stimuli, *Science*, 125, 1957, 1297-1298.

If we accept the fact that flicker may somehow be altered by flutter, we face an unresolved problem. For the successive case, the influence of the auditory stimulus must carry over a time-delay. Worse, the carry-over is not clearly one of order, since either the light or the sound, as freely controlled by *O*, could come first. *O* might have introduced a bias here by tending to sample only in one direction. If he did so, there is no way of analyzing our data to reveal such a bias. *O* generally kept the pause between stimuli short, but it is possible that the carry-over effect might be reduced if long pauses were interposed between stimuli.

SUMMARY

Four sets of measurements were made of the ability of *O* to match the rates of intermittent white light and white noise. Cross-sensory rate matching is less accurate than intra-sensory rate matching. The relative *DL*s for inter-sensory matches vary from 0.01 to 0.024, whereas cross-sensory matches produce relative *DL*s of 0.03 to 0.42. Matches are more accurate when flicker is matched to flutter, and simultaneous presentation of the stimuli gives smaller matching errors than successive presentation.

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[8] J. C. Ogilvie, Effect of auditory flutter on the visual critical flicker frequency, *Can. J. Psychol.,* 10, 1956, 61-68; The interaction of auditory flutter and *CFF;* The effect of brightness, *idem,* 207-210.

A QUANTITATIVE STUDY OF REVERSAL OF CLASSICAL LIGHTNESS-CONTRAST

By HARRY HELSON and FREDERICK H. ROHLES, JR., University of Texas

In classical lightness-contrast, stimuli on dark ground are perceived to be lighter than they are on light ground, the rule being that differences between adjacent stimuli are enhanced by contrast.

As long ago as 1874, Von Bezold showed that colored areas overlaid with white arabesques appeared lighter than the same areas overlaid with black lines contrary to the classical law of contrast.[1] Using Von Bezold's chromatic designs, and also gray squares, Burnham found that most *O*s reported the 'color mixture' or 'spreading effect,' as Von Bezold called it,[2] if they viewed the designs as wholes and adopted a casual, non-rigid fixation.[3] A less compelling stimulus, a face seen either as a gray outline or as a solid figure, was employed by Riedel to demonstrate reversal of contrast,[4] but in Newhall's study only 6 out of 17 *O*s reported reversal in the case in the outline-stimulus and none with the solid figure.[5] In spite of the fact that Von Bezold's designs are very striking and elicit reports of reversal of contrast from almost all *O*s they do not, as they stand, lend themselves to quantitative study. For the purpose of measurement, stimulus-patterns are needed which are as compelling as the Von Bezold figures, yet can be varied with respect to the conditions responsible for this anomalous phenomenon.

A stimulus-pattern which can be varied dimensionally and which is as compelling as the Von Bezold designs was devised by the senior author some years ago and is reproduced in Fig. 1.[6] It consists of white vertical lines, 1-mm. wide, drawn on one-half of a gray ground and black vertical lines drawn on the other half of the ground. With a separation of 3 mm.

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* Received for publication December 30, 1958.

[1] W. von Bezold, *Die Farbenlehre*, 1874, 118, 200, 215, 245f. Some of Von Bezold's designs in color have been reproduced by R. M. Evans (*Introduction to Color*, 1948, 181, 192) and by R. W. Burnham (Bezold's color-mixture effect, this JOURNAL, 66, 1953, 379).

[2] Burnham, *op. cit.*, 378.

[3] *Idem*, 385.

[4] G. Riedel, Ueber die Abhängigkeit optischer Kontraste von Gestaltsbedingungen, *Neue psychol. Studien.*, 10, 1937, 1-44.

[5] S. M. Newhall, The reversal of simultaneous brightness contrast, *J. exp. Psychol.*, 31, 1942, 393-409. This article describes cases of anomalous contrast and assimilative effects reported in the literature up to the time of its publication.

[6] The original of this figure has been shown to fairly large groups as a lecture demonstration and has never failed to elicit reports of reversal of classical contrast from almost everyone who has seen it. The effect is favored by distance and hence is good for classroom demonstrations.

between the lines no one has failed to perceive the gray overlaid with white lines to be lighter than the gray overlaid with black lines, unless by special effort the gray stripes are so isolated from their context that they

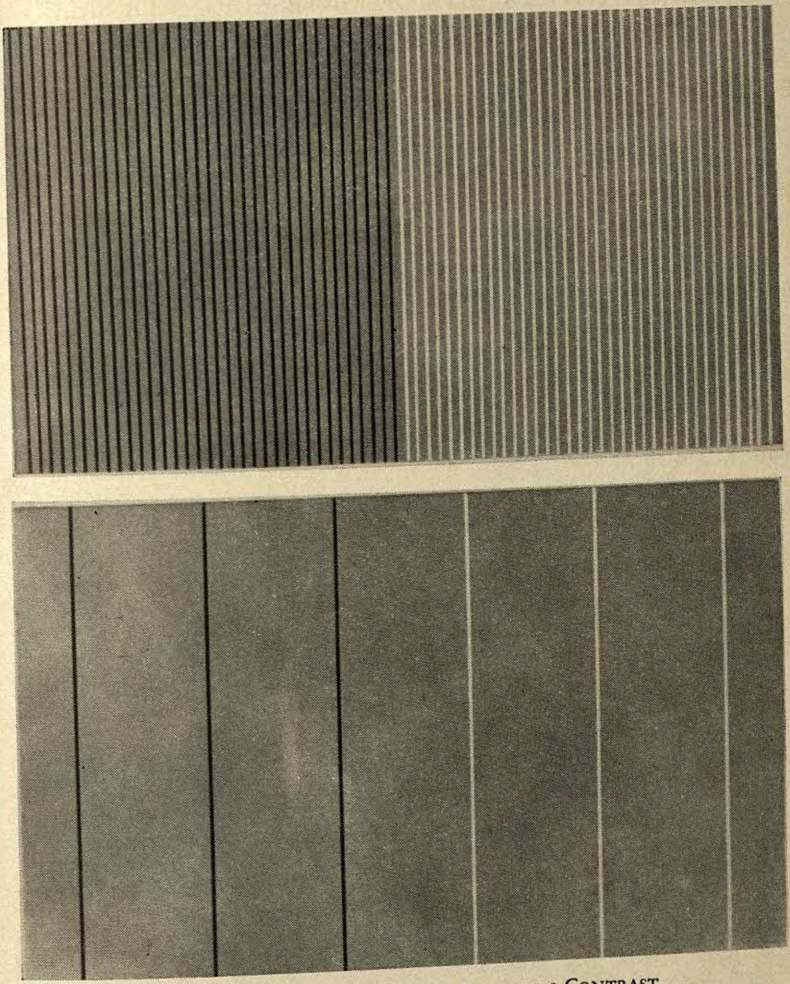

FIG. 1. REVERSAL OF CLASSICAL LIGHTNESS-CONTRAST

exhibit the usual contrast-effects. It is apparent that this pattern is capable of dimensional variation: the distance between the vertical lines may be varied, keeping their width constant, or the width of the lines may be varied, keeping the background between the lines constant, or both width of lines and of background may be varied simultaneously. Still other stimulus-conditions may be varied, such as lightness of the background and of the overlying lines. In this study the results of varying only the separation between the lines will be reported, while in a subsequent study effects of varying widths of the lines and of the intervening gray strips will be reported.

METHOD AND PROCEDURE

Stimulus-materials. The stimulus-materials consisted of 14 gray cards, 7× 11 in., with daylight reflectance of 36% and approximately equal to Munsell N 6.5/0. On one-half of the card, vertical white lines 1 mm. in width were drawn and on the other half similar black lines. The 14 cards were identical except for the distance between the vertical lines, which varied from 3 to 55 mm. in steps of 4 mm. The separation of 3 mm. subtended a visual angle of 3′ 25″ while the 55-mm. separation subtended a visual angle of 1° 3′ at a viewing distance of 3 m. The cards were presented with long axis horizontal in an aperture 11 × 7 in. in the center of a homogeneous gray field 44 × 30 in. The surround was of the same material as the stimulus-cards. The aperture was equipped with a manually operated shutter which was raised for 3 sec. and then lowered while the *O*s gave their judgments and the stimulus-cards were changed. The cards and the room in which the experiment was conducted, were evenly illuminated by two banks of Cool White 40-w. fluorescent lamps. The walls of the room were light gray and contained no windows, thus insuring uniform achromatic illumination at all times during the observations.

Instructions. Actual procedure is best understood by reference to the instructions given the *O*s, which were as follows:

> The purpose of this investigation is to study the way in which certain grays are perceived. I will present a series of cards, one at a time, such as the one which is now before you. You will notice that the left side of the card appears lighter than the right side. [Here a card with line separations of 5 mm. was shown to the *O*s. Since in preliminary observations not a single *O* reported that the gray on black-lined side of this card was lighter than the gray on the white-lined side, our instructions did not bias the experimental *O*s.] Before I present each card, I will ask you to judge the one gray with respect to the other gray. Using one of the categories on this card, give your answer.
>
> If I ask you to judge the left gray with respect to the gray on your right and in your opinion the left gray looks *much lighter* than the gray on the right, say 'much lighter.' If I ask you to judge the right gray with respect to the gray on the left, and the gray on the right looks *very much darker,* say 'very much darker.'

Judgments of lightness were reported in terms of a comparative rating-scale containing 11 categories as follows:[7] (1) very, very much lighter; (2) very much lighter; (3) much lighter; (4) lighter; (5) a little lighter; (6) equal; (7) a little darker; (8) darker; (9) much darker; (10) very much darker; and (11) very, very much darker. The categories were typed on a card to which *O* could refer when necessary. The set of 14 stimuli was first judged for practice, after which the experimental series was begun. Ten *O*s, 5 men and 5 women, served in the experiment, and the data represent a total of 560 judgments—10 *O*s × 4 series × 14 stimuli.

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[7] Harry Helson and W. C. Michels, The use of comparative rating scales for the evaluation of psychophysical data, this JOURNAL, 67, 1954, 321-326.

From the instructions given the *O*s it is apparent that the psychophysical procedure consisted of a modification of the traditional method of paired comparisons. Light- and dark-lined sides of each card were compared so that the *same* separations between lines were always judged rather than every separation with every other, as in the usual method of paired comparisons. This procedure was adopted for two reasons: (1) to facilitate analysis of variance of the data; (2) to determine by least squares fit the separation between white and black lines at which the two sides of the stimulus-cards would be judged equal. The cards were presented in random order so far as separation between lines was concerned, but with control over position such that if the white-lined side of a card appeared on *O*'s right in one series, it was presented on his left in the second series. Each *O* judged each card four times: twice in terms of the white-lined side and twice in terms of the black-lined side in counterbalanced, ABBA, order. Thus each side of the cards was judged when to the right and to the left of *O*'s line of regard.

Treatment of data. The judgments of the 10 *O*s in the 4 series were averaged for each stimulus-card by translating the category-scale into a numerical scale by assigning a value of 10 to very, very much lighter, 9 to very much lighter, and so on down to 0 for very, very much darker. Judgments pertaining to the white-lined side of the cards were transformed into their complements to yield judgments equivalent to those of the black-lined side on the assumption usually made in reversing order of standard and variables that a judgment of a card in one order will elicit the symmetrically opposite judgment in the other order, *e.g.* very, very much lighter for the white-lined side implies that the black-lined side would be judged as very, very much darker. In accordance with this assumption, 10 becames 0, 9 becomes 1, and so on down the scale. This transformation is necessary in order to make a single analysis of variance of all the data and to test significance of differences between judgments of the stimulus-cards. The best values of the 'equal' judgment were derived from the judgments of the white- and black-lined sides by fitting by method of least squares to the following function originally proposed by Helson for absolute judgments and subsequently found to fit data obtained by use of comparative ratings:[8]

$J = K\ (X_i - A)/[X_i + (1+b)A/(1-b)] + 0.5K$, where K is the top value of the numerical scale, X_i refers to stimulus-separation, A is the adaptation-level and corresponds to the value of stimulus which elicits a judgment of equality, and b is the y-intercept of the linear function into which this equation is transformed for purposes of curve-fitting.[9] Comparison of the experimental and fitted values derived from the equations containing the constants given in Table I shows that a negatively accelerated function provides a good fit to these data. In addition an analysis of variance of the lightness judgments was made and the significance of the differences between judgments of all separations was determined.

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[8] Harry Helson, Adaptation-level as a basis for a quantitative theory of frames of reference, *Psychol. Rev.*, 55, 1948, 297-313.

[9] For details of fitting to this function, see Harry Helson and Philip Himelstein, A short method for calculating the adaptation-level for absolute and comparative rating judgments, this JOURNAL, 68, 1955, 631-637.

RESULTS

The data in Table I show unmistakably a strong reversal of classical contrast in that the gray overlaid with white lines is always reported *lighter* than the gray overlaid with black lines. Moreover, the closer together the lines are spaced, the greater is the lightening effect of the white lines and the greater is the darkening effect of the black lines. At 3-mm. separation, the white lines produce the effect of a very, very much lighter gray than the black-lined gray, and the black lines give the effect of a very, very much darker gray than the white-lined gray. Even at the widest separation employed in this study, there is still some spreading-effect of the white and black lines over the gray ground, as shown by the fact that with 55-mm. separation the average judgments are slightly below 5.0 (equal) for the black-lined side and slightly above 5.0 for the white-lined. Averaging the fitted values for equality-judgment, we find that the lines overlying the gray would have to be separated 62.65 mm. in order to obliterate the effect investigated here.

TABLE I

EXPERIMENTAL AND FITTED LIGHTNESS-VALUES OF BLACK-LINED AND WHITE-LINED GRAYS

Values in parentheses have been translated to take account of reversed order of standards and variables

Line separation (mm.)	Black-lined gray		White-lined gray		
	exper.	theor.*	exper.	compl.	theor.†
3	0.10	1.06	(9.80)	0.20	1.11
7	0.85	1.42	(9.05)	0.95	1.50
11	1.75	1.77	(8.20)	1.80	1.79
15	2.65	2.10	(7.50)	2.50	2.10
19	2.85	2.41	(7.35)	2.65	2.40
23	3.35	2.71	(7.15)	2.85	2.69
27	3.30	3.00	(6.60)	3.40	2.96
31	3.45	3.27	(6.45)	3.55	3.22
35	3.50	3.53	(6.35)	3.65	3.47
39	3.90	3.78	(6.20)	3.80	3.71
43	4.05	4.02	(6.10)	3.90	3.96
47	4.30	4.25	(5.90)	4.10	4.18
51	4.15	4.46	(5.95)	4.05	4.39
55	4.40	4.67	(5.65)	4.35	4.59

* Based on fitted equation: $J=[\![10(X_i-61.52)/(X_i+145.47)]\!]+5.0$.

† Based on fitted equation: $J=[\![10(X_i-63.78)/(X_i+153.47)]\!]+5.0$.

The clear-cut results of Table I are borne out by the analysis of variance of the judgments of lightness in Table II and by the tests of the significance of the differences between judgments of the 14 cards.[10] From Table

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[10] We are indebted to Dr. Robert Morin for advice in connection with this analysis.

II, it is seen that the separations between the lines exert a highly significant effect on the lightness of the grays. Since it does not matter whether the white (black) lines are perceived on the right (left) or on the left (right) there is no space-error at work in these data. There is, however, a highly significant interaction between the sides and *O*s, showing that *O*s may be affected differently by viewing the cards in their different positions.

TABLE II

ANALYSIS OF VARIANCE OF LIGHTNESS-JUDGMENTS

Source	df.	Mean square	Error term	*F*
(1) Separations	13	32.61	(1)×(3)	163.05*
(2) Sides	1	0.26	(2)×(3)	2.50
(3) Subjects	9	6.14	(1)×(2)×(3)	2.95*
(1)×(2)	13	0.16	(1)×(2)×(3)	0.77
(2)×(3)	9	0.65	(1)×(2)×(3)	3.22*
(1)×(3)	117	0.20	(1)×(2)×(3)	0.96
(1)×(2)×(3)	117	0.208		
Total	279			

* $P<0.01$.

TABLE III

MEANS, *SD*, AND SIGNIFICANCE-LEVELS OF THE DIFFERENCES IN LIGHTNESS AMONG THE VARIOUS LINE-SEPARATIONS

L-s. (mm.)	Line-separations (mm.)														Mean	SD
	3	7	11	15	19	23	27	31	35	39	43	47	51	55		
3		1%	1%	1%	1%	1%	1%	1%	1%	1%	1%	1%	1%	1%	0.150	.278
7			1%	1%	1%	1%	1%	1%	1%	1%	1%	1%	1%	1%	0.900	.718
11				1%	1%	1%	1%	1%	1%	1%	1%	1%	1%	1%	1.775	.901
15					ns	ns	1%	1%	1%	1%	1%	1%	1%	1%	2.575	.576
19						ns	5%	1%	1%	1%	1%	1%	1%	1%	2.750	.766
23							ns	ns	ns	1%	1%	1%	1%	1%	3.100	.735
27								ns	ns	ns	5%	1%	1%	1%	3.350	.614
31									ns	ns	ns	5%	5%	1%	3.500	.689
35										ns	ns	5%	ns	1%	3.575	.597
39											ns	ns	ns	ns	3.850	.477
43												ns	ns	ns	3.975	.402
47													ns	ns	4.200	.485
51														ns	4.100	.490
55															4.375	.441

ns=not significant at the 5% level of confidence

To determine the line-separations which elicit significantly different judgments when all separations are compared, significant differences among the cards were calculated from the means and standard deviations of the judgments and are given in Table III. From these data it is seen that the differences between line separations of 3, 7, and 11 mm. are significantly different from each other and also from all the wider separations. The reversal in contrast is therefore maximal with the three smallest separations between the lines used in this study. These results show that addition of white lines lightens and addition of black lines darkens the areas to a highly significant degree. Even at the widest separation (55 mm.), the two grays were not judged equal to each other, as shown by comparison of white-lined with black-lined judgments which differ by 1.25 category steps (5.65–4.40). Comparison of the original white-lined judgments with the black-lined judgments (Table I) brings out the differences between the two grays even more than does comparison of the transformed white-lined judgments with the black-lined judgments, as is done in Table III.

Discussion

It is difficult to 'explain' our results in terms of only one factor or process, although the facts are quite clear descriptively: narrow white lines lighten, and black lines darken, gray background over a wide range of line-separations (3–55 mm.). We know that if more light is sent to the eyes by any object, the object is not only brighter but also, as Katz pointed out, more pronounced, or lighter, if it is achromatic.[11] The gray overlaid with white lines obviously is responsible for a higher luminous flux than is the gray overlaid with dark lines. On this basis, the surface with white lines should look lighter, as a whole, than the surface with dark lines. Classical contrast is usually regarded as resulting from the mutual relations of areas within the field of view and hence as independent of total luminous flux so long as ratios of excitation from adjacent areas remain constant. At first sight, it might appear that the explanation of the Von Bezold effect is, therefore, to be sought in 'totalizing' perception, but this same mechanism has been invoked to explain contrast effects, such as enhancement of lightness in the Benary effect, where a gray partly imbedded within a black cross and perceived as belonging to it is lighter than a gray surrounded by more black but perceived as belonging to the adjacent white background.[12] The perception of figures or areas as wholes, therefore, may facilitate either classical contrast or assimilation, so we must seek for additional principles to account for both contrast and assimilation or spread-effects.

It is probable that not one but several factors are involved in both contrast and assimilation and whether one or the other occurs depends upon the net results of interactions among these factors. First let us consider the possibility that we are dealing with either mixture- or spread-effects, or both, of the Von Bezold type of assimilation. For mosaic mixture to occur it is usually assumed that the stimuli must be below the threshold of visual acuity, or the visual two-point threshold, under the prevailing conditions of viewing. In this study, the narrowest separation between the lines was 3′ 25″ and the widest separation was 1° 3′, whereas the separation necessary for the effect to be completely absent was found to be 1° 11′ (61.5 mm.) with black lines and 1° 13′ (63.8 mm.) with white lines. We cannot, therefore, say that this is a 'mixture'-effect in the sense of mosaic mixtures, since these angles are far above the resolving power of the eye. It may, however, be due to 'spread' of the local stimuli. While it is usually assumed that spread of light and of excitation on the retina is absent or minimal when two stimuli are perceived as two, because intervening receptor-elements must be 'unstimulated' for the stimuli to be discriminated, it is nevertheless possible for an area between two sources of light to be lightened by the sources, even though they are perceived as two. In this case, receptors between the primary areas of stimulation are excited but not enough to obliterate differences in the field. Similarly, stray light is present in some degree from all stimulation, as Bartley has shown and this must also be taken into account in addition to effects of local stimulation.[13] The fact that reversal of contrast varies inversely with the separation between the lines is evidence in support of spread as the chief factor in the results reported here. Furthermore, the effectiveness of the lines separated by visual angles as high as 1° 11′ means that effects of stimulation extend to greater distances on the retina than are generally assumed.

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[11] David Katz, *The World of Colour*, 1935 (trans. by R. B. MacLeod and C. W. Fox).

[12] W. Benary, Beobachtungen zu einem Experiment über Helligkeitskontrast, *Psychol. Forsch.*, 5, 1924, 131-142.

If local stimulation spreads more or less over the retina as a result of interaction, stray light, irradiation, or halation, then we have a basis for assuming that the level of stimulation for the white-lined gray is higher than that for the black-lined gray. We further assume that the adaptation-level of the eye as a whole is somewhere between the levels associated with the light flux from each of the gray regions, since adaptation-level is the pooled effect of all stimuli acting on the eye. On this basis, the white-lined gray is above adaptation-level and hence undergoes lightening, while the black-lined gray is below adaptation-level and hence undergoes darkening in accordance with the principle that stimuli on opposite sides of adaptation-level are displaced in opposite directions. This account is, in a sense, an explanation in terms of contrast, since it assumes that differences from level are enhanced. Instead, however, of assuming that only differences between *contiguous* local areas (gray between white lines and gray between black lines) are affected, we assume that differences between the two gray areas and the *general level* are enhanced. An essentially similar hypothesis, in terms of successive contrast or adaptation, was advanced by Burnham to account for the fact that non-rigid fixation favors the Von Bezold effect when he suggested that in looking from one part of the field to the other a negative after-image is projected from the first to the second.[14] For this to occur, the eye must be in a higher state of light-adaptation on viewing the white-lined gray than when viewing the black-lined gray, and this implies exactly what we have assumed so far as each part of the field taken by itself is concerned.

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[13] S. H. Bartley, *Vision: A Study of its Basis,* 1941, 57 ff.

We began this discussion-section by pointing out that contrast and assimilation are affected by many factors, no one of which determines by itself whether a part of the visual field will show contrast or assimilation. Sometimes the same condition will produce opposite effects in different contexts. Thus introduction of borders may facilitate contrast, as in the case of the gray annulus on half-black, half-white background, which is a fairly homogeneous gray until a line is drawn on the gray where the two backgrounds join, whereupon the ring is lightened on the black ground and darkened on the white ground; but in the case of a shadow, if a dark border is added to the shadow, it becomes darker instead of lighter, as pointed out by Hering and others.[15] Preliminary observations, in which the areas of the white, black, and gray portions of Fig. 1 were altered, show that this variable is of prime importance and there can be no doubt that the reflectances of the lines and background are also important.[16] We must conclude that the Von Bezold type of assimilation is the result of complex interactions among a number of stimulus-dimensions, only one of which has been investigated here. In a subsequent study the effect of varying the areas of the lines and background will be reported in an endeavor to envisage classical contrast- and assimilation-effects within a single frame of reference.

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[14] Burnham, *op. cit.*, 385.

[15] Still other figures showing effects of contours on assimilation and contrast will be found in Harry Helson, Some factors and implications of color constancy, *J. opt. Soc. Amer.*, 33, 1943, 555-567.

[16] Burnham, *op. cit.*, 380, reproduces designs which show contrast or reversal of contrast as the width of borders changes and also as a function of order of presentation.

STRATEGIES OF SEARCHING IN TWO SIMPLE SYSTEMS

By H. C. A. DALE, Cambridge, England

Bartlett has proposed that it is useful to regard thinking "as an extension of evidence . . . in such a manner as to fill gaps in the evidence."[1] If this is the case, then searching should be of especial interest to students of thinking, for searching is overt behavior serving to extend the evidence. The locus of the object that is sought is defined within broad limits at the beginning of the search and the successful seeker then gathers more evidence until he determines its exact position. The rules of the game may be simple or complex. The search in a cubicle fitted with a number of doors for the one which is unlocked is a case where the rules are simple; the search for a faulty component in a fire-control radar equipment is a case where the rules are more complex. In both cases, the moves the subject (*S*) makes can be recorded and hence the way in which he seeks evidence and the use he makes if it can be studied.

The present study is descriptive. Two experiments are reported. In both the *Ss* searched among a large set of clearly displayed items. In one experiment the items were quite separate and *independent*. In the other they formed a simple *ordered* series.

In no previous study of searching among independent items have the items been both large in number and clearly displayed.[2] There are two studies of searching in an ordered series: Dale used a complex series; Goldbeck, Bernstein, Hillix, and Marx trained their *Ss* to use a given strategy and then examined the effect of display-complexity.[3] A description of the way naïve *Ss* tackle the simple ordered series is therefore still lacking.

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* Received for publication November 17, 1958. These investigations were made at the Medical Research Council's Applied Psychology Research Unit. The writer is indebted to the Royal Navy and R.E.M.E. for supplying *Ss*.

[1] F. C. Bartlett, *Thinking: An Experimental and Social Study*, 1958, 20.

[2] G. V. Hamilton, A study of perseverance reactions in primates and ordents, *Behav. Monogr.*, 3, 1916, No. 2; D. R. Meyer and P. M. Settlage, Analysis of simple searching behavior in the frontal monkey, *J. comp. physiol. Psychol.*, 51, 1958, 408-410; M. H. Detambel, Probabilities of success and amounts of work in multichoice situation, *J. exp. Psychol.*, 51, 1956, 41-44; M. H. Detambel and L. M. Stolurow, Probability and work as determiners of multichoice behavior, *ibid.*, 53, 1957, 73-81; A. Rey, *L'intelligence pratique chez l'enfant*, 1935, 196 ff.; G. A. Miller and F. C. Frick, Statistical behavioristics and sequences of responses, *Psychol. Rev.*, 56, 1949, 311-324.

[3] H. C. A. Dale, Searching in a structured system: A methodological study, *Medical Research Council, A.P.U. Report No. 247*, 1955, 17; R. A. Goldbeck, B. B. Bernstein, W. A. Hillix, and M. H. Marx, Application of the half-split technique to a problem-solving task, *J. exp. Psychol.*, 53, 1957, 330-338.

EXPERIMENT I. SEARCHING AMONG INDEPENDENT ITEMS

The aim of this experiment was to examine the strategies employed when a person searches among a set of independent and unordered alternatives for the one that differs from the rest. The experiment was broken down into two conditions: in one *S* recorded his moves as he made them; in the other, he did not.

Apparatus. *S* saw two parts of the apparatus: the Display and the Tester.[4] These were presented as parts of a task in which *S* was to discover a fault in the Display. The Display consisted of 400 miniature jack sockets arranged in a 20 × 20 block. A cardboard mask covered all but a chosen number of the jacks. The Tester was a device for obtaining information from the Display. A cord ending in a plug led from it. To make a check *S* had to insert the plug into the socket he wished to test, adjust the Tester, and then pull a lever. A socket was either 'correct' or 'faulty'. If it was correct, a green lamp on the Tester would light. If it was faulty, a red lamp would light. This operation took about 15 sec. Unknown to *S* the result of each test was determined by the *E*, who decided whether the red or green light was to go on.

Procedure. *E* was in the same room as *S*, but was separated from him by a screen. Every *S* was tested individually.

The task was introduced as fault-finding. The masks used revealed a row of 19 sockets. *S* was told that the row of sockets represented a group of 19 *separate* items of equipment, one of which was known to be faulty. He was shown how to use the Tester to check each piece of equipment and was then asked to locate the faulty equipment with as few checks as possible.

Each *S* was given three problems. On the first problem he was allowed to make nine checks without finding the fault, and to be successful on the tenth. On the second and third problems, a 'faulty' socket was specified and *S* continued until he found it. To minimize serial effects a different display mask was used for each problem selected at random from a set of 10. When *S* had solved the first problem *E* changed the mask and said, "That took x moves. Now see how few moves you need to locate the faulty equipment in this set." The same procedure was repeated after the second problem.

Forty *S*s participated, all of them young enlisted men. Twenty were given pegs with which to record their tests, the other 20 had to remember the checks they made. The *S*s were not told how many problems to expect but they knew the session would last about half an hour.

Analysis. The records that were kept by *E* were scrutinized to see what strategy had been adopted on each problem. It was found possible to distinguish three approaches: (1) Stepwise exploration—every item had been taken in turn, beginning at one end of the row and working toward the other. (2) Patterning—some readily apparent pattern of exploration was followed, *e.g.* testing every other item from left to right, and then filling in the gaps. (3) Random—an order was used in which no patterning could be perceived.

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[4] The apparatus was especially developed for research of this kind. A description is given in supplementary material deposited as Document Number 5867 with the ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington 25, D. C. See also H. C. A. Dale and I. D. Brown, An apparatus for investigating certain aspects of fault finding, *Medical Research Council, A.P.U. Report No. 318*, 1958, 9.

When *S* was successful in one or two tests the strategy was called Stepwise if the tests could have been the beginning of a Stepwise approach. If they could not, it was categorized separately under the head "*n* one or two but not Stepwise."

Five judges (including *E*) assessed the results. Independent assessments were made initially; these were subsequently discussed so that genuine disagreement could be distinguished from a failure to take all relevant factors into account. The definition of a Stepwise pattern was clear and did not lead to disagreement, but the distinction between Pattern and Random orders was not always easy. The cases in which judges disagreed, or in which at least one of them could not be sure whether the label should be Pattern or Random, were recorded separately.

TABLE I

STRATEGIES EMPLOYED IN SEARCHING AMONG SEPARATE ITEMS

Strategy	Problem					
	1		2		3	
	pegs	no pegs	pegs	no pegs	pegs	no pegs
Stepwise	10	6	3	4	4	3*
Pattern	3	7	7	5	7	4
Random	6	2	5	2	4	3
P or R (judges disagree)	1	5	5	8	1	7
n less than 3; not stepwise	0	0	0	1	4	3

* The three members of this group used this strategy on all three problems. Two members of the other (pegs) group did so also.

Results. It can be seen from Table I that the presence or absence of the recording pegs had no clear effect upon choice of strategy. If anything, more of those *S*s given pegs were proceeding systematically (were using Stepwise or Patterning strategies) on the third problem.

The decrease from the first to the second and third problems, in the number of *S*s who used a Stepwise strategy is significant at the 1% level (McNemar's test was used).[5] Retrospective reports indicate that on their first problem some *S*s felt obliged to use this strategy; in other words they had read more in the instructions than was intended. This most probably is the reason for the decrease.

Discussion. The sole requirement of this task was that the faulty equipment should be found in as few moves as possible. Since there was no information to the contrary, the most reasonable assumption about relative probabilities would be that they were equal, therefore any one order of examination of the 19 possibilities was as good as any other. When the *S* had no means of recording his moves, a systematic procedure would ensure that no moves were forgotten and inadvertently repeated. Since a Stepwise exploration is less likely to lead to confusion than any other strategy, this would seem to be the best approach. The results of this experiment are striking insofar as they show how few *S*s use this strategy.

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[5] Quinn McNemar, *Psychological Statistics*, 1959, 77-82.

It can be said on intuitive grounds that the Stepwise strategy is boring. The common use of the Random and Pattern strategies might therefore be attributed to an attempt on the *S*s' part to avoid this boredom. Other evidence, however, indicates that this is not likely to be the case, since these results are only found when the *S*s are searching, not when they have to explore all possibilities.[6]

Two assumptions were made when it was argued that the Stepwise strategy was the best approach: that the items were equally likely to be faulty; and that moves might be forgotten if an unsystematic procedure were adopted. It might well be that the *S*s do not adopt these. The fact that the provision of means of recording moves did not affect the choice of strategy certainly suggests that the *S*s were not making conscious allowance for their limitations of memory.

The negative finding in respect to the memory-aid is in contrast to the findings of Bruner, Goodnow, and Austin, and of Whitfield.[7] The difference could be due to differences in *S*s. Bruner's were "advanced undergraduates or graduates" and Whitfield's were research workers. Few of the *S*s used in the experiment reported here had attended university. Alternatively, it could be that those *S*s who worked without a memory-aid and who did not explore systematically, felt quite capable of remembering up to 20 moves.

EXPERIMENT II: SEARCHING IN AN ORDERED SYSTEM

The purpose of this experiment was to examine the strategies employed when persons have to find the faulty member of an *ordered* set of items.

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[6] Ten *S*s were given the same display but instead of being told to search for one faulty item they were told to check all 19. Four *S*s had the memory-aid, six did not. All *S*s were given two problems. On the first problem seven *S*s used the Stepwise strategy, one the Pattern, and two the Random (one of these two had the memory-aid). On the second problem the results were 9, 1, and 0 respectively.

[7] J. S. Bruner, J. J. Goodnow, and G. A. Austin, *A Study of Thinking,* 1956, 125; J. W. Whitfield, An experiment in problem solving, *Quart. J. exp. Psychol.*, 3, 1951, 184-197.

It was administered in two ways; in one, the *S*s recorded their moves, in the other, they did not.

Procedure. The task was described as an analogue of electronic fault-finding. The instructions began with a description of the idea of a signal-flow. *S* was told that electronic equipment was built up of units called stages and that it worked by transforming a signal, stage by stage, until it was of the right strength and form to work some device such as a loudspeaker. The Tester was then described as a means of examining the state of a signal at points along the chain. It was pointed out that if a fault were at a given point, a signal passing along from the input would be correct at all stages preceding the faulty stage, and incorrect at all succeeding stages. The Tester was used in the same way as in the first experiment. The green and red lamps on the Tester indicated whether the signal was correct or faulty at the point being tested. *E* determined the result of each check; he always ensured that it was consistent with previous checks. Two procedures were used. In one, the jacks represented the stages. In this case both lamps lit when the faulty stage itself was tested. With the other procedure the jacks represented check points which lay between the hypothetical stages so that the faulty stage lay between the last correct check and the first faulty one (reading from input to output). *S*s were able to make a direct confirmation that they had located the faulty stage by using a second special test device. Half the *S*s were given the first procedure, half the second. Since the differences in procedure did not affect the choice of strategy, they will not be further mentioned.

Forty *S*s participated. Twenty had the memory-aid, 20 did not. Each *S* was given three problems. The goal was predetermined in every case. Three *S*s who showed by their procedure that they had failed to grasp the idea of a flow system were replaced.

Analysis. The following strategies were distinguished. (1) Dichotomy—the first check was in the middle of the row, the second in the middle of the relevant half and so on. (2) Bracketing—the search began in the middle and proceeded much as dichotomy, but a redundant check at one end of the row or at both ends was used early in the search. (3) Unit-steps—moves were at an end or were adjacent to a previous move. (This category includes some responses which in Experiment I would have been classified Pattern.) (4) Steps of two or three—each move was within three units of a previous move or an end. (Steps were not necessarily in a single direction.) (5) Other—all other responses. (This category includes a few clear cases of mixed strategies and all cases where the number of moves was not more than two.)

The responses were assessed in the same way as in Experiment I by the same judges. The decision was unanimous in all but a few instances. Where at least one judge, but not all, labelled the response Dichotomy, it has been placed in a special category: Dichotomy (doubtful).

Results. From the results shown in Table II it can be seen that the group given pegs as a memory-aid behaved in much the same way as the others.

Miller, Folley, and Smith have pointed out that in problems of this kind, Dichotomy (as they call it, the half-split technique) is the best strategy.[8] The advantage of Dichotomy shows most clearly when there are a large number of items and the number is a power of two. With the trouble occurring at random among 19 items, the expected number of moves per problem is 3.63 with Dichotomy. This compares with 4.37 for Bracketing, with 5.68 for Steps of Three and with 10.0 for Steps of One. There are some other strategies included here in the 'Other' category in which the difference is smaller; for instance, that of dividing the array into 3 (steps of 6?) and then dichotomizing, in which case the expected number is 3.80.[9] The results show that most Other strategies could not have been very efficient since for the most part an average of five or more moves were required. When allowance is made for the fact that some responses were placed under this head because there were too few moves to see what strategy had been adopted, the number is even higher.

TABLE II

STRATEGIES EMPLOYED IN SEARCHING AMONG ORDERED ITEMS

Strategy	Problem 1		Problem 2		Problem 3	
	pegs	no pegs	pegs	no pegs	pegs	no pegs
Dichotomy	3	5	4	6	6	5
Dichotomy (doubtful)	1	0	1	1	1	0
Bracketing	3	2	3	2	2	2
Unit Steps	2	4	2	0	1	1
Steps of two or three	1	3	1	2	1	1
Other*	10	6	9	9	9	11
Mean moves per problem when using 'Other' strategies†	7.7	5.3	6.1	4.3	5.0	5.5

* Some judges labelled one of these responses Bracketing on the third problem.

† Allowing for six cases in which strategies were classified 'other' because only two moves were required the overall mean number of moves per problem is 6.2.

Relatively few *S*s dichotomized; even on the third problem, the number was less than one third of the total. On the first problem, a greater number of *S*s used a strategy with an expected number of moves greater than 5 than used dichotomy. The numbers choosing these inefficient strategies had dropped sharply, however, by the third problem. (This change is significant at the 5% level using McNemar's test.[10] This evidence of learning is interesting, since the conditions were not such that improvement would be expected. In the first place, *S*s were not given a target for which to aim; they had, therefore, no real knowledge of results. In the second, with most strategies, there is an element of luck, so that even if a target were given, it would not be easy to assess the value of different strategies empirically. Therefore it would still be difficult for *S* to tell how close his strategy was to the optimal one.

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[8] R. B. Miller, J. D. Folley, and P. R. Smith, Systematic trouble shooting and the half-split technique, *HRRC Tech. Report* 53-21, 1953.

[9] These numbers assume the procedure in which jacks represent stages. When they represent test-points between stages, the numbers will be roughly 0.5 moves per problem larger.

[10] McNemar, *op. cit.*, 77-82.

The conclusion to be drawn is that the idea of dichotomizing is sophisticated and we must suppose that many persons do not know of it. The fact that this strategy was not being used does not imply, however, that the *S*s failed to understand the chainwise nature of the display. Those who had shown that they had failed to grasp this aspect of the task had been replaced. The *S*s were making use of the information gained from one check to guide them in selecting the next checkpoint, but only those who dichotomized were getting the most information from each check.

INTELLIGENCE AND CHOICE OF STRATEGY

The experiments showed considerable variability among the *S*s in their approach to the searching tasks. To discover whether this could be accounted for on the grounds of intelligence, test scores that were available for nearly all the *S*s were examined.

Procedure. Twenty of the *S*s were sailors. They had been given the AH4 test (a verbal test) at Cambridge.[11] The others were soldiers. They had been given a battery of tests on induction which included a verbal test of intelligence. Their scores on this test were used in the analysis. The *S*s were separated into groups according to the strategies they used. They were then ranked according to their intelligence test-scores, according to Whitfield's method of analysis.[12] The results below are all given in terms of normal deviates. In the first experiment both soldiers and sailors were used as *S*s. In this case the separate ranking scores were combined.

*Results: (1) Experiment I. S*s who used Stepwise or Pattern strategies on all three problems were compared with the rest. They were found to rank more highly on intelligence, $ND = 3.2$. Those who always used the Stepwise strategy were more intelligent than those who did not, $ND = 2.2$. The difference between those always using Stepwise and those using Stepwise or Pattern strategies was not great, the former ranked slightly higher than the latter but the numbers were too small to justify the conversion to normal deviates.

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[11] A. W. Heim, *Manual for the Group Test of General Intelligence* AH4, 1955.

[12] Whitfield, Rank correlation between two variables, one of which is ranked, the other dichotomous, *Biometrika*, 34, 1947, 292-296.

(2) Experiment II. *S*s who used a strategy of Dichotomy were found to rank higher than those who did not. The results for the three problems taken separately gave $ND = 2.87$, 2.36, and 2.01, respectively.

Of the 77 *S*s whose test results were known, only 4 were below the mean population as a whole. Sixty were in the top 30% of the general population.

In both tasks, it was found that those *S*s who used the more appropriate strategies were, on the whole, more intelligent than those who did not. By itself this result is no more than suggestive since the number of *S*s involved is small. There is other evidence, however, which points in the same direction. Rey found that older children would search systematically for a sweet hidden in an experimental room, whereas younger ones dashed from place to place in a random fashion.[13] He considered the age-difference to reflect a difference in intelligence. Binet used a searching task (the Ball-in-a-field sub-test) in his intelligence test.

SUMMARY

Two simple searching tasks were given to groups of young enlisted men. In one task, *S*s had to locate a faulty member of a row of independent items. In the other task, *S*s had to locate a fault in a simple flow system.

Relatively few *S*s, when searching among independent items, used what was considered to be the best approach, that of examining each in turn, working from one end of the row to the other, and few *S*s used the best strategy when searching in a simple flow system. The provision of a memory-aid did not affect choice of strategy in either task.

The intelligence of the *S*s, as measured by paper and pencil tests, was found to be correlated with their methods of searching. The more intelligent used the better strategies.

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[13] Rey, *op. cit.*, 196-199.

THE APPARENT SIZE OF AFTER-IMAGES

By T. G. CROOKES, St. John's Hospital, Aylesbury, England

The problem of the relationship between Emmert's law and the law of size-constancy was raised by Boring in 1940.[1] He claimed that each could be derived mathematically from the other, and that they were, in fact, aspects of the same law. He was taking Emmert's law to refer to the 'apparent' size of after-images, that is, their phenomenal size, as compared with standards at different distances, in the way that apparent size is defined in size-constancy experiments. On this reading of Emmert's law, the two laws can, on the face of it, be related to each other. The argument can be summarized as follows: an object of constant size tends to have the same apparent size when its distance from the eye is increased, although its retinal size decreases (size-constancy); therefore, if the retinal size remains the same, as in the after-image, apparent size must increase with distance (Emmert's law).

Emmert's law, however, at least as worked out by Emmert,[2] refers to the 'real' size of after-images, that is, size as measured on the surface of projection. Thus it is related, not to size-constancy, but to the optical law which derives the size of the retinal image from that of the object and its distance from the eye. An object of constant size produces a decreasing retinal image with increasing distance; therefore, when the retinal image is constant (as in the after-image) with increasing distance, the object must increase. Emmert himself looked at it in this way. He wrote: "das einmal auf der Netzhaut entwickelte Nachbild eines Objects, so lange er besteht, auf der Netzhaut dieselbe Grösse behält. Nach aussen projicirt wird dieses Nachbild, wenn man so sagen will, selbst Object."[3]

This was pointed out by Young,[4] and there followed a correspondence between Boring and Edwards, on the one hand, and Young on the other, at the end of which Edwards and Boring agreed that the law as worked out by Emmert referred to 'real' size, but suggested that Emmert was not clear about the distination between 'real' and 'apparent' size.[5] They insisted that there must be an 'Emmert's Law' of apparent

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* Received for publication February 5, 1959.

[1] E. G. Boring, Size constancy and Emmert's law, this JOURNAL, 53, 1940, 293-295.

[2] E. Emmert, Grössenverhältnisse der Nachbilder, *Klin. Monatsbl. d. Augenheilk.*, 19, 1881, 443-450.

[3] *Idem*, 448.

[4] F. A. Young, Boring's interpretation of Emmert's law, this JOURNAL, 63, 1950, 277-280.

[5] Ward Edwards, Emmert's law and Euclid's optics, this JOURNAL, 63, 1950, 607-612; Young, Concerning Emmert's law, this JOURNAL, 64, 1951, 124-128; Edwards and Boring, What is Emmert's law?, this JOURNAL, 64, 1951, 416-422.

sizes of after-images, because it could be deduced from the law of size-constancy. Now this deduction, as summarized above, is plausible, but it assumes something which is not self-evident. It assumes that the perception of objects and of after-images are events of the same type, and must therefore follow the same law as far as apparent size goes. It is equivalent to saying that they must show the same size-constancy. If there is a law of size-constancy which states that an object of constant size has the same apparent size when its distance increases and its retinal image decreases, then it is fair to deduce as a corollary that an *object* of increasing size and constant retinal size will have increasing apparent size as its distance increases; but you cannot apply this deduction directly to after-images. An after-image is not simply an object seen at a certain distance; it is recognized as something subjective, as being somehow a figment of one's sense-organs. It could be that the very objectivity of the object is a factor in its size-constancy which is lacking in the after-image.

There is one possible source of confusion when speaking of the size-constancy of after-images. If an after-image had the same apparent size at whatever distance it was projected, it would, in a sense, be constant, but in the technical sense it would be showing no size-constancy, because its size would depend entirely on the size of the retinal image, which is constant. It would show 100% constancy if it varied directly with distance since it would be dependent entirely on the 'real' size of the image, which varies in that way, according to the original Emmert's law. Strictly, to justify Boring's deduction, the after-images should show 100% constancy, since the law of size-constancy is stated in its 100% form; however, since he agrees that absolute constancy is not always found, it would be sufficient if the after-images showed the same constancy as objects under identical conditions. This is something which can only be discovered empirically.

There have been two empirical studies aimed at throwing light on this problem; Edwards hypothesized that, if 'Emmert's law' of apparent sizes were related to the law of size-constancy, it would work in ordinary conditions but not in conditions of reduction, *i.e.* with cues to distance absent.[6] He devised an experiment on the apparent size of after-images at different distances under reduced conditions, and found that the apparent size did not vary with distance, but remained the same. What he did not show was that the law *did* work in ordinary conditions. Whether it did or did not, the apparent size would be invariable in reduced conditions. If it happened that after-images showed no constancy in ordinary conditions, but had the same apparent size at all distances, then *a fortiori* they would show no constancy under reduction.

Hastorf and Kennedy considered the differences between Boring and Young to be due to their different methods of measurement, Boring and Edwards using comparison-objects at a different distance from the object being considered, Young getting his *Ss* to indicate the boundaries of the after-images with spotlights.[7] They set up an experiment in which *Ss* in one condition cast an after-image onto a distant screen and tried to adjust an object on a nearer screen to the same size; this was done with and without reduction. In the second condition the size of the after-image was indicated by spotlights, also with and without reduction. In all cases instructions were toward reproducing the actual size. In the first condition the estimated sizes were smaller with reduction; in the second, the obtained sizes were the same with and without reduction. The authors take this to mean that the argument about Emmert's law is simply a matter of method of measurement. When comparison-objects are used, Emmert's law fails under reduced conditions (Edwards), but with spotlights it still holds (Young).

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[6] Edwards, Apparent size of after-images under conditions of reduction, this JOURNAL, 66, 1953, 449-455.

[7] A. H. Hastorf and J. L. Kennedy, Emmert's law and size-constancy, this JOURNAL, 70, 1957, 114-116.

The use of spotlights in effect provides *O* with an extensible ruler and gives a direct measure of the 'real' size of the image, whether cues to distance are removed or not, and the results will in each case agree with Emmert's law, which is a law of 'real' sizes. The other method, designed for assessing apparent size, is a poor method of assessing 'real' size and is poorer still when distance cues are removed.

Certainly the method of measurement makes a difference, but the point is that the two methods are designed to measure two different sizes—the apparent and the real. The question is how these two sets of sizes are related to Emmert's law. On all sides it is agreed that the real size does vary in accordance with Emmert's law. What no one has shown is that the apparent size obeys Emmert's law, at least, as closely as the apparent size of objects obeys the law of size-constancy, as it should if Boring and Edwards are right. The purpose of the present experiment was to examine this question.

METHOD

The experiment was carried out in three stages for each observer (*O*).

Stage 1. Stage 1 was a quick verification of Emmert's original law (of real sizes) for the particular conditions of the experiment. The after-images were generated at 2½ ft. from the *O*'s eye with 3 black circles ½, ¾ and 1 in. in diameter, each on a rectangular white card 3½ × 2½ in., and having a small white fixation-point in the center. When *O* had been shown how to get the after-image with the 1-in. circle, and had described what he saw when it was projected onto a plain white surface, he was asked to get it again and project it onto a white sheet of paper at 5 ft. on which were 7 circles (outlined in India ink) of increasing size from left to right, and pick out which of the circles was the same size as the after-image, into which it fitted exactly. This was done with each of the three sizes of circle at two distances, 5 and 7½ ft.—six readings in all.

Stage 2. Stage 2 dealt with the apparent size of after-images. After-images were generated from the same circles at the same distance (2½ ft.) and projected onto a white sheet at 7½ ft., a cross marking the required position. The projected image was compared for apparent size with a row of seven circles at 3¾ ft., *i.e.* half-way between the eye and the image. It was emphasized that *O* should not estimate actual size but say which looked the same size from where he was sitting. A demonstration was given with two pencils of unequal lengths; it was shown how they could be made to look equal in length by putting the larger one further away, and *O* was told that this was the kind of judgment of equality required. Estimates were made twice for the after-image of each circle—using different sets of comparison-circles for each—six estimates in all.

Stage 3. Stage 3 dealt with the apparent size of objects. Three white circles on gray background were prepared, three times the linear size of the black circles, *i.e.* 3, 2¼, and 1½ in., respectively, the expected size of the after-images when projected to 7½ ft. They were painted in gouache color and made to look as near as possible to my own experience of the after-image in these conditions. These were placed at 7½ ft. and compared in the same way as the after-images with circles at half the distance. Again, there were six judgments, the circles being presented in the same order as the corresponding after-images, and the same comparison-circles being used. In this stage, it had to be emphasized that *O* must continue to fixate the object-circle when making his judgment, and not look from it to the comparison-circles. This was to make the judgment comparable to that with the after-images, where clearly changes of fixation could not be allowed, since the after-image would move. This meant that there was a doubling of the comparison circles, but the *O*s said it did not interfere with their judgments. The limitation also had an advantage, in that it controlled the question of change of accommodation which has been shown to influence apparent size.[8]

Stage 1 always came first—the order of the second and third stages was varied with alternate *O*s on the chance that there might be a sequence-effect. The *O*s were told they could give judgments in between two of the comparison circles.

The *O*s were kept as homogeneous as possible with respect to the factors known to affect judgments of apparent size. They consisted of 10 women, aged 24–33 yr., all of good intelligence, occupying professional and secretarial posts in a mental hospital. None had any experience of this type of experiment.

RESULTS

Each of the six judgments given by each *O* was multiplied by that fraction which would make it 3 in. if Emmert's law were being followed exactly. The mean of the six figures so obtained was *O*'s 'Emmert's law-score,' and the nearer to 3 it came, the closer was the agreement with Emmert's law. The mean of the 10 *O*'s was 2.996, which seems a reasonable approximation to 3. With one exception, the deviations from 3 all were less than 8%. The exception was one *O* with a score of 2.53, but the deviation was due mostly to one exceptionally small judgment, the first one, and it may have been due to some misunderstanding. What deviations there were were not usually consistent for a given *O*, but there were two *O*s whose three judgments at the greater distance all exceeded the expected one by the same proportion.

The mean Brunswik-ratio for each *O* for each of the two conditions was calculated. For the objects, in Stage 3, the mean ratio of the 10 *O*s was 65.1, with a range of 36–108; for the after-images, in Stage 2, taking as the 'real' size that expected by Emmert's law, the mean was 37.2, range 4–75. Subtracting each *O*'s after-image ratio from her object-ratio gave values from −11 to +51, with a mean of +27.9. There was only one negative value, the next lowest being +13. The standard error of this mean is 5.75. Testing the significance of its difference from 0 gives $t = 4.85$, and the probability of its being drawn from a population whose mean is 0 is less than $\frac{1}{10}$%. As there were some *O*s showing a consistent, if small, deviation from Emmert's law, the after-image constancy also was calculated, taking as the 'real' size the size actually given in Stage 1. When these ratios are subtracted from the object-ratios, it gives a set of figures ranging from +2 to +64, with no negative values. The mean is 30.5, standard error 6.21, $t = 4.91$, again less than $\frac{1}{10}$%. The order of Stages 2 and 3 made no difference.

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[8] B. B. Smith, A contribution to the problems of apparent size and shape, *Bull. Brit. Psychol. Soc.*, 26, 1955, 14-15 (summary, manuscript circulated privately); W. S. Duke-Elder, *Textbook of Ophthalmology*, 1932, 745.

DISCUSSION

It appears then that after-images show less size-constancy than objects of the same size and color when viewed under similar conditions. Given an object and an after-image of the same real size, the same retinal size, seen at the same distance under the same conditions, the after-image appears considerably smaller. Hence it is not only theoretically objectionable to derive an 'Emmert's law' of apparent size from the law of size-constancy, but it is not emp[illegible]cally justified.

At the root of this problem is the question of whether it is advisable to speak of a 'law' of size-constancy at all. Emmert's law is a law in the sense that when certain ascertainable quantities are known, others are predictable, and can be verified. In general it works very well. A few people apparently show considerable deviation from it,[9] but these cases are probably confusions of two kinds of image, the true perceptual after-image, and the 'mental' image (of the kind when you 'think about' a red patch). Young's *O*s gave average values very close to Emmert's law over distances up to 24 m., although he himself emphasizes the individual differences.[10] A few amendments should be made to it because of the physics of the optical system. In accommodation, for instance, the nodal point moves away from the retina,[11] and, therefore, for a given visual angle, the retinal image increases with increasing accommodation. Conversely, when there is a constant retinal image, with decreasing accommodation, the nodal point moves nearer to the retina, and the angle subtended by the retinal image is increased. Hence when the after-image is generated from a near object, then projected to a greater distance, the angle will be larger, and the projection larger than would be expected from Emmert's law. This may account for some individual differences, because, when the projection is made, the accommodation may or may not be altered completely to suit the new distance. This and similar considerations might involve slight modifications to Emmert's law, but the effects in practice are small.

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[9] Ian Oswald, After-images from retina and brain, *Quart. J. exp. Psychol.*, 9, 1957, 88-100.

[10] Young, The projection of after-images and Emmert's law, *J. gen. Psychol.*, 39, 1948, 161-6.

[11] Duke-Elder, *op. cit.*, 745.

With this, and perhaps other similar amendments, Emmert's law really is a law, but the law of size-constancy is not of the same kind. One cannot, given sets of measurements, make a numerical prediction of the apparent size of an object. One might say that in ordinary conditions the apparent size lies between the real size and the stimulus-size, but even this is not always true. There is a vast range of individual differences. In a recent study, made under the same conditions, the *O*s gave Brunswik ratios ranging from 0 to 108.[12] Mean values could be worked out for certain limited sets of conditions, but certainly in the 100% form, as stated by Boring and Edwards, the law is hardly ever true, and it certainly leads to absurdities as pointed out by Young.[13]

There are two sets of facts, and this controversy is due to the confusion of the two. One set is concerned with various lengths which are measurable, either by direct application of a ruler or with the aid of geometry and a knowledge of optical systems—*i.e.* the size of an object, its distance from the nodal point, the distance of the nodal point from the retina, the retinal image. Emmert's law belongs here; it is the relationship between the retinal image, the projected size of the image, and the distance of the projection from the nodal point. The other set is concerned with apparent size which is not measurable. It is, of course, meaningless to say an object has an apparent size of x inches, or that its apparent size subtends an angle of x degrees. (You can, of course, say it *appears* to be x inches or to subtend x degrees, but this is a different matter, the estimated size.) It is something quite subjective and can only be given a meaning by relating it to other apparent sizes—the apparent size of *A* is the same as (or greater than or smaller than) that of *B*.

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[12] T. G. Crookes, Size constancy and literalness in the Rorschach test, *Brit. J. Med. Psychol.*, 30, 1957, 99-106.

[13] Young, Concerning Emmert's law, this JOURNAL, 64, 1951, 124-128.

It is with these relationships between apparent sizes that size-constancy is concerned—the fact that two objects may have the same apparent size when, for instance, their real sizes are unequal, and so are the visual angles they subtend. So many factors are known to be concerned—the distance between the objects involved, the cues to distance, *O*'s interpretation of the very notion of apparent size, that it would seem unlikely that any law of apparent size could be profitably linked to the relatively clear-cut optical laws mentioned above. With very much more knowledge, the facts about apparent size presumably could be formalized into a body of laws, but with many more variables than the other system of facts to which Emmert's law belongs.

SUMMARY

Emmert's law is concerned with 'real,' measurable sizes and is related to the laws governing the size of the retinal image. If a new 'Emmert's law' is to be derived from the law of size-constancy, it can only mean that after-images show the same constancy as objects in the same conditions. It was shown empirically that after-images show less size-constancy than objects under the same conditions. Put the other way round, this means that the recognition of the percept as a 'real' thing contributes to its size-constancy. It is suggested that, since size-constancy is so variable, depending on so many factors, and since the very notion of apparent size (as distinct from estimated size) is a difficult one—to many *S*s scarcely comprehensible—it is not helpful to speak of a 'law' of size-constancy, certainly not in the same sense as Emmert's law.

A COMPARISON OF TWO METHODS OF LEARNING SERIAL ASSOCIATIONS

By ROBERT K. YOUNG, University of Texas

Recently two studies of learning by paired associates have presented strong evidence for 'backward' learning.[1] For example, if an A–B association is established, then A will have a high probability of being elicited upon presentation of B. It is apparent that when an A–B association is formed, the association is not only in the 'forward' direction but also in the 'backward' direction. This is true despite the fact that A has never served as a response (R) and B has never served as a stimulus (S) prior to the time of testing for backward learning.

Murdock has shown that the idea of backward learning can be used in a transfer-paradigm.[2] He found that the learning of a B–C association following the learning of an A–B association yields negative transfer, suggesting interference from the B–A association established during the A–B learning.

The procedure employed in this paradigm is essentially the same as the procedure used in learning by serial order *i.e.* a series of associations is formed: A–B–C–D, etc.; where all but the first and last items of the list are used both as S and R. It might be assumed that backward learning also occurs in serial learning and that serial learning is also a paradigm of negative transfer. With lists of equal length, learning by paired associates is more difficult than by serial order and the data on perservative errors in learning by serial order give little indication that backward learning occurs to the extent which might be inferred from the studies of backward learning in paired associates.[3]

As was noted above, every item in a serial list (except the first and last items) serves both as a stimulus and a response. Thus a serial list, *e.g.* A–B–C–D–E, etc., may be thought of as being composed of pairs of items, A–B, B–C, C–D, etc. If these items were presented randomly *e.g.* B–C, E–F, A–B, etc., learning by paired associates would be approximated, *i.e.* a series of associations would be learned in the manner that a typical list of paired associates is learned. With this type of learning an approach may be made to the analysis of the function of the stimulus in learning by serial order similar to the analyses made in learning by paired associates.

Primoff reports a study in which a serial list was learned in the method of paired associates mentioned above and this learning was compared to learning by typical method of serial order.[4] He found that the list of paired associates was much more

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[1] S. M. Feldman and B. J. Underwood, Stimulus recall following paired-associate learning, *J. exp. Psychol.*, 53, 1957, 11-15; B. B. Murdock, Jr., "Backward" learning in paired associates, *ibid.*, 51, 1956, 213-215.

[2] Murdock, "Backward" associations in transfer and learning, *ibid.*, 55, 1958, 111-114.

[3] B. R. Bugelski, A remote association explanation of the relative difficulty of learning nonsense syllables in a serial list, *ibid.*, 40, 1950, 336-348.

[4] Ernest Primoff, Backward and forward association as an organized act in serial and in paired associate learning, *J. Psychol.*, 5, 1938, 375-396.

difficult to learn than by serial order; and, in addition, that the associations formed during the learning of paired associates showed high positive transfer to the serial list from which the paired associates were derived. This was in comparison to an appropriately controlled serial list.

Apparently backward associations do not retard serial learning while the same backward associations inhibit learning by paired associates. A study of the modified form of learning by paired associates originated by Primoff may help in resolving this apparent paradox. The purpose of the present study is to replicate Primoff's study and to extend his investigations.

METHOD

Procedure. Forty-eight students from introductory psychology classes served as *Ss* in this study. Each *S* served for two days, the first being for practice. On the second day, each *S* was assigned at random to one of two groups—S-P or P-S. The *Ss* of Group S-P first learned a Serial List and then a list of paired associates (PA-List) which was constructed from the Serial List previously learned. Those of Group P-S first learned a list of paired associates (PA) and then a Serial List from which the PA-List had been constructed.

TABLE I

LISTS OF WORDS USED IN THIS STUDY

Serial list	PA-list		
CUTTING	POLITE	—	AWKWARD
EAGER	CERTAIN	—	OBLONG
POLITE	PUBLIC	—	MIDWAY
AWKWARD	EAGER	—	POLITE
MORBID	MORBID	—	CERTAIN
CERTAIN	INTENSE	—	PUBLIC
OBLONG	LEGAL	—	WEARY
INTENSE	CUTTING	—	EAGER
PUBLIC	AWKWARD	—	MORBID
MIDWAY	OBLONG	—	INTENSE
LEGAL	MIDWAY	—	LEGAL
WEARY			

An 11-pair PA-List was developed from a 12-item Serial List composed of adjectives of low similarity taken from Melton.[5] With the exception of the first and last, each item served both as a stimulus and as a response, *e.g.* TIMID–AWARE, AWARE-PIOUS, etc. These pairs were in turn assembled in a list of paired associates with four different orders of presentation and with the restriction that pairs with items in common would be separated by at least two pairs. Order 1 of the serial and PA-Lists are presented in Table I.

All lists were learned to a criterion of one perfect recitation and the only difference between the two groups was the order in which they learned the Serial and PA-lists.

Apparatus. Three different Serial Lists were constructed from the same 12 adjectives and three PA-Lists were in turn assembled from these Serial Lists. Each of the three PA-Lists had four orders of presentation to reduce the possibility of serial learning. The first item of the Serial List served as the anticipative symbol.

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[5] A. W. Melton, Materials for use in experimental studies of learning and retention of verbal habits, Mimeo manuscript, University of Missouri, 1940.

The lists were presented by an Animatic continuous film-strip projector with a 2-sec. rate for the Serial Lists and a 2 : 2-sec. rate for the PA-Lists. The intertrial interval was 6 sec. for both Serial and PA-Lists and approximately 1 min. elapsed between the learning of the two lists. No observational trial was given in the learning of the second list (whether it was PA or Serial). The *Ss* were told that the items and S–R associations were the same for both lists and that they should be able to get some correct even on the very first trial (this in contrast to the procedure employed by Primoff).[6]

The practice day consisted of the learning of a Serial List followed by the learning of a PA-List. In this case, however, the two lists had no items in common. The practice lists were composed of adjectives and were as low in similarity to the experimental lists as careful inspection would allow.

RESULTS

(1) Practice. Performance of the two groups on both the Serial and PA-Lists during the practice session was compared. A *t* of 0.13 was obtained between the two groups on serial learning and one of 0.11 for PA-learning. Neither approaches significance.

(2) Learning. The mean number of trials required by the *Ss* of both groups to learn each list and the mean number of errors per trial that they made in each list are presented in Table II. With reference to trials to learn, performance varies significantly as a function of the type of list learned. PA-learning is more difficult than serial learning. A comparison of the learning of the Serial List by Group S-P and the learning of the PA-List by Group P-S yields a *t* of 7.47, which is significant beyond the 1/10% level. Thus PA-learning is more difficult than serial learning despite the fact that the same items and the same S–R associations were learned in both the Serial and the PA-Lists. These results are in agreement with those obtained by Primoff.[7]

TABLE II

MEAN TRIALS TO LEARN, MEAN ERRORS PER TRIAL AND MEAN 'BACKWARD' ERRORS PER TRIAL

Mean	Group S-P				Group P-S			
	PA-list		Serial-list		PA-list		Serial-list	
	Mean	S_m	Mean	S_m	Mean	S_m	Mean	S_m
Trials	36.71	2.68	17.79	1.29	40.21	2.68	8.08	.87
Errors per trial	1.76	.16	1.20	.12	2.25	.22	.44	.08
'Backward' errors per trial	.89	.09	.03	.03	1.20	.12	.02	.07

Although the PA-List contained only 12 adjectives it was extremely difficult to learn, and 16 *Ss* were dropped from the experiment for failure to do so. Of these, 9 *Ss* were in Group S–P and 7 in Group P–S. Since the number dropped from each group was approximately the same, it is assumed that the procedure was not biased in favor of either group.

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[6] Primoff, *op. cit.*, 375-396.
[7] Primoff, *op. cit.*, 375-396.

(3) Transfer. For purposes of discussion, transfer will be defined in terms of comparisons between the rates of learning of lists of the same type. In PA-learning, Group S–P showed, in comparison with Group P–S, an 8% positive transfer. In terms of *t*, this difference is not significant ($t = 0.92$, $P > 0.05$). On the other hand, in Serial-List learning Group P–S showed, in comparison with the Group S–P, a 55% positive transfer which is highly significant ($t = 6.26$, $P < 0.001$). It is apparent that prior PA-learning facilitates serial learning but that prior serial learning facilitates PA-learning little, if at all.

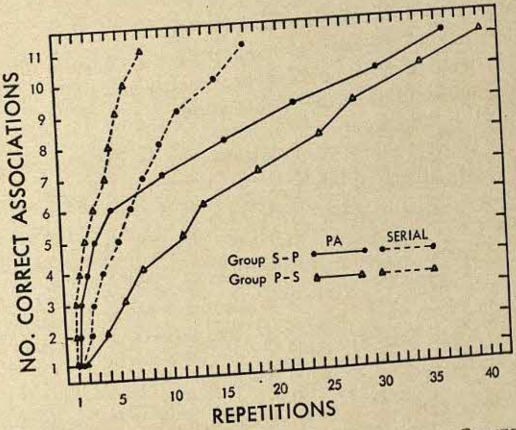

FIG. 1. TRIALS REQUIRED TO REACH SUCCESSIVE CRITERIA

Fig. 1 shows the number of trials required to reach our criterion of learning for all four lists. Until a criterion of six correct associates had been reached, the second list learned by both groups showed high positive transfer and both of these lists were roughly equivalent. It is interesting to note that with a criterion of six correct the PA-List was being learned significantly faster by Group S–P than by Group P–S ($t = 5.81$, $P < 0.001$). Thereafter the Serial-List continues to show high positive transfer while the transfer of the PA-List, which is 65% at a criterion of six correct, is reduced until the list is learned only slightly faster than the PA-List by Group P–S. Apparently some inhibitory factor reduces the rate of learning of the PA-List after it has reached a certain criterion of learning.

(4) Errors. The mean errors per trial for each list are presented in Table II. Two types of errors are reported in the table: total errors per trial and errors which indicate backward learning, *i.e.* errors which, in serial learning, would be perserverative errors of one degree remoteness. While errors of this second type rarely occur during serial learning, approximately one-half the errors made during PA-learning are of this type.

The differences in mean overt errors per trial between serial and PA-learning were found by comparing the first list learned in each group and the reliability of this difference was found to be highly significant ($t = 4.20$, with 46 *df*. $P < 0.001$). It may be concluded that PA-learning produces significantly more errors per trial than does serial learning.

Because of the occurrence of many zeros in the distribution of backward errors during serial learning, the Mann-Whitney 'U'-test was used to compare the differences in backward-learning errors per trial between serial and PA-learning.[8] PA-learning was found to produce more backward learning errors ($P < 0.001$).

DISCUSSION

Analyses of the data have shown: (a) that there are great differences in the ease of learning serial and PA-lists; (b) that prior learning of a serial-list facilitates PA-learning only until a criterion of approximately one-half the items has been reached, while prior learning of a PA-list yields high positive transfer to serial learning; and, (c) that backward errors of one degree remoteness occur rarely in serial learning but very often in PA-learning.

Clearly PA-learning is very difficult. This is true despite the fact that a PA-trial took 44 sec. while a serial trial took only 24 sec. (This might be a rare exception to the rule that distributed practice facilitates learning.) One hypothesis which might account for the difficulty in PA-learning is found in the inability of the *S* to discriminate between the two responses which are associated with each stimulus-word. For example, if Item C were presented as the stimulus (after a certain criterion of learning had been attained), both Items B and D would be associated to it. In this case the C–B association (backward learning) would be incorrect, but when Item B was presented as the stimulus the B–C association would be correct.

It would appear that the stronger of the two associations to a stimulus was elicited. The error-data lend some support to this since more than half the errors made during PA-learning were errors which would correspond to perseverative errors of one degree remoteness. Very few of these errors were made during serial learning. If the difficulty of PA-learning is due to the competition of backward and forward responses, then the difficulty of learning and the number of perseverative errors are related. If this hypothesis is true, some mechanism must be operating during serial learning which enables *S* to discriminate between the forward and backward associations since few backward errors are given and the serial lists are relatively easy to learn.

Comparison of serial learning in the two groups indicates that high positive transfer occurred from PA-learning to serial learning. The transfer of the present experiment is probably overestimated because of the general practice and warm-up effects (due to prior PA-learning) which occurred with the Serial Lists for Group P–S but not for Group S–P. These transfer results are, however, in substantial agreement with Primoff.[9]

Comparison of PA-learning in the two groups indicates that there was no diftransfer occurred in spite of the fact that in PA-learning Group S-P should have been facilitated by the general effects of practice and warm-up as well as by the specific effects due to prior learning of the PA-associations during serial learning.

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[8] Sidney Siegel, *Nonparametric Statistics*, 1956, 116-126.

[9] Primoff, *op. cit.*, 375-396.

From the difficulty of PA-learning and the absence of positive transfer from serial to PA-learning, it would appear that tremendous interference occurs during PA-learning. The large number of errors of one degree remoteness would seem to indicate that the locus of interference lies in the competition of two responses each time a stimulus is presented. The high positive transfer in PA-learning that occurred early during the learning by Group S-P lends support to this idea; *S* reports the more dominant of the two responses (whether correct or incorrect) and reaches a high criterion relatively early in learning, but, during later learning, *S* must discriminate between associations in the forward and backward directions *e.g.* D–E is correct but E–D is incorrect. Unfortunately the data do not give clear-cut support to this hypothesis.

It is surprising that little, if any, transfer occurs from serial to PA-learning in view of the facts that the items and the S–R associations between items are the same for both tasks and that high positive transfer occurs from PA-learning to serial learning. Perhaps greater degrees of learning of the serial list or a less difficult PA-task would enhance transfer from serial to PA-learning. In any event the influence of serial associations formed during learning by paired associates warrants further investigations.

SUMMARY

The associations formed during learning by paired associates and serial order were compared. It was found that learning by paired associates (PA) was more difficult than by serial order. Prior learning of a list of paired associates facilitated learning of a serial-list, but prior learning of a serial list facilitated learning by paired associates only until a criterion of 6 correct out of 11 associates was reached. Thereafter, facilitation was reduced until the list of paired associates, which followed a serial list, was learned no faster than a list of paired associates that was learned with no prior serial learning. It should be noted, however, that the method of paired associates we employed differed from the typical method in that 10 of the 12 items in the list served *both* as stimuli and responses.

THE ROLE OF LEARNING, PERCEPTION, AND REWARD IN MONKEYS' CHOICE OF FOOD

By WILLIAM A. WILSON, JR., University of Colorado

Harlow and Meyer have reported that monkeys given a choice between two different amounts of food usually choose the larger amount, although 'errors' (*i.e.* choices of the smaller amount) sometimes occur.[1] The authors recognize that the ability of the animals to discriminate differences in amount of food may enter into such choices—at one point in their paper they account for an aberrant finding in terms of "the ease with which a half-peanut may be confused with a whole peanut"—but they give the impression that their results are to be understood primarily in terms of the reinforcing properties of reward. Whether there was any evidence of learning during the early stages of testing, they do not say. It may be significant, however, that the animals used had earlier been trained in paired comparisons.

In the experiment reported here an attempt was made to analyze some of the factors entering into choices of the kind studied by Harlow and Meyer. Different amounts of food were presented to monkeys, and choices were recorded until a stable level of performance was achieved. Then the situation was so changed that the amounts of food presented continued to function as cues but not as rewards for choice; that is, discriminative and reinforcing factors were unconfounded.

Subjects. The *Ss* were six immature rhesus monkeys. All had extensive previous training in a probability-learning situation for peanut-reward.[2] They also had some previous experience with an automatic apparatus which yielded a single pellet of glucose for correct response.

Apparatus. Within a Wisconsin General Test Apparatus a special testing board was presented to *S*. This board was painted flat black except for two gray squares, 2.5 in. on a side and 5 in. apart. A piece of ⅛-in. plywood, also black, raised the surface of the 2.5 × 5-in. space between the squares. For Series I, each of the squares was covered by a transparent 2.5-in. square lucite box, 1 in. deep, placed with its open top down and hinged at the side away from the monkey. The food-reward, placed on the gray square, could easily be seen through the bottom of the box and obtained by tilting the box. For Series II, a special set of five boxes was prepared. Transparent tops were placed on the boxes and on the inner sides of the tops appropriate numbers of glucose-pellets were glued. The last 0.5 in. of the top farthest away from the monkey was cut out, and a strip of aluminum was

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* Received for publication November 12, 1958. This research was done at the Institute of Living, Hartford, Connecticut. It was supported in part by the Office of the Surgeon General (Department of the Army), Contract DA-49-007-MD-763.

[1] H. F. Harlow and D. F. Meyer, Paired-comparisons scales for monkey rewards, *J. compar. & physiol. Psychol.*, 45, 1952, 73-79.

[2] W. A. Wilson, Jr., and A. R. Rollin, Two-choice behavior of rhesus monkeys in a noncontingent situation, *J. exp. Psychol.*, in press.

so fixed to it that *S* could not see into this part of the box. These boxes were placed topside-down upon the squares, as described below. A microswitch was attached to the testing board next to each gray square, and one or the other was tripped when *S* responded by lifting a box during either series.

Procedure. Since the *Ss* had previous experience with similar boxes in a similar apparatus, no special preliminary training was given. There were two experimental procedures (Series I and II).

Series I. Glucose-pellets, 0.05 gm. in weight and 4 mm. in diameter, were used, in the five amounts of one, two, three, four, and five pellets. On each trial in Series I, one of these amounts of reward was placed under one box and another amount was placed under the second box. (In both series, the pellets visible in each box were in a straight line, parallel to the front of the box, and approximately one pellet-diameter from each other.) Then the one-way screen between the rewards and *S* was opened and *S* was allowed to lift one box and obtain the corresponding reward. Opening the one-way screen started a clock which was stopped by opening either of the boxes. The latency of response, measured in units of 0.25 sec., was recorded throughout the experiment.

There were 40 scored trials given each day for 10 days. Within each block of 10 trials, each possible comparison appeared once, in random order. On any day, two of the four different presentations of a given comparison were made with the larger amount on the right side, and two with the larger amount on the left. Two warm-up trials were presented before the 40 trials of a daily session; for these, the same rewards as were to be used on the last two trials of the day were employed.

Series II. In Series II, the boxes with pellets glued inside were used. These boxes looked very much like those of Series I, but, when *S* lifted a box, the apparent reward came away also and was unobtainable. Instead either four pellets or no pellets (placed beforehand by *E* on the portion of the gray square screened from *S* by the aluminum strips) became available. The random schedule used in Series I was used to determine which boxes (in terms of numbers of pellets) would be used, and on which sides of the testing board they would be placed; four pellets were always put under the box with the larger number of stimulus-pellets, and no pellets under the other box. Again, 40 trials (plus the warm-up trials) were given each day for 10 days.

Results. On early trials of Series I, the choices made were random with respect to the number of pellets in the boxes, but choices of the larger number of pellets increased to an asymptote in approximately 300 trials. Results for individual comparisons (*e.g.* five vs. three pellets) yielded negatively accelerated learning curves similar in form to that of the over-all results shown in the left part of Fig. 1. On the first trials of Series II, the *Ss* appeared to be surprised that they could not get the pellets which now served only as cues, but immediately found and consumed the reward-pellets when they chose the correct box. The second half of Fig. 1 shows the mean and range of accuracy of the tests in Series II. There is no evidence that the procedure in Series II led to a higher percentage of choices of the box with the larger number of pellets.

The gradual increase in mean percentage of choices of the larger reward during the course of Series I presumably reflects a gradual increase in *S*'s knowledge of the situation and of the results of comparison and choice. The mean curve masks the fact that some *Ss* exhibited more rapid increases from a near-chance to a near-perfect mode of behavior, but even in the data for these animals the transition is not abrupt. The 'errors' that remain after the animals have reached asymptotic performance in Series I are due, presumably, to a certain lack of distinctiveness between the groups of pellets being compared. The evidence from Series II leads to the conclusion that the asymptotic number and pattern of errors in both series are due primarily to a lack of *perceptual* distinctiveness rather than a lack of distinctiveness in reinforcement-value. In Series II, the difference in reward between a correct and an incorrect chaice always is equivalent to the maximal difference in reward between the two choices in the comparisons of Series I, yet the asymptotic performance is no higher.[3] These results cast doubt upon the

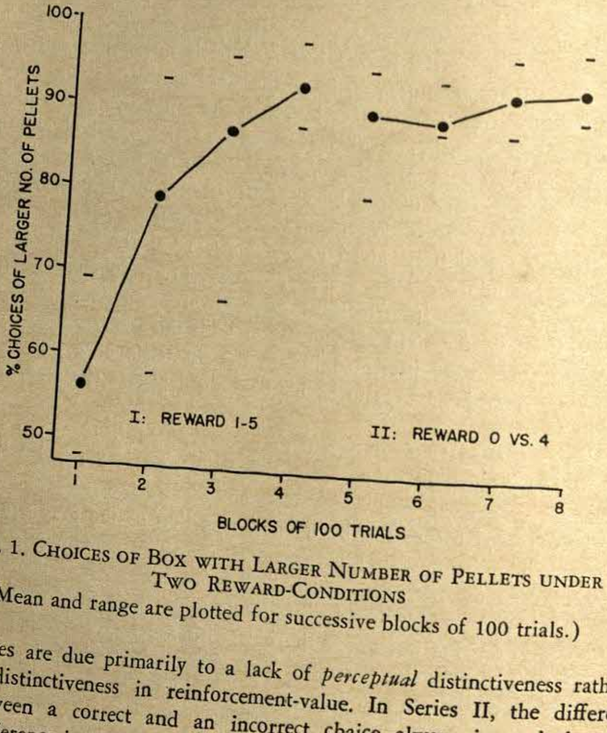

FIG. 1. CHOICES OF BOX WITH LARGER NUMBER OF PELLETS UNDER TWO REWARD-CONDITIONS

(Mean and range are plotted for successive blocks of 100 trials.)

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[3] The monkeys later were retested after half of them had been subjected to bilateral lesions of the inferotemporal cortex. The *Ss* received 200 trials under the procedure of Series I, followed by 200 trials under the procedure of Series II and then a final set of 100 trials under Series I. The operated animals displayed a temporary reduction in percentage of choice of the larger number of pellets in both situations; on the second and third blocks of 100 postoperative trials the inferotemporal animals were uniformly inferior to the normals. Ablation of the inferotemporal area has been shown to affect the performance of monkeys in problems of visual discrimination, but does not appear to affect (as do lesions of certain rhinencephalic areas) reactions to alterations in deprivation or magnitude of reward (W. A. Wilson, Jr., and Mortimer Mishkin, Comparison of the effects of inferotemporal and lateral

immediate applicability to animals of simple choice-methods of measuring utility.[4]

Scale-values were computed for the different numbers of pellets based upon the choices of all *Ss*, for each block of 100 trials.[5] With arbitrary values of 0 and 10 assigned to the extreme amounts (zero and five pellets, respectively), the values shown in Table I were obtained for two, three, and four pellets. On the first block of trials of Series I, the scale-values for three pellets and for four pellets are about as high or higher than the value for five pellets. After the third block, there is but little evidence for further change in scale-value. The values obtained during the final block of Series I and the final block of Series II were compared by computing scale-values for individuals, and comparing the means then obtained. The differences between the values obtained in the two series were not significant for any amount of reward; nor did the difference between the scale-values for two and four pellets vary significantly from the last block of Series I to the last block of Series II. The results for each *S* on the last block of each of the two series were pooled to give asymptotic scale-values. The relation of these values to number of pellets is neither arithmetic nor logarithmic. Plotted, they seem to lie almost midway between $y = a \log x$ and $y = a\ (x\text{-}1)$, and the experimentally determined points differ significantly from both curves. The relation found by Harlow and Meyer, who used amounts of ¼, ½, 1, 2, and 4 peanuts, was logarithmic. The discrepancy may be due to differences in kind of food or to differences in the pattern of amounts.[6]

TABLE I

SCALE-VALUES OF DIFFERENT NUMBERS OF PELLETS

(Value of 0.0 assigned to one pellet, 10.0 to five pellets.)

No. of pellets	Blocks of 100 trials (Series I) 1	2	3	4	(Series II) 5	6	7	8
2	3.1	3.3	3.4	2.8	3.1	3.3	3.8	3.2
3	9.7	6.5	6.6	5.6	6.1	5.5	5.7	5.8
4	12.2	8.3	9.1	8.3	8.8	8.1	8.0	7.9

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occipital lesions on visually-guided behavior in monkeys, *J. compar. & physiol. Psychol.*, 52, 1959, 10-17; L. Weiskrantz, Behavioral changes associated with ablation of the amygdaloid complex, unpublished doctoral dissertation, Harvard University, 1953; J. S. Schwartzbaum, Food-maintained behavior in monkeys following bilateral ablation of the amygdaloid complex, unpublished doctoral dissertation, Stanford University, 1958). Thus the deficit displayed by the operated *Ss* in this study is further evidence that the behavior of normal monkeys in such a situation is limited by perceptual or learning factors and not by reinforcement-value distinctiveness. (Beverly D. Overstreet assisted with this postoperative testing, which was done in the Psychological Laboratories of the University of California, Berkeley.)

[4] R. D. Luce and Howard Raiffa, *Games and Decisions*, 1957, 12-38.

[5] J. P. Guilford, *Psychometric Methods*, 1936, 236.

[6] This experiment does not answer the question of exactly what aspect of the stimuli was being discriminated; it may, for example, have been either numerosity, length, or volume. The situation did allow for a simpler discrimination than did that of Harlow and Meyer, who used a mixture of different portions of peanuts and different numbers of peanuts in their series.

The median latency was computed for each of the 10 comparisons, separately for each *S* in each block of 100 trials. The mean of the median latencies for each comparison in the last block of trials of Series I is plotted in Fig. 2. From *a priori* considerations, there exist several reasonable bases upon which to predict the relative latencies of the various choice-combinations as a function of the alternatives presented. Some interesting suggestions are provided by rank-difference correlations computed between the observed latencies and the ranking of latencies predicted from consideration of the total number of pellets presented, the size of the larger number of pellets presented, the difference in size between the two groups of pellets, and combinations of these factors.

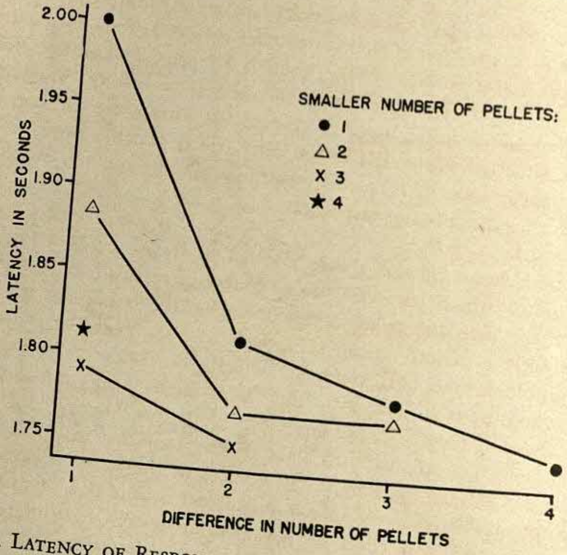

FIG. 2. LATENCY OF RESPONSE FOR EACH OF THE CHOICES IN FINAL BLOCK OF SERIES-I TRIALS

Although more parsimonious explanations might be offered, the latency-data for Series I seem to be most completely explained by invoking three factors. At asymptote, response is more rapid if the total number of pellets presented is large, or if the number of pellets in the larger reward is large. These two factors are themselves highly correlated, and exact analysis of their separate contributions is not possible, but the data are consistent with the suggestion that the total number is more important in the early trials, when responses and latencies are primarily random and the choice-situation is not well understood, and that the larger reward becomes more important at asymptote. The difference in size of the two rewards also affects latency. At asymptote, a larger difference leads to a faster response; it may be that very early in learning an opposite relation holds, since only a very large difference in reward makes the animal pause and choose carefully. Thus, after the choice-situation is well learned, latency of response can be best predicted by some negative function of the maximal reward possible, and the difference in the size of the stimuli. In Series II, the maximal reward is constant from trial to trial; thus we should not expect the larger number of pellets presented (or the total number presented) to contribute directly to determination of the latency of response. The correlational results are generally in agreement with this prediction. The difference in the number of pellets in the two boxes continues to operate, as a stimulus-factor, correlating positively with the speed of response.

It may be concluded, then, that behavior of the monkeys in this experiment may be explained in the following fashion. In the early trials, *S* learns about the nature of the choice-situation, the responses that may be made, and their various outcomes. Only when the stimuli (and the rewards) are greatly different, does he choose carefully. When the situation is well learned, all choices (except for random errors) are made of the larger reward, to the limit of *S*'s perceptual ability; decision-time is greater when the difference between the stimuli is smaller. The difference in reward does not limit the accuracy of response at asymptote; the total amount of reward in the situation (primarily in the early trials) and the larger reward in the situation (primarily after it is learned that only one reward may be obtained) do positively influence the speed of response.

Summary. Six rhesus monkeys were allowed to choose between two transparent boxes, in each of which one, two, three, four or five small glucose pellets could be seen. For the first 400 trials, *S* lifted one of the boxes and was allowed to retrieve and eat the pellets he had seen there. For the next 400 trials, the pellets that were visible in the box could not actually be obtained; instead *S* got four pellets for choosing the box with the larger number of 'stimulus-pellets' and nothing for choosing the alternative box.

The animals learned gradually to make the response that led to the larger of the two possible rewards on each trial. The choices of the larger reward were limited by the perceptual ability of the monkeys, and not by the lack of distinctiveness in value of the rewards. An effect of reward-value which *was* differential between trials may be noted: the amount of reward in a trial interacted with the perceptual factor of the relative size of the stimuli to influence the latency of response.

THE PERFORMANCE OF BRAIN-INJURED CHILDREN ON HIDDEN-FIGURE TASKS

By LEONARD COBRINIK, Creedmoor State Hospital, New York

Many studies have demonstrated that brain-injured children and adults perform poorly on tasks involving the isolation of figures from complex surroundings.[1] The Gottschaldt figures and a variety of other stimulus-patterns have been used. Werner and Strauss found more impairment in brain-injured mentally defective children than in non-brain-injured defectives or in normal children.[2] All the brain-injured *Ss* responded to background-configurations, while the control *Ss* were able to isolate and respond to the intended figures. Dolphin and Cruickshank, in a series of studies on cerebral-palsied and normal children of average intelligence, confirmed the finding of impairment in brain-injured *Ss*.[3]

Recently, interest has shifted from the demonstration of impairment to more detailed study of the field-conditions underlying figure-ground performance. Employing normal chilldren aged 4–13 yr., Ghent found that embedded figures were more difficult to perceive than figures which were masked by overlapping lines.[4] There was a difference also in the age at which normal children achieved mastery of the several tasks. Earlier it had been noted that brain-injured adults found embedded figures like the Gottschaldt more difficult than 'mixed' or overlapping stimulus-patterns.[5] Embedded figures of the Gottschaldt type have not yet been used with brain-injured children.

In view of the findings that normal children also experience difficulty with embedded figures, the present study of the influence of field-conditions on groups of normal and brain-injured children was planned. Its major purpose was to determine whether differences between brain-injured and normal children are consistent for a variety of hidden-figure tasks. A secondary concern was with the relationship of intelligence and of degree of motor impairment to hidden-figure performance.

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* Received for publication January 8, 1959. This paper, based upon a doctoral dissertation presented to the Graduate School of Arts and Science, New York University, was directed by Dr. H.-L. Teuber.

[1] H.-L. Teuber and Sidney Weinstein, Ability to discover hidden figures after cerebral lesions, *A.M.A. Arch. Neurol. Psychiat.*, 76, 1956, 369-379; Heinz Werner and A. A. Strauss, Pathology of the figure-background relation in the child, *J. abnorm. soc. Psychol.*, 36, 1941, 236-248; W. S. Battersby, H. P. Krieger, Max Pollack, and M. B. Bender, Figure-ground discrimination and the abstract attitude in patients with cerebral neoplasms, *A.M.A. Arch. Neurol. Psychiat.*, 70, 1953, 707-712; Sidney Weinstein, H.-L. Teuber, Lila Ghent, and Josephine Semmes, Complex visual task performance after penetrating brain injury in man, *Amer. Psychologist*, 10, 1955, 408 (abstract); H.-L. Teuber, W. S. Battersby, and M. B. Bender. Performance of complex visual tasks after cerebral lesions, *J. nerv. ment. Dis.*, 114, 1951, 412-429; J. E. Dolphin and W. M. Cruickshank, The figure-background relation in children with cerebral palsy, *J. clin. Psychol.*, 7, 1951, 228-231; Visuomotor perception of children with cerebral palsy, *Quart. J. Child Behav.*, 3, 1951, 198-209.

[2] Werner and Strauss, *op. cit.*, 236-248.

[3] Dolphin and Cruickshank, *op. cit.*, 228-231, 198-209.

[4] Lila Ghent, Perception of overlapping and embedded figures by children of different ages, this JOURNAL, 69, 1956, 576-587.

[5] Teuber, personal communication.

METHOD

Subjects. The *S*s were 36 children with cerebral palsy (*CP*) and 36 normal children ranging in age from 6–11 yr. of age. The normal children were obtained from a public school; the *CP* children, from a variety of sources, including *CP* classes in the public schools, schools supported by United Cerebral Palsy, and *CP* outpatient-clinics. The *CP* group contained equal numbers of boys and girls. The normal group contained 19 boys and 17 girls. Each of the groups was divided into six subgroups according to age, each subgroup comprising six children within a given year-range, *e.g.* 0–6 through 6–11 yr.

Only children with average intelligence were used. Normal children who were considered to be of average intelligence by their teachers were given an abbreviated Stanford-Binet Form *L,* and only those with *IQ*s of 89-111 were accepted. The *CP* children, whose *IQ* ranged from 86–112, were selected on the basis of previously recorded intelligence-test scores. Mean *IQ*s for the normal and *CP* groups were 101.4 and 96.9, respectively ($t = 2.68$, $p < 0.01$).

TABLE I

NUMBER OF CEREBRAL-PALSIED *S*S IN EACH DIAGNOSTIC CATEGORY

Diagnostic category	Degree of impairment				Total
	Mild	Mild-moderate	Moderate-severe	Severe	
Spastic:					
Quadriplegia	1	1	6	4	12
Paraplegia	2	0	3	0	5
Triplegia	0	0	1	0	1
Rt. Hemiplegia	2	0	0	0	2
Athetosis:					
Athetoid mainly	1	2	3	1	7
Tremors mainly	0	0	0	4	4
Ataxia	1	3	0	0	4
Flaccid hemiplegia	0	0	1	0	1

The *CP* group included *S*s with many neurological disorders and varying degrees of motor impairment. Ratings of severity were based upon the combined, independently obtained, ratings of three judges.[6] The judgments were based upon a summary of the child's physical status obtained from the case-history and from *E*'s recorded observations. This information is summarized in Table I.

Tests. Three hidden-figure tests were constructed with the general purpose of representing different points (or ranges) on the continuum described by Ghent.[7] The tests, each of which consisted of 10 items, made use of simple everyday figures. They are illustrated in Fig. 1.

The *overlapping* test was designed to represent the lower end of the masking continuum: shared points (rather than contours) of figure and background. The figures, simple objects drawn in black and white, were intersected by lines and parts of objects belonging to the same category. Each card presented the masked condition, and, adjacent to it, four multiple-choice figures in the same category, one of which was identical with the hidden figure. The *S* was required to point to the correct alternative and show where it was located in the masked condition. Attached to each card was an overlay consisting of: (a) a semitransparent plastic material which covered the areas surrounding the hidden figure, and (b) a drawing on clear plastic which corresponded exactly to the contour of the figure. After completion of the entire test, each overlay was placed over all items which had been failed, and *S* was asked again to find the hidden figure.

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[6] I am grateful to Dr. Lila Ghent and to Dr. Sidney Weinstein, who served with me as judges.

[7] Ghent, *op. cit.*, 576-587.

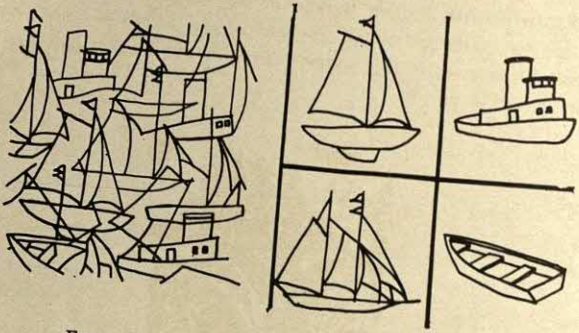

FIG. 1. SAMPLE OF AN OVERLAPPING FIGURE

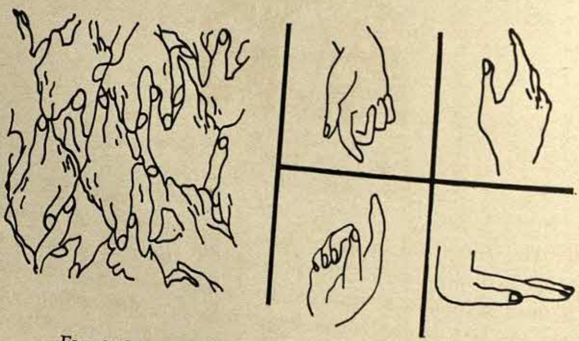

FIG. 2. SAMPLE OF A NON-OVERLAPPING FIGURE

The *non-overlapping* test represented the second point on the above mentioned continuum: shared contours of figure and background. Actually, these cards involved a number of masking principles and not all of them could be said to involve contour-sharing in the strict sense of the term. Several of the cards consisted simply of a physical juxtaposition of figure and other 'part figures,' the figure itself remaining intact, *i.e.* not being intersected. Others involved simple continuation of figural contours or of lines within a figure. Four of the 10 cards involved greater or lesser degrees of contour-sharing in the strict sense of the term. This test was most clearly differentiated from the overlapping series by the fact that the figures were in no case intersected by background-forms. In other details, such as the use of a multiple-choice technique, use of overlays, and so forth, this series was similar to the overlapping figures.

The *puzzle-picture* test was considered to approach the upper end of the masking continuum: embedding.[8] As in the Gottschaldt figures, the contours of the puzzle-pictures 'belong' to other, more readily perceived configurations. In these cards, the masking is achieved, at least in part, by the impression of a meaningful whole to which the hidden object does not belong. To find the hidden figure, *S* must be able to 'yield' his initial impression and search for new configurations. There were no alternatives in this series. The child was simply told what the hidden object was and asked to find it.

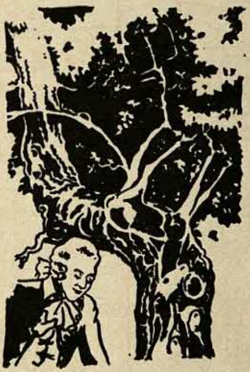

FIG. 3. SAMPLE OF THE PUZZLE-PICTURES
(Find the colt.)

Procedure. The three hidden-figure tests were given to all children. Testing usually required two sessions except with several of the more impaired *CP* children, for whom a third session was required. The normal children were tested with the Stanford-Binet and one of the hidden-figure tests in a first session, and they completed the battery in the next. The schedule for the *CP* children was more flexible. *E* attempted to give two of the hidden-figure tests during the first session. The order in which the tests were presented was systematically balanced to include all possible orders for the six children in each year-group.

Each child was tested individually. If the first test was either the overlapping or non-overlapping one, *E* pointed out the alternatives and told *S* that he was to find one of the four objects within the masked condition. Since parts of all of the alternative figures were included in the latter, it was emphasized that the whole or complete figure, rather than a part, had to be found. Errors were corrected on a sample card. During the test-proper, a child was cautioned up to two times for errors on the first four cards, after which no further aid was given. The time-limit was 1 min. for each card. The score was the number right in each series.

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[8] Adapted from picture-puzzles appearing in *Children's Activities,* published by the Child Training Association, Inc. I am indebted to Miss Lillian Davidson, editor, and to the Child Training Association, for permission to use this material.

The puzzle-pictures were presented differently. *S* was told in each case to find a given object in the presented scene. Two sample cards were presented and any errors corrected. Since the orientation of the pictured scene was seldom the same as the normal orientation of the hidden object, each card was presented first in its proper orientation for the pictured scene, and then rotated clockwise by 90°-turns until either a correct response was made or the card returned to its original position. The card was presented for 12 sec. in each position, except that in the position for which the hidden figure was in its normal orientation the time was 15 sec.

RESULTS

The mean scores of the normal and *CP* groups on each test as a function of age are presented in Table II. Both groups improved with age on all tests, and the normal group was superior to the *CP* group at all ages. These data were analyzed by a mixed-type three-factor analysis of variance. The variables considered were: age, type of task employed, and group. The results show that all of the major variables significantly influenced performance ($p < 0.01$), but that they did not interact. The puzzle-pictures were more difficult than either of the other tests, although not differentially so for the *CP* and normal groups. On the basis of the present data, it cannot be said whether the greater difficulty of the puzzle-picture reflects some inherent complexity of this type of task or is specific to the figures employed.

TABLE II

MEAN SCORES OF NORMAL AND CEREBRAL-PALSIED SS ON EACH OF THE THREE TESTS

Tests	Ss	Age					
		6	7	8	9	10	11
Overlapping figures	Normal	4.33	6.50	7.33	6.83	8.33	9.17
	CP	2.50	4.67	3.63	4.17	5.50	6.67
Non-overlapping figures	Normal	5.50	6.33	7.17	7.67	8.33	7.50
	CP	3.67	5.00	4.83	4.83	5.83	7.17
Puzzle-pictures	Normal	5.50	6.50	6.33	6.33	7.17	7.67
	CP	3.17	3.67	3.67	4.67	2.83	6.33

In the present sample, with its limited range of *IQ*s, intelligence-test scores were not significantly related to the ability to detect hidden-figures. Product-moment correlations between *IQ* and hidden-figure performance ranged between -0.31 to $+0.23$, none reaching the 5% level of significance.

Performance on the hidden-figures also was studied as a function of degree of motor impairment. Since the severity-ratings were found to be disproportionate with respect to age, each score (total of three tests) was converted into a deviation-score from the mean of the appropriate age-group. An analysis of variance demonstrated that this was a significant variable ($p < 0.01$). The mean scores of the groups in the order mild to severe were 8.19, 7.28, 5.63, and 3.14. The group with severe motor impairment was significantly inferior in performance to all other groups ($p < 0.05$). The difference in favor of the mild over the moderate-severe group fell just short of significance.

DISCUSSION

In demonstrating that *CP* children are impaired in their performance on hidden-figure tasks, this study replicates previous results. The finding that performance improves with age also agrees with previous work.[9]

The fact that *CP* children were impaired on all tests deserves further comment in view of the predictions based on Ghent's work. It may be important to note that while the over-all statistical analysis failed to reveal a significant test × group interaction, there were wide differences in the relative performances of the groups on individual items. Thus, large numbers of *Ss* in both groups passed the simpler items; on other items, there were either slight differences in favor of the normal children or instances in which four times as many normal as *CP* children passed. Such findings cannot, of course, be taken to indicate basic differences in the tests used, but they may indicate the complexities involved in hidden-figure performance.

One of the complicating factors is that of item-difficulty. Analysis reveals that the groups perform about equally well on the simpler items, but that the *CP* children perform relatively less adequately as the items become more difficult. This finding suggests that group-differences may be influenced by choice of items. Item-difficulty also was found, however, to depend on identifiable aspects of the stimulus-field.[10]

The finding that severity of motor impairment is a significant factor in hidden-figure performance is of great interest. If one assumes that severity of motor impairment is indicative of degree of brain-injury, it would appear that impairment of performance increases with neurological involvement. The group with severe motor impairment included *Ss* in different diagnostic categories and, most likely, with great variation in size and extent of the lesion. The results suggest that hidden-figure deficit is more a matter of the extent of cerebral damage than of location of the lesion, in confirmation of Teuber and Weinstein's findings.[11] It is of further interest that the groups which were differentiated on the basis of severity of motor (and thus presumably neurological) impairment could not be so differentiated on the basis of *IQ*.

SUMMARY

This study was concerned with the performance of normal and cerebral-palsied children on a variety of hidden-figure tasks. Normal children demonstrated superior performance on all tests. The performance of children of both groups increased significantly with age. Severity of motor impairment in the *CP* group was a significant factor in performance, while test-intelligence was irrelevant. The findings suggest that impairment in hidden-figure performance may be more dependent upon extent than locus of cerebral damage.

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[9] Ghent, *op. cit.*, 576-587; H. A. Witkin, Individual differences in the ease of perception of embedded figures, *J. Pers.*, 19, 1950, 1-15.

[10] It is perhaps significant that the most difficult items in the non-overlapping figures were those judged to involve the greatest degree of contour-sharing. It is of further interest that these items also appeared to differentiate the normal and *CP* groups more effectively than other items in this series.

[11] Teuber and Weinstein, *op. cit.*, 369-379.

CONDITIONING TEST OF STIMULUS-PREDIFFERENTIATION

By MATTHEW YARCZOWER, Naval Medical Research Institute

In a recent experiment, Arnoult studied the effects of pretraining with a set of nonsense-forms on the ability of *Ss* to discriminate the forms in subsequent tests.[1] Experimental *Ss* learned paired-associates made up of the curved forms of Fig. 1 as stimulus-items and letters of the alphabet as response-items. Control groups were trained with the linear forms of Fig. 1 or had no pretraining at all. In subsequent paired-comparisons and tachistoscopic tests, the groups did not differ significantly, although there was some indication that positive transfer might have been manifested if the original training had been carried farther. The failure of significant differences to appear in Arnoult's experiment is a matter of some theoretical importance in view of the increasing interest in the relationship between learning and perceptual phenomena.[2]

In the experiment reported here, Arnoult's forms were employed once more, but level of pretraining was varied systematically, and subsequent discrimination was measured by the galvanic skin response (*GSR*).

METHOD

Predifferentiation training. Four groups of 10 *Ss* each received paired-associates training on either of two sets of nonsense-shapes. One experimental group (Group E_1) and one control group (Group C_1) were trained until one correct anticipation per paired-associate was given, while the other two groups (Groups E_3 and C_3) were trained to three consecutive correct anticipations. All training was done by the adjusted learning method. Each *S* was allowed to study the five stimuli for 30 sec. prior to the actual paired-associates trials. The stimulus-shapes and the associated words are shown in Fig. 1.

Conditioning trials. Immediately after the pretraining trials, *GSR*-electrodes were placed on the palmar and dorsal side of the left hand and the shock-electrodes were attached to the right wrist. Individual thresholds of faradic stimulation were determined and the intensity of shock employed for each *S* was based on this determination. One of the five stimulus-shapes used in pretraining the experimental group was used as a conditioned stimulus (*CS*) in the subsequent conditioning trials. The *CS* used varied from individual to individual with the requirement that each of the five experimental stimuli be used with equal frequency. Ten con-

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* Received for publication December 8, 1958. This paper is adapted from a doctoral dissertation submitted to the Faculty of the Graduate School of the University of Maryland.

[1] M. D. Arnoult, Transfer of pre-differentiation training in simple and multiple shape discrimination, *J. exp. Psychol.* 45, 1953, 401-409.

[2] H. W. Hake and C. E. Eriksen, Role of response variables in recognition and identification of complex visual forms, *ibid.*, 52, 1956, 235-243; J. S. Robinson, The effect of learning verbal labels for stimuli on their later discrimination, *ibid.*, 49, 1955, 112-114; E. J. Gibson, Improvement in perceptual judgments as a result of controlled practice or training, *Psychol. Bull.* 50, 1953, 401-431.

ditioning trials were given in which the *CS* was presented for 4 sec., and the *US* (shock) administered in the final 2 sec. The intertrial interval varied from 20-50. The choice of the interval was governed somewhat by the responses of *S*. Anticipatory *GSR*s were noted, and the interval varied in an attempt to inhibit this reaction.

Discriminative task. The test-trials were conducted without any break in procedure after the conditioning trials. They consisted of 25 differential conditioning trials, which can be described best as 5 blocks of 5 trials each. Each block of trials consisted of the *CS* paired with the *US* and the other four consisted of test-stimuli (experimental shapes) presented alone. These test-trials yielded the generalized response (*GGSR*). The interval between successive stimuli varied from 12-30 sec. When the test-trials were completed, *S* was engaged in a discussion designed to minimize rehearsal. After 15 min. had elapsed, another 25 trials were presented to the same manner as before. These trials—the 'spontaneous recovery' trials—were presented to determine whether a 15-min. rest-interval was sufficient to produce any spontaneous recovery of the generalization—potentials, and, if so, to evaluate the recovery of these potentials with respect to the four groups.

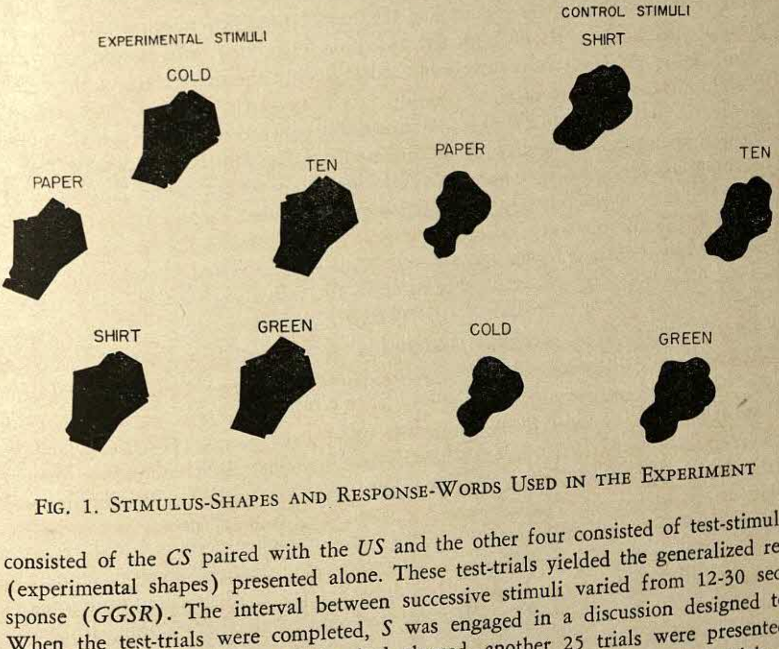

FIG. 1. STIMULUS-SHAPES AND RESPONSE-WORDS USED IN THE EXPERIMENT

Apparatus. The paired-associates were presented by means of a device which allows for an adjusted learning. A description of the device is presented in detail elsewhere.[3]

During the conditioning trials, the stimuli were presented in the same manner as in the learning of paired-associates, but the shutter was not opened at the end of the 2-sec. period. Instead, the *US* was administered to the right wrist of *S* for approximately 2 sec. The shock-circuit consisted of two 1.5-v. dry cells in series with the primary of an inductorium. The two copper electrodes (1.5 × 2 in.) were shaped to fit comfortably on *S*'s right wrist. The timing circuit was the same as that for the learning of paired-associates. The measurement of the *GSR* was made by means of a bridge-circuit connected to a General Radio DC Amplifier (No. 715-A) and in turn to an Esterline Angus Milliammeter Recorder (RD-49/U). *S* was connected to this circuit by two zinc electrodes housed within a plastic cup to the back of which metal plugs were soldered. The connecting leads were inserted into these plugs and tightened by the screw-down tops. Electrode paste filled the $\frac{1}{16}$-in. space between the electrode and the top of the open plastic cup. The paste was made of equal parts of bentonite and glycerin diluted with Ringer's solution. A U.S. Navy ANB-H-1A headset was used to reduce the noise-level of the experimental room.

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[3] A. H. Shephard and Don Lewis, A new device for presenting paired-associates in verbal-learning, this JOURNAL 63, 1950, 600-606.

Subjects. The *Ss* were 40 students in an introductory course in psychology at the University of Maryland. They were assigned at random to one of four groups. All *Ss* were given the option of withdrawing from the experiment at any time. (The only *S* who chose to do so was replaced by another student.)

Results: (1) Predifferentiative training. Analyses indicated that the Group C_3 needed a significantly greater number of trials to reach criterion than did Group E_3 ($p < 0.01$). There was no statistically significant difference between Groups E_1 and C_1. There was homogeneity of variance in both cases. Group C_3, then, had a greater opportunity in which to 'learn-how-to-learn' and for 'warming up.' If the greater number of trials needed to reach the criterion had an effect, it would operate to reduce the predicted superiority of the group E_3.

(2) Conditioning trials. Log change in conductance was used; it was relatively independent of the basal level of conductance, and the distribution appeared to be normal.[4]

(3) Discriminative task. The differential conditioning procedure provided a test of discrimination and generalization. If pretraining reduced generalization (increased differentiation), the effect should be evident in differential conditioning. Generalization was expressed as a ratio: response to the non-reinforced stimulus divided by response to the reinforced stimuli. Generalization as a function of the number of trials is plotted in Fig. 2.

The analyses performed on the generalized data were *t*-tests of the significance of the difference between the means, with adjustments for heterogeneity of variance where necessary.[5] The *t*-tests in which Group E_3 was compared with any other group were one-tailed, since the direction of difference could be predicted; if the direction was reversed one would have to look for artifacts within the experiment that might have produced this effect.[6] The differences obtained were, however, in the expected direction.

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[4] E. A. Haggard, On the application of analysis of variance to GSR data: I. The selection of an appropriate measure, *J. exp. Psychol.* 39, 1949, 378-392; O. L. Lacey, An analysis of the unit of measurement of the galvanic skin response, *ibid.*, 39, 1949, 122-127.

[5] H. M. Walker and Joseph Lev, *Statistical Inference*, 1952, 158.

Group E_3 generalized significantly less than did the other three groups. The *t*s for E_3 vs. E_1 C_3, and C_1 were 2.179 ($p < 0.05$), 2.021 ($p < 0.5$), and 2.266 ($p < 0.05$) with 18, 11, and 15 df. respectively, when all trials were considered. When only the first two trials were analyzed, *t*s of 2.533 ($p < 0.05$), 1.851 ($p < 0.05$), and 1.730 ($p < 0.05$) with 18, 11, and 11 df. were obtained for the same comparisons. No other comparison showed statistical significance.

The results are relatively clear in indicating greater differentiation for the Group E_3 than for the other groups. Group E_3 maintained greater differentiation throughout all the test-trials and throughout the 'spontaneous recovery' trials. The functions depicted in Fig. 2 appear to be the result of transfer of factors both specific and nonspecific to the stimuli. Groups that received little original pretraining (E_1 and C_1) show a decrease in generalization as the number of test-trials increases, although only Group E_1 shows a statistically significant decrease.[7] It would be expected that as exposure to the test-stimuli became greater, the effects of initial warm-up would become minimal. Such appears to have been the case in the present experiment.

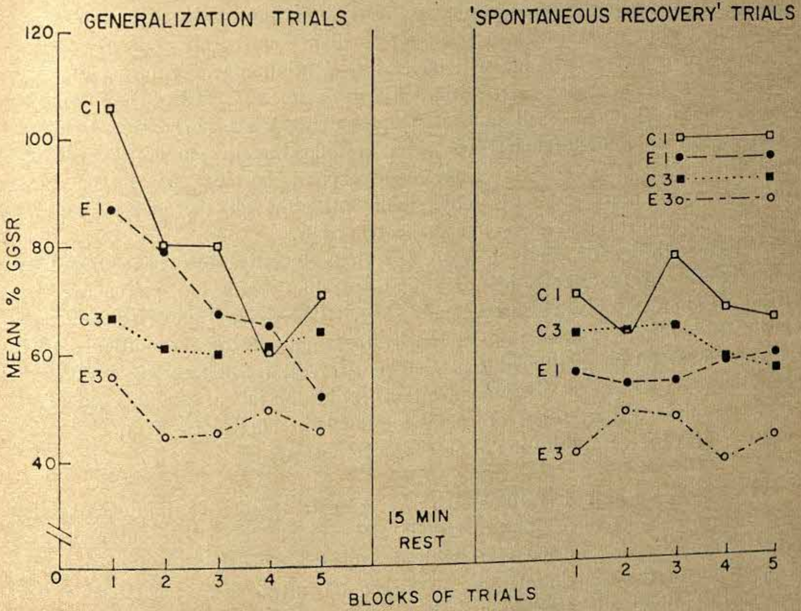

FIG. 2. MEAN PERCENTAGE OF GENERALIZATION OF *GSR* ON DIFFERENTIAL CONDITIONING TRIALS

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[6] H. D. Kimmel, Three criteria for the use of one-tailed tests, *Psychol. Bull.* 54, 1957, 351-353.

[7] Matthew Yarczower, A classical conditioning test of stimulus predifferentiation, Unpublished doctoral dissertation, University of Maryland, 1958, 38.

Discussion. Pretraining involving the attachment of distinctive verbal labels, though irrelevant to the stimulus-dimension, reduces generalization among the stimuli in a subsequent discriminative task. The group given the greatest amount of training was better able to differentiate among the stimuli than any of the other groups; generalization of the *GSR* was at a lower level for this group throughout the entire experiment. This finding supports the hypothesis that predifferentiation-training reduces the intrastimulus-generalization and at the same time supports the assertion that "attaching distinctive cue-producing responses to similar stimulus-objects tends to increase their distinctiveness."[8]

Large amounts of stimulus-pretraining appear to facilitate subsequent differentiation among stimuli. What about relatively small amounts of pretraining? Gibson has speculated: "Generalization will increase to a maximum . . . during the early stages of practice with a list, after which it will decrease as practice is continued."[9] The group receiving a small number of pretraining trials (E_1) generalized *less* than did its appropriate control (C_1), but it did generalize to a greater extent than the control group receiving a greater number of 'warm-up' trials (C_3). None of these differences was statistically significant, although they were consistent throughout the first three blocks of generalization trials.

Although theory demands it, many previous investigations have found no improvement in the recognition of stimuli previously used in paired-associates training. The data obtained in this study suggest that recognition of the stimuli is facilitated when recognition is measured by the amplitude of a conditioned *GSR.* There are several possibilities that may account for the facilitating effects of verbal pretraining on subsequent discriminative tasks: reduction in intrastimulus-generalization,[10] acquired distinctiveness of cues,[11] increased meaningfulness of the stimuli,[12] and transfer of receptor-orienting or observing responses.[13]

Summary. Four groups of 10 *S*s each received paired-associates training involving five nonsense-shapes to one of two criteria of learning. Then all groups underwent classical conditioning of the *GSR* with one of the nonsense-shapes used in the pretraining of the experimental groups as the *CS,* after which generalization was measured in differential conditioning trials.

The results indicated that paired-associates learning, if carried to a sufficiently high level, facilitates subsequent discrimination among the stimuli. Previous research generally has yielded negative results.

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[8] N. E. Miller, Theory and experiment relating psychoanalytic displacement to stimulus response generalization, *J. abnorm. soc. Psychol.* 43, 1948, 174.

[9] E. J. Gibson, A systematic application of the concepts of generalization and differentiation to verbal learning, *Psychol. Rev.* 47, 1940, 206.

[10] *Ibid.,* 196-229.

[11] John Dollard and N. E. Miller, *Personality and Psychotherapy,* 1950, 101; A. E. Goss, A stimulus-response analysis of the interaction of cue-producing and instrumental responses, *Psychol. Rev.* 62, 1955, 20-31; Miller, *op. cit.,* 155-178.

[12] Arnoult, Stimulus predifferentiation: Some generalizations and hypotheses, *Psychol. Rev.* 54, 1957, 339-350; C. E. Noble, An analysis of meaning, *ibid.,* 59, 1952, 421-430.

[13] K. H. Kurtz, Discrimination of complex stimuli: the relationship of training and test stimuli in transfer of discrimination, *J. exp. Psychol.* 50, 1955, 283-292.

THE EFFECT OF ALTERING THE MIDDLE OF THE LIST DURING SERIAL LEARNING

By Robert C. Bolles, Hollins College

The prevailing theoretical account of serial learning is based upon the assumption that all S–R associations are strengthened with every trial. To explain the greater difficulty a subject (*S*) has in learning the middle items of a list, it is customary to suppose that there is a greater accumulation of inhibition in the middle than at the ends. The present paper suggests, alternatively, that *S* does not begin to learn the middle items until the beginning and end of the list have been learned. This hypothesis was tested by an adaptation of the well-known technique of cue-reversal in which the serial order of items in the middle of the list was altered during the course of learning.

Experiment I. Eight adult *Ss* were used; they had no previous experience in experiments of this type. The lists each contained nine nonsense-syllables of 27% to 40% association-value according to Glaze.[1] The lists were presented on a Stoelting memory-drum, with a 2-sec. exposure-time, and were learned by the method of anticipation. Each *S* learned four lists, two under the experimental condition and two under the control condition. Conditions were counterbalanced across *Ss* and lists.

The critical procedural manipulation occurred at a point approximately one-third of the way through the course of learning. At that time, the window of the memory-drum was moved over, revealing an alternate list, on which, for the experimental condition, the fourth and sixth syllables had been interchanged. For the control condition, this alternate list was unchanged. Alterations were made in such a way that *Ss* in the experimental and control conditions ended up learning the same list.

If the S–R sequences in the middle of a list actually do acquire associative strength through a process of gradual accretion in the early trials, we might expect learning to be slower under the experimental condition than under the control condition because four of the eight S–R sequences were changed under the experimental condition. According to the hypothesis advanced here, however, there should be little or no difference between conditions unless *S* should happen to learn a syllable in the middle of the list relatively early, and the universality of the serial-position effect guarantees that that will not happen very often.

One methodological problem to be faced centered about the impossibility of knowing beforehand which trial will be one third of the number necessary to reach criterion. For the later lists, this critical point was estimated from the number of trials taken on the previous list. For the first list, the transition-point was the seventh trial unless the early performance suggested that a revision was necessary.

The results of the experiment are shown in Fig. 1. Clearly, there is no marked

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* Received for publication October 9, 1958.

[1] J. A. Glaze, The association value of nonsense syllables, *J. Genet. Psychol.*, 35, 1928, 255-267.

difference between the results obtained under the two conditions. Separate analyses of variance for errors and for trials to various criteria of learning were carried out, and no significant difference could be found between conditions.

The experimental design permits the partialling out of practice-effects, list-differences, and *S*-differences, leaving a relatively small residual error-term. In fact, the power of these *F*-tests is such that a 'real difference' as small as two trials would probably have been detected. The traditional theory (which might be called the *continuity theory*) of serial learning would seem to require a real difference of about seven trials. These results suggest that serial learning follows non-continuity principles. That is to say, if *S* takes 20 trials to get the last syllable in the middle of a list, he is probably not getting his full 20 trials' worth of practice on that particular syllable.

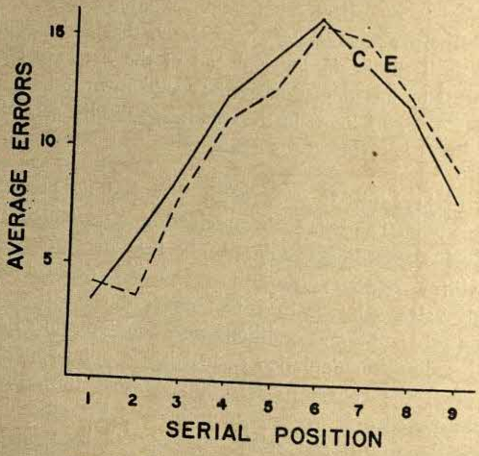

FIG. 1. RESULTS OF EXPERIMENT I

E = experimental; syllables 4 and 6 interchanged one-third of the way through the experiment; mean trials for learning 21.9; C = control; mean trials, 22.7.

There remains a question of whether S is learning anything *at all* about the syllable in the early trials. The second experiment is addressed to this question.

Experiment II. The procedure was essentially the same as before except that 6 *Ss* were used, and they first learned two practice-lists to reduce possible practice-effects, and then they were given 12 lists to learn in three experimental sessions. There were three conditions counterbalanced across lists and *Ss*. Two of these conditions were the same as before, thereby providing a replication of Experiment I.

Under the third and new condition, when the window of the memory-drum was moved over, the alternative list revealed new syllables inserted in the place of the old Syllables 4 and 6. Continuity theory suggests that *S* would lose some time under this condition if only in learning to recite and in becoming familiar with the novel items.

The results did not, however, confirm that expectation. Fig. 2 shows the serial-position curves obtained under the three conditions. Again, it is clear that there is no decrement under the experimental conditions; if anything, learning proceeds faster in the unstable learning situations. In some instances, this may be the case. In post-experimental interviews some *S*s report that their attention was caught by the appearance of a novel syllable, and, paying particular attention to it, they learned it easily. The protocols show that changing the position of a syllable may also cause it to be learned quite quickly, sometimes in one trial.

Most *S*s did indicate in the interview that they detected changes in the lists. Few, however, had a very clear picture of what changes were involved. There was no indication that any *S* deliberately 'suppressed' the learning of the middle syllables prior to the transition. On the contrary, 36% of the time *S* had correctly anticipated the fourth syllable prior to the introduction of the alternative list, making it just that much harder to demonstrate non-continuity.

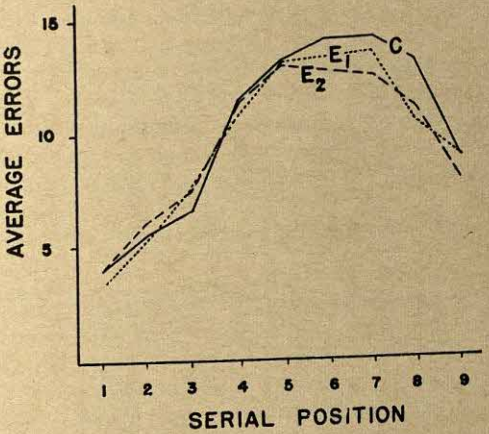

FIG. 2. RESULTS OF EXPERIMENT II

E_1 = experimental; syllables 4 and 6 replaced by new ones; mean trials for learning 20.5. E_2 = experimental; syllables 4 and 6 interchanged; mean trials, 20.6. C = control; mean trials, 22.0.

The results of this study support the view that the associative strength of S–R sequences in the middle of a list is not gradually built up in a continuous manner through all of the trials. It seems more likely that the learning of the middle syllables begins after the adjoining syllables have been learned. These results are suggestive of those of Rock, who argues that the associative strength necessary for performance is acquired in one trial.[2] If this is the case, then we have to explain why the performance of the whole list requires 20 trials.

An answer to this question can perhaps be obtained only by an analysis of the competing responses. The appearance of any given item not only provides the occasion for the correct response but it arouses the response that is to follow and it is also associated with several responses that compete with the correct one. One competing response is the recitation of the syllable presently showing; others are those of waiting and watching. These competing responses have the advantage over the correct one of postremity and frequency. They have, moreover, the advantage of rhythmicity. The conditions which presumably elicit these competing responses become disturbed whenever the preceding or succeeding syllables has just been learned.

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[2] Irving Rock, The role of repetition in associative learning, this JOURNAL, 70, 1957, 186-193.

Whether this analysis can account for all the facts remains to be seen but one thing is apparent. The time has come when it no longer suffices to look only at the over-all achievement. If we are to understand human learning, we must determine what our *S*s are really doing.

STABILITY-CHARACTERISTICS OF THE SEMANTIC DIFFERENTIAI

By WARREN T. NORMAN, The University of Michigan

The semantic differential is a rating procedure introduced by Osgood for the measurement of meaning.[1] It involves a set of 7-point scales terminating in bipolar adjectives, such as *good-bad*. *S*s are asked to rate each concept on the entire set of scales, and the rating profile which results is said to indicate the 'meaning' of the concept. Osgood and Suci have proposed that the statistic D may be used as a measure of the degree of meaningful similarity between any two concepts so rated.[2] On the assumption that the scales are mutually orthogonal, with a common unit and zero-point, the scale-values are treated as coördinates of a point representing the concept in the space defined by the scales; the degree of similarity between a pair of concepts is inversely proportional to the distance between them. To facilitate studies of verbal behavior with this technique, an atlas of semantic-differential profiles on a set of 360 concepts has been constructed.[3] Data on interrelationships among the 360 words, including a set of 64,620 D-values, also are available.[4] The present study is concerned with the stability of the semantic differential over time and *S*s.

Method. The original set of 360 words for the Atlas had been grouped into sets of 20 for purposes of reducing the amount of time required for each *S*. Hence, 18 groups of 30 *S*s (15 men and 15 women) were used in collecting the data. To study the various kinds of time-lapse stability and the effect of sampling variability, 30 comparable *S*s, drawn from the same population, were asked to rate a single set of 20 concepts chosen at random from the 18 original Atlas-sets. Instructions, forms, and scales identical to those used in the Atlas-study were employed. After a period of four weeks. the *S*s performed the same task again. The choice of a complete set from the Atlas-study rather than a sampling of individual concepts permitted a determination of *S*-sampling variability in ratings and D-values with the effects due to concepts and scales held constant.

While the coefficient of correlation is the traditional index of stability, it is not always an appropriate or a meaningful one. For certain purposes, a percentage-index may be more instructive (and sobering). Consider the distribution of the ratings of the first-administration of the set of 20 concepts, on the 20 scales of the semantic

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* Received for publication October 30, 1958. This study is one of a series on verbal behavior conducted at the University of Minnesota under contract with the Office of Naval Research.

[1] C. E. Osgood, The nature and measurement of meaning, *Psychol. Bull.*, 49, 1952, 197-237.

[2] C. E. Osgood and G. J. Suci, Factor analysis of meaning, *J. exp. Psychol.*, 50, 1955, 325-338.

[3] J. J. Jenkins, W. A. Russell, and G. J. Suci, An atlas of semantic differential profiles for 360 words, this JOURNAL, 71, 1958, 688-699.

[4] Jenkins, Russell, and Suci, *Table of Distances for the Semantic Differential Atlas,* Technical Report No. 20, ONR contract N8 onr-66216, University of Minnesota. Available on interlibrary loan.

differential. This distribution consists of $30 \times 20 \times 20 = 12{,}000$ values between *one* and *seven*. The greatest change that an initial rating of *one* (or *seven*) could show in a second administration would be six, *i.e.* it could move to the other extreme of the scale. Similarly, initial ratings of *two* (or *six*) could show shifts no greater than five points, and so forth. With a change in a given rating of one scale-unit defined as a *unit-discrepancy*, one may compute the maximal number of unit-discrepancies possible in a second administration by multiplying the number of initial ratings at each scale-position by the maximal shift possible and summing these products over all seven categories. The number of unit-discrepancies actually appearing in a second administration may be expressed as a proportion of the maximum (*% MUD*). Such an index provides some information that the coefficient of correlation does not, if indeed the later is at all useful in this setting.

The expected rather then maximal number might, of course, be chosen as a denominator for an index of this kind, on the assumption that, under random marking of the scales on a second administration, a shift of any magnitude is equally likely. The resulting indices would be twice as large numerically as the *% MUD*-values but perfectly correlated with them in a linear manner.

Another easily interpreted stability-metric is the *same-word D-value*.[5] Given the profiles of a concept based on average ratings by two different groups or by the same group on two occasions, the *D*-values that can be computed between these pairs of profiles provide lower bounds on the interpretability of between-word *D*s. In many situations, it is more important to know that *D*s of a given value may be generated by the profiles of the same words rated by the same group of *S*s on two different occasions than to know the correlation between the two series of mean scale-values.

Each major 'factor' or 'dimension of meaning' is represented by two or more scales in the total set of 20. To offset possible effects due to response-position biases, some of the scales within each subset had been reversed systematically (*i.e.* the labels identifying the ends of the scales had been interchanged). To obtain 'factor-scores' (unit-weighted sums of the ratings on scales within each subset), those scales which had been reversed were first complemented.

Results: (1) Consistency of individual ratings. For the two complete sets of ratings, the '*% MUD*' was 23. About 40% of the ratings remained the same; 35% shifted by one unit; and 25% changed by two or more units on these 7-point scales. The average shift was slightly greater than one position ($12{,}799/12{,}000 = 1.07$). There was no evidence of a marked sex-difference in over-all consistency.

To determine whether consistency varied markedly from concept to concept, from *S* to *S*, or from scale to scale, separate analyses were performed. Separate analyses also were made for men and women on the concept- and scale-consistency breakdowns.

The range of '*% MUD*-values' for concept-consistency for men and women was about the same (18-29). The average shift in ratings from the first to the second test varied from 0.92 (*leper* and *tornado*) to 1.28 (*stars*) scale-units. (The maximum for such an average shift lies between 3.0 and 6.0, depending on the distribution of initial ratings.) The median corresponding *r* was 0.66 and the values ranged from 0.38 to 0.77.[6]

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[5] This measure was suggested by J. J. Jenkins (personal communication).

[6] All correlations reported are Pearson product-moment coefficients.

The range of the indices for person-consistencies was 17–29% for the men and 18–34% for the women. The average shift in ratings ranged from 0.65–1.76 scale-units. Definite response-tendencies seem to characterize certain of these *Ss'* ratings. For instance, the one woman with the highest percentage-index had an initial rating distribution in which there were no responses in the middle category. Her rather high general level of inconsistency may have resulted from this flip-flip response-set. What seemed at first to be a tendency by the women to use the extremes more often did not stand up when the ratings of the Atlas-group on the same words were plotted.

The data on scale-consistency also indicate little sex-difference in variability. In the pooled results for men and women, the average shifts in ratings varied from 0.81–1.41 scale-units. The least consistent scale for the set of words used was *curved-straight*. The coefficient of correlation corresponding to this scale's combined percentage-index of 29.7 was 0.44. The most consistent scale was *beautiful-ugly*, which had a stability-coefficient of 0.76 and a percentage-index of 17.1. The scale-consistencies (*% MUD*) for men and those for women correlated 0.92, as compared to a corresponding coefficient of 0.67 for concept-consistencies.

TABLE I

CONSISTENCY-INDICES FOR FACTORS

Factor	No. of scales	Percentage *MUD*		*r*	
		Men	Women	Men	Women
Evaluation	8	13.6	14.3	0.79	0.79
Potency	3	16.9	16.1	0.77	0.75
Activity	3	16.7	19.0	0.82	0.78
Tautness	2	22.4	26.6	0.54	0.52
Novelty	2	25.4	23.1	0.37	0.59
Receptivity	2	19.5	20.6	0.69	0.67

(2) Consistency of factor-scores. Osgood, taking cognizance of the general instability of the ratings on the separate scales, has suggested that factor-scores rather than single-scale values be used in work with a single *S*.[7] Accordingly, the data from the test-retest study were processed to yield two-scale, three-scale, and eight-scale factor-scores for the various combinations of scales included in the total set of 20 employed. The scales for each factor were chosen on the basis of previous factor-analyses done at Illinois. A factor-score was formed by unit-weighting those scales which had been included to represent the given factor. Percentage-indices and corresponding coefficients of correlation are given in Table I.

The moderate increases in consistency reflected by the percentage-indices and coefficients of Table I for two-, three-, and eight-scale factors partly justify Osgood's recommendation. It should be noted, however, that the increases in stability are not very large. The two-scale factors are hardly more stable than are the separate scales. The three-scale factors are about as stable as the best single scales. On the eight-scale factor, we still may expect about 14% *MUD* over a four-week interval without specific treatments. The picture is much the same when viewed in terms of the correlational measures. The one factor represented by eight scales has test-retest correlations of less than 0.80. That is, more than 35% of the variance in the second administration is unaccountable in terms of the first set of ratings. It appears, therefore, that the practice of computing factor-scores, especially for factors represented only by a few scales, is not likely to be a fruitful one.

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[7] Personal communication.

(3) Stability of individual semantic spaces. The low degree of stability of the individual ratings suggests that distance-functions for single *Ss* based on these ratings also might show low time-lapse stabilities. To avoid excessive computation, only the ratings of the most consistent and the least consistent *Ss* were analyzed. The complete set of 190 inter-word *D*s on each administration plus the 20 same-word *D*s across the two testings were computed for each of these *Ss*. Even for the most consistent *S*, the test-retest stability of the distance-function ($r = 0.37$) was not within the acceptable range. The *r* for the second (least consistent) *S* was 0.16. When same-word *D*s range as high as 17.1 with an average of 11.5, as they did for this *S*, interpretations based on single administrations seem unjustifiable.

(4) Group-mean D-values and group-mean ratings. The correlation between the set of 190 group-*D*s from the first administration with those from the second was 0.97; that between the set of *D*s based on the Atlas-study with those of the first administration of the present investigation (*i.e.* when a different group of *Ss* was involved) was 0.92. It would appear, therefore, that a rather high degree of time-lapse and sampling stability exists for the semantic differential for groups of *Ss* from an undergraduate population. If one correlates the mean scale-values upon which the above *D*s were based, similar results are obtained. The two series of 400 pairs (20 scales × 20 concepts) of mean scale-values yielded coefficients of 0.96 and 0.94, respectively.

(5) Same-word group D-values. When *D*-values for each word from first to second administrations were computed, the 20 values ranged from 1.07-2.04. The range of values for the first administration vs. the Atlas was 1.28-2.92. Hence if one is interested in interpreting group-*D*s (or differences in *D*) based on multiple testings of the same *Ss*, values or changes in value less than 2.0 should not be taken very seriously. If the samples change also, then perhaps values of 3.0 or less may represent nothing more than accumulated error over the 20 scales.

The group-mean ratings and *D*s therefore show very high stability over time in absence of any systematic intervening treatment. Hence, experiments aimed at restructuring 'semantic spaces' are feasible if a large enough number of *Ss* is employed.

MAINTAINING SET IN RELATION TO MOTIVATION AND AGE

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When reaction-time (*RT*) is measured in relation to the fore-period or preparatory interval (*PI*), it is found that the same *PI*s result in different *RT*s, depending upon whether they are in irregular or regular series. If the *PI*s are of relatively short duration, then *RT* is longer with an irregular series. If the *PI*s are relatively long, then *RT*s of a regular series are similar to or longer than *RT*s of an irregular one. The exact *PI*-length in which this is the case depends—among other things—upon the population tested. Thus, Huston, Shakow, and Riggs demonstrated that the *PI*-length at which *RT* is quicker to irregular series than to regular series is shorter in a schizophrenic group than in a normal one.[1] This finding was discussed in terms of maintaining set, *i.e.* schizophrenics, as compared to normals, maintained set less well in the regular series relative to their ability to do so in the irregular series. The purpose of the present study was to determine whether the maintenance of set as defined in this way is related to conditions of motivation and age.

Subjects. The *S*s were 41 volunteer men, divided into two age-groups. The younger group comprised 22 *S*s aged 19–31 yrs., and the older group comprised 19 *S*s aged 65–81 yr. Median ages were 25 and 72 yr., respectively. The older *S*s were chosen only after rigorous medical standards were met. All *S*s were residents of the community.

Procedure. Some aspects of the procedure have been already reported in two previous studies.[2] At the signal 'ready,' *S* pressed the index finger of his preferred hand on a telegraph response-key that set off a 0.50-sec. warning-light. This was followed by the *PI* that was terminated by the auditory stimulus delivered through earphones. The warning-light was a glow from a frosted 7.5-w. lamp. The *PI*s were of 10 lengths in both regular and irregular series and the auditory stimulus was approximately 1000 ~ at 85 db. The response (finger-lift) terminated the auditory stimulus. Interval-timers were used to program the warning signal and the *PI*s. *RT*s were measured to 0.01 sec.

The 10 *PI*s were of the following durations: 1.0, 1.5, 2.0, 2.5, 3.0, 4.0, 5.0, 6.0, 15.0, and 25.0 seconds. In the irregular series, each *PI* was given three times in fixed quasi-random order. In the regular series, each *PI* was presented randomly except for the 15.0- and 25.0-sec. intervals which were given last in alternate order. Each *PI* was given nine consecutive times.

The median *RT* for the total irregular series of 30 measurments was computed for each *S*, as was the median for the nine *RT*s for each *PI* of the regular series.

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* Received for publication January 14, 1959.

[1] P. E. Huston, David Shakow, and L. A. Riggs, Studies of motor function in schizophrenia: II. Reaction-time, *J. gen. Psychol.*, 16, 1937, 39-82.

[2] Jack Botwinick, J. F. Brinley, and J. E. Birren, Set in relation to age, *J. Geront.*, 12, 1957, 300-305; Jack Botwinick, J. F. Brinley, and J. S. Robbin, The effect of motivation by electrical shocks on reaction-time in relation to age, this Journal, 71, 1958, 408-411.

Ss were informed that other series would be given with a mild shock to the wrist of the responding hand if *RT* was longer than the median *RT*. The shock was 0.20 ma. applied with a constant current stimulator for 0.20 sec.

All *Ss* performed in the non-shock procedures, with the regular series being given first to approximately half the *Ss* in each age-group. Then, for a different purpose, *Ss* were given non-shock followed by shock *RTs* to irregular series of *PIs* ranging from 1.0 to 6.0 sec. This work usually was done on a second day. Then the shock-*RTs* of the present study were obtained, usually on a third day. The irregular and regular shock-series were given in the same order in which they were administered during the non-shock series.

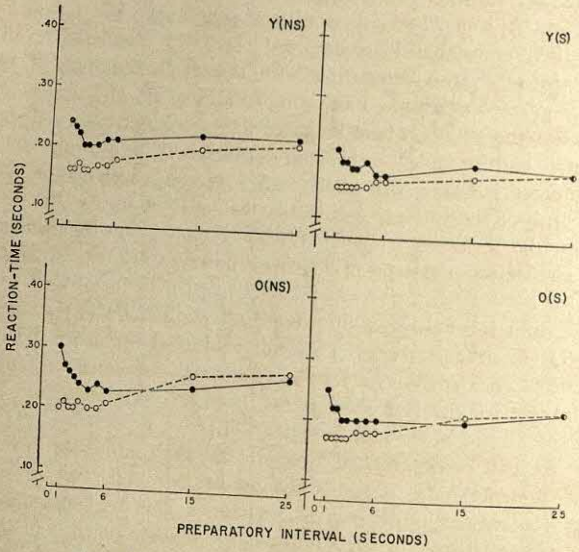

FIG. 1. MEAN *RT* AS A FUNCTION OF THE *PI*

○ - - - ○ = regular series
●—● = irregular series
Y = *Ss*, 19-31 yr. old
O = *Ss*, 65-81 yr. old

NS = verbal instruction to respond as quickly as possible
S = shock applied to wrist when response slower than median *NS* reaction

Results. The results of this study are presented in Fig. 1. It may be seen in this figure that the *PIs* of 6.0 and 15.0 sec. are the crucial ones for testing the maintenance of set, since it is between these two *PIs* that *RTs* in some conditions are longer with regular series than with irregular series. Accordingly, *t*-tests of differences between age and between motivational conditions were made with respect to *RT*-differences between these two *PIs* and the two types of *PI*-series.[3] That is, *t*-tests were made of the interactions between the two *PIs*, the two types of *PI*-series, and, in one case, the two conditions of motivation, and, in the other case, the two age-groups. Tests were based upon median *RT*s of each *S* for each *PI*-condition.

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[3] In addition, variance analyses were made of the *RTs* associated with the *PIs* of: (a) 6, 15, and 25 sec.; (b) minimal *RT* within each type of series, 15 and

When the difference in *RT* between the *PI*s of 15.0 and 6.0 sec. was *t*-tested for age with respect to the type of *PI*-series (PI × age × PI-series), statistical significance was found in both motivational conditions combined ($p < 0.02$), in the shock-condition ($p < 0.01$), but not in the non-shock condition ($p > 0.05$). Although from Fig. 1 it would seem that shock affected the relation between *RT*s of *PI*s 6.0 and 15.0 sec. with respect to the type of *PI*-series (the interaction of motivating condition, *PI*, and type of *PI*-series), statistical significance was not found within the younger, older, or combined age-groups ($p > 0.05$). Similarly, no differential age-effect was found in this interaction with respect to condition of motivation ($p > 0.05$ for interaction of age, motivating condition, *PI*, and type of *PI*-series).

Summing across the 10 *PI*s, t-tests indicated that shock decreased *RT* significantly in both regular and irregular series and within both age-groups ($p < 0.01$). Tests of differences indicated that shock had a greater effect on the *RT*s of the elderly group than of the younger one within the series of regular *PI*'s ($p < 0.01$) but not within the series of irregular *PI*s ($p > 0.05$). The interaction between age, type of motivation, and type of *PI*-series, however, was not significant ($p > 0.05$).

As indicated, nine RT measurements were made for each PI in regular series and three were made in irregular series. This methodological difference did not appear to have an effect, since analysis of *RT*s in relation to sequential measurements indicated that, except for the very shortest *PI*s, little if any improvement in *RT* was seen after the second trial of the nine in regular series.

Discussion. At least two conceptual aspects of *RT* are confounded in the measurements of the present study. There is the aspect that older *S*s are slower than younger ones for reasons other than set. There is also the aspect that changes in *RT* are related to changes in set. *RT*, therefore, is some function of set and non-set factors. The concept of maintaining set as defined in this study involves the consideration that the *PI* at which *RT* is minimal is the point at which non-set factors contribute most to the behavioral measurement. The *PI* at which *RT* is maximal is the point at which non-set factors contribute least. In all cases, however, both factors contribute to *RT*.

Implicit in the formulation of Huston, Shakow, and Riggs,[4] with respect to the set-aspect of *RT* during long *PI*s, is the notion that, in the irregular series, the uncertainty about when the stimulus will occur, or the possibility that it can occur at any time, makes for a high level of attention. The regular series makes for a lower level of attention because of the knowledge that the stimulus will not occur until a period of time elapses. *RT*s associated with long, irregular *PI*s thus are the result of maintained set.

Accepting this formulation of *RT*-set, we interpret the results of the present study as showing that the maintenance of set was affected by the age of *S* but not

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25 sec.; and (c) minimal RT within each type of series, and 15 sec. The *F*-ratios in these analyses that are associated with the effect of shock-motivation and of age on the maintenance of set (motivational condition × *PI* × type of *PI* series, and age × *PI* × type of *PI* series) were congruent statistically with the comparable tests involving the 6- and 15-sec. *PI*s.

[4] Huston, Shakow, and Riggs, *op. cit.*, 39-82.

by shock. Since shock did reduce *RT*s significantly in all conditions, and since, on a subjective level, the threat of shock appeared to be present, its lack of effect on the maintenance of set emphasizes the importance of the positive result with respect to age of *S*.

The problem of set may be quite different, at least insofar as age is concerned, when the *PI*s are of short duration. This aspect was investigated in a previous study.[5] *RT*s were measured in relation to *PI*s of 1.0-6.0 sec. in irregular series, and disproportionately slower *RT*s were found for the elderly with the shorter *PI*s. One explanation of the results was in terms of age-difference in the time taken for organization or preparation of response. Another explanation was in terms of overestimation of the *PI* because of an expectancy of a *PI* near the mean or median of the series. In the regular-interval data of the present study, no disproportionate age-increase in *RT* was found with the shorter *PI*s. This formulation suggests that when there is relative uncertainty about when stimuli of short *PI*s will occur, the age-difference is either in the organization of response or in the reorganization as a result of inaccurate expectancy. When, as was the case with the long *PI*s of the regular series in the present study, there is relative certainty, but stimulus-occurrence is delayed, the age-difference is in maintaining attentive set.

Summary. Simple auditory *RT* was measured in relation to preparatory intervals (*PI*) in both irregular and regular series for *S*s in two age-groups and under two conditions of motivation. Median ages of the two groups were 25 and 72 yr. One condition of motivation involved verbal instruction to respond as quickly as possible. A second condition involved a mild shock to the wrist for *RT*s slower than individual median non-shock *RT*.

The results indicated that with longer *PI*s, *RT*s of the elderly group relative to those younger group tended to be slower with regular series than with irregular series. It was concluded that the younger group maintained set better than did the older group. Shock-motivation decreased *RT* but had no significant effect on the set-phenomenon.

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[5] Botwinick, Brinley, and Birren, *op. cit.*, 300-305.

SERIAL LEARNING UNDER CONDITIONS OF RAPID PRESENTATION OF STIMULI

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The experiment to be reported here concerns the learning of nonsense-syllables under conditions of very rapid presentation, with the ratio betwen interstimulus-interval and duration of exposure held constant. Experiments on serial learning usually have involved intervals of several seconds between stimuli, though there have been several utilizing relatively brief intervals. Bergstrom exposed 12 syllables for 0.082 sec. each, using intervals of 0.30, 0.69, and 1.45 sec. between their successive exposures.[1] He found that amount learned increased with the length of the intervening interval. Froeberg, utilizing the paired-associates method, had intervals of 0, 1, 2, 3, 4, and 5 sec. between the successive members of each pair.[2] Under these conditions, learning proceeded best with shorter intervals. Guthrie used a continuous belt of motion-picture film to present 10 pairs of stimuli.[3] Each frame was exposed for 0.41 sec., with the intervals between the members of the pairs varying, in three conditions, from 2.55 to 4.93 sec., and the intervals between successive pairs being 5 sec. Learning was best for the longer intervals. Hovland utilized 12 nonsense-syllables under two different speeds of presentation—2 and 4 sec.—and found errors to be notably less for the longer interval.[4] It may be noted that the time-periods involved in these studies, while brief in comparison with the intervals used in typical serial-learning studies, are not particularly short when compared with exposure-times used in tachistoscopic studies (and known to be adequate for perception of a stimulus) or with intervals used in studies of reading. In the present experiment, the stimulus-objects were presented much more rapidly than in any of the studies just cited.

Apparatus. The instrument used can be described as a high-frequency sequence-tachistoscope.[5] Its basic unit was a modified Craig 16-mm. Film Editor. The stimulus-objects (nonsense-syllables) were filmed, and the film joined to make a continuous belt. The belt was driven by an electric motor and its speed was calibrated up to a rate of 170 frames per sec. The mechanism was mounted in a cabinet with only the aperture (3 × 4 in.) visible to *S*. In the present study, the syllables were on every third frame, the frames intervening being blank. There were 7 syllables in the

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* Received for publication January 30, 1959.

[1] J. A. Bergstrom, Effect of changes in the time variables in memorizing, this JOURNAL, 18, 1907, 206-238.

[2] Sven Froeberg, Simultaneous vs. successive association, *Psychol. Rev.*, 25, 1918, 156-163.

[3] E. R. Guthrie, Association as a function of time-interval, *Psychol. Rev.*, 40, 1933, 355-367.

[4] C. I. Hovland, Experimental studies in rate-learning theory. II. Reminiscence with varying speeds of syllable presentation, *J. exp. Psychol.*, 22, 1938, 338-353.

[5] The apparatus was developed, and the film prepared, by Heward and Dixon, 2630 Broadway, Redwood City, California.

sequence to be learned, with a total of 21 frames for each sequence. The sequence was repeated 8 times in the belt of film, making the total of 168 frames. Since the sequences of syllables were joined directly together, there was no first or last syllable, and no intervals between trials. To minimize the effects of clarity and recency attending the first and last syllables which *S* might perceive, a variable transformer (Powerstat, Type 116) was inserted in the light-circuit; the stimulus-aperture was lighted gradually, with the film running, at the beginning of presentation, and darkened gradually at the conclusion.

Material. The material to be learned, which was presented on the film as described above, consisted of the following nonsense-syllables in the order indicated: NIT, YAC, FOL, DET, RUJ, KAF, MUR. These syllables, chosen from Glaze's list of 93% association-values, were arranged insofar as possible in accordance with criteria set forth by Hilgard.[6]

Subjects. The *Ss* were 50 friends and colleagues of the investigators, 36 men and 14 women. All except three were college graduates, and many had served in learning experiments previously, although not in studies in which stimulus-materials were presented rapidly.

Procedure. The *Ss* were divided into four groups (I-IV) of 10 each, and a fifth group (IIa) of 10 *Ss* was added later. The main part of the experiment involved the comparison, with respect to amount learned, of Groups I-IV. Each of these four groups had 16 presentations of the syllables, but they differed in the rate at which the stimulus-objects were presented. The rates were 12, 8, 4, and 2 per sec. for Groups I-IV, respectively, with corresponding stimulus-intervals (defined as the time between the appearance of one syllable and the appearance of the next) of 83, 125, 250, and 500 m. sec. Group IIa, with the same rate of presentation as Group II, but with twice as many presentations (32), was added later to make possible at least one comparison between two groups differing only in number of presentations. All five groups otherwise were run under identical conditions. For Group I, the total times for presentation of the syllables was 9.3 sec.; for Group II, 14 sec.; for Group III, 28 sec.; for Group IV, 56 sec.; and for Group IIa, 28 sec.

When *S* entered the experimental room he was seated before the apparatus. The ceiling light was turned off and the window shade drawn, providing near, but not complete, darkness. After dark-adaptation, *S* was informed that this was a study in the learning of nonsense-syllables which would be brief and would require his best efforts at concentration; that he should attend, during the exposure, to the syllables and their order; that the serial list had no fixed beginning or end; that the list would appear over and over, with no break between repetitions; and that the total duration of the presentation would be very brief. The apparatus was then started, the illumination of the aperture being gradually increased as noted above. When the aperture was completely illuminated—after about 5 sec.—*E* began timing with a stopwatch, and after the appropriate time had passed to provide *S* with 16 presentations (32 for IIa *Ss*) the illumination of the aperture was reduced to darkness. It is to be noted that the number of presentations probably varied slightly from *S* to *S* in each group since only the over-all time of presentation was controlled.

A 7.5-w. desk-lamp was turned on; *S* was asked to indicate on paper the syllables he remembered seeing and in the correct order as nearly as possible. He was not told how many syllables were in the list. *S* was then given a list of 21 nonsense-syllables which included the 7 presented and 14 others chosen from Glaze's 93% list, and was asked to check the ones he recognized as having been in the experimental presentation. Guessing was encouraged. *S* was then given 7 cards on which, he was informed, the correct 7 syllbables were printed, and asked to arrange them in the proper order. Lastly, he was given a sheet with 21 pairings of the 7 correct syllables on it, and asked to check the 7 correct pairings. A pair was defined as two consecutive syllables.

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[6] E. R. Hilgard, Methods and procedures in the study of learning, in S. S. Stevens (ed.), *Handbook of Experimental Psychology*, 1951, 517-567.

Results. We first report on the learning of the syllables without regard to order. Performances was expressed in terms of three scores: (a) number syllables correctly recalled immediately at conclusion of the presentation; (b) a weighted recall-score in which one point was given for having only the first letter of a syllable

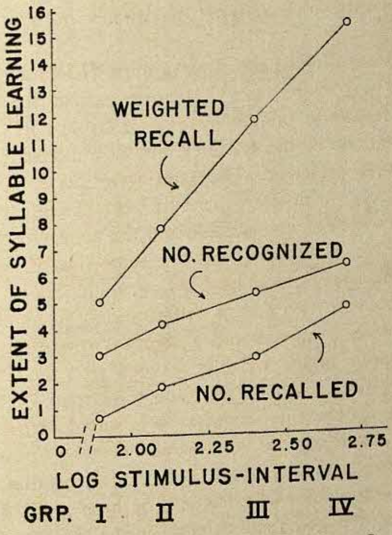

FIG. 1. EXTENT OF LEARNING AS A FUNCTION OF LOG-STIMULUS-INTERVAL (IN M. SEC.)

correct, two points for any two letters correct, and three points for having all three letters correct; and (c) number of syllables correctly recognized in the list of 21 syllables. In Fig. 1, the scores of the four main groups are plotted as a function of log stimulus-interval (in m. sec.). A Kruskal-Wallis one-way analysis showed variance due to rate of presentation to be significant at the 1% level in the case of each score.[7] Groups II and IIa, which had the same rate of presentation but differed in number of presentations, did not differ significantly with respect to any of the three measures.

Three kinds of data were available for evaluating the extent of learning of syllable order: (1) the extent to which the syllables recalled were in the correct order; (2) the extent to which the *Ss* arranged the seven cards with the experimental syllables on them in the correct order; and (3) the extent to which the *Ss* recognized, in the list of 21 syllable-pairings, the correct pairings. For each of these three methods of evaluation, the unit used was number of correct pairs. A correct pair was defined as any two syllables recalled, recognized, or ordered in the same way as they had appeared in the experimental presentation. Data obtained by the second method—arranging the cards—proved not to be feasible for analysis, due to difficulty in evaluating chance-performance. The four main groups (Groups I-IV) did not differ significantly on the test of recognition. They did differ significantly on the test of recall, but the difference was due to recall of individual items, because it disappeared when the pair-recalls of each *S* were corrected for number of syllables recalled. Groups II and IIa did not differ significantly on any of the measures.

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[7] Sidney Siegel, *Nonparametric Statistics for the Behavioral Sciences*, 1956, 1-312.

Discussion. The major contribution of the present study is to extend the available data concerning the effect on learning of rate of stimulus-presentation to much faster rates than have been reported previously. The results indicate clearly that considerable learning occurs when stimuli are presented serially at rates as high as 12 per sec., and, further, that the amount of learning increases with stimulus-interval. The logarithmic relationship suggested by Fig. 1 supports the conclusion of Bergstrom that "The acquisition and retention of a series of familiar associable words varies approximately as the logarithm of the interval at which the words are spoken."[8] The failure of Group IIa to learn more than Group II is surprising, since the former had twice as many stimulus-presentations as the latter. This problem should be studied further.

Comments of the *Ss*, made after the experiment, indicated that the rapid presentation was only partially successful in ruling out rehearsal. Many *Ss* reported trying to evolve some scheme for dealing with the task, such as attempting to count the syllables, to pronounce them, to associate them with some key word, or to learn a few of the syllables first and then add more on successive presentations. It was our impression that the *Ss* with previous experience in learning-studies were most inclined to try such techniques. In general, however, the *Ss* seemed to feel that such techniques were not particularly helpful to the task at hand, due to the rapidity of presentation. It seems a safe conclusion that while the use of deliberate techniques of rehearsal was not eliminated by our procedure, it was greatly attenuated. It is to be noted that the ratio of stimulus-exposure to interstimulus-interval was held constant in this study. It would seem worthwhile in further studies to vary this ratio to determine the relative significance of exposure-duration and interstimulus-interval.

Summary. A technique was developed for investigating the learning of nonsense-syllables under conditions of very rapid presentation of the stimulus-materials. With the ratio between exposure and inter-exposure interval constant, and rate of presentation varying from 2-12 syllables per sec., it was found that the amount learned increases logarithmically as the rate of presentation decreases.

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[8] Bergstrom, *op. cit.,* 221.

'ABSOLUTE' VERSUS 'RELATIONAL' DISCRIMINATION OF INTERMEDIATE SIZE IN THE RHESUS MONKEY

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Transposition of responses to stimulus-objects of intermediate size has been considered a critical test by proponents of both S–R and perceptual theories of discriminative learning. Spence failed to find evidence of relational learning in chimpanzees when 'absolute' and 'relational' tendencies were directly opposed in the test for transposition.[1] Gonzales, Gentry, and Bitterman so modified the experimental procedure that no single choice was determined by 'absolute' stimulus-values, and significant 'relational' learning was evidenced by the chimpanzees in their experiment.[2] Recently, Gentry, Overall, and Brown confirmed the finding of relational learning of the problem of intermediate size in an experiment involving rhesus monkeys.[3]

The present investigation compares the performance of a single group of rhesus monkeys on two transpositional problems in an effort to rationalize the apparently conflicting evidence. Each *S* in the present study was first trained and tested on a transpositional problem of Gonzales, Gentry, and Bitterman's design. Subsequently, each *S* was retrained and tested on an intermediate-size transposition problem of Spence's design. An increase in understanding of the discriminational process is sought through comparison of results obtained from the two procedures.

Method: Subjects. Nine adult, male, rhesus monkeys, approximately 4 yr. of age, were selected as the *S*s. Each had previous experience on a variety of discriminational tasks. The monkeys were caged separately in air-conditioned quarters and fed at the conclusion of each test-day.

Apparatus. A string-pulling apparatus, previously described in detail, was employed throughout the experiment.[4] The stimulus-objects were nine cubical, wooden boxes, each of which was open at the top and painted black inside and out. The boxes were designated in order of size with the numbers 1 to 9. Each face of the smallest box was 9 sq. in. in area, and the surface area of each successive box in the series increased by a factor of 1.15. On every trial, 3 of the boxes were presented, 8 in. apart, at a distance of 30 in. from *S*.

Procedure: Experiment I. In the first experiment each *S* was first trained, to a criterion of 16 correct out of 18 trials, to choose the intermediate size when presented with Boxes 3, 5, and 7, with the box (Box 5), intermediate in size being

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* Received for publication September 11, 1958.

[1] K. W. Spence, The basis of solution by chimpanzees of the intermediate size problem, *J. exp. Psychol.*, 31, 1942, 257-271.

[2] R. C. Gonzales, G. V. Gentry, and M. E. Bitterman, Relational discrimination of intermediate size in the chimpanzee, *J. comp. physiol. Psychol.*, 47, 1954, 385-388.

[3] G. V. Gentry, J. E. Overall, and W. Lynn Brown, Transpositional responses of rhesus monkeys to stimulus-objects of intermediate size, this JOURNAL, 72, 1959, 453-455.

[4] *Ibid.*, 453-455.

'positive', *i.e.* rewarded. The correction-method was employed on all training trials. In the test for transposition, two new sets of boxes—Boxes 2, 4, 6 and 4, 6, 8—were employed. During this phase of the experiment, the *S*s continued to receive 18 trials per day—12 training trials with 6 test-trials interspersed. The training trials continued with Boxes 3,5, and 7, with Box 5, intermediate in size, being again rewarded. Three test-trials each day were made with Boxes 2, 4, 6 and three with Boxes 4, 6, 8; and on these trials any choice was rewarded. In four days of testing, each set was presented 12 times, twice in each of the six possible spatial arrangements.

TABLE I

NUMBER OF TIMES THE VARIOUS BOXES WERE CHOSEN DURING THE TEST-TRIALS (Experiment I)

S	Boxes				No. times intermediate box	
	2	4	6	8	chosen	not chosen
1	0	9 2	3 10	0	19	5
2	0	3 1	9 11	0	14	10
3	0	0 1	12 11	0	11	13
4	2	3 2	7 10	0	13	11
5	0	3 0	9 12	0	15	9
6	1	3 2	8 10	0	13	11
7	0	5 5	7 7	0	12	12
8	0	2 1	10 10	1	12	12
9	0	6 2	6 10	0	16	8

Experiment II. In the second experiment, every *S* was retrained to a criterion of 16 correct out of 18 trials on Boxes 3, 5, 7—a criterion which was quickly reached. The correction-method was again employed. In this problem the test for transposition was conducted exactly as in the first problem, 6 tests out of 18 trials per day for four days, except that the test-sets now included Boxes 1, 3, 5, and 5, 7, 9. Thus, each test-set included one box (Box 3) previously rewarded and one (Box 7) not previously rewarded—the latter intermediate in size within the second test-set, *i.e.*, Boxes 5, 7, 9. On test-trials any choice was rewarded, as in the first experiment.

Results: Experiment I. When the test for transposition paired a 'relational' choice with chance preference based upon 'absolute' values, the monkey demonstrated significant choice of the box of intermediate size in the test-sets. Table I presents the results of the problem for each of the nine *S*s. A simple S–R associational theory predicts equal preference for Boxes 4 and 6 in each test-set; thus, response on the basis of 'absolute value' should result in choice of the intermediate box about 50% of the time. Choice of this box was found to exceed chance at the 5% level of significance. At the same time, the *Ss* indicated, in the test-trials, a significant preference ($p < 0.01$) for Box 6—the box with 'absolute value.'

Experiment II. The results of the second transpositional experiment are presented for each of the nine *Ss* in Table II. When the test for transposition paired a box intermediate in size with the 'absolute' box (Box 5)—a choice rewarded during training—the *Ss* consistently chose Box 5. Whereas, in Experiment I, the *Ss* had indicated a significant 'relational' tendency, these same *Ss* reverted to a highly consistent choice of the 'absolute' stimulus-value in Problem II.

TABLE II

NUMBER OF TIMES THE VARIOUS BOXES WERE CHOSEN DURING THE TEST-TRIALS (Experiment II)

S	Boxes					No times intermediate box	
	1	3	5	7	9	chosen	not chosen
1	0	1	11				
			12	0	0	1	23
2	0	0	12				
			12	0	0	0	24
3	0	0	12				
			10	1	1	1	22
4	0	0	12				
			12	0	0	0	24
5	0	0	12				
			12	0	0	0	24
6	0	0	12				
			12	0	0	0	24
7	0	1	11				
			12	0	0	1	23
8	0	0	12				
			12	0	0	0	24
9	0	1	11				
			12	0	0	1	23

A non-parametric sign-test for paired observations was employed and the number of choices of the box intermediate in size was found to differ at the 1% level of significance for the two tests of transposition.

Discussion. It is apparent that rhesus monkeys learn both 'absolute' and 'relational' properties of the stimulus-objects during discriminative training; thus neither an 'absolute' nor a 'relational' theory of discriminative learning accounts for all of the facts. The 'relational' theories predict that objects intermediate in size would be chosen by the *Ss* in each of the present experiments. Although significant evidence of relational learning was obtained in Experiment I, no evidence of it was obtained in Experiment II. On the other hand, whereas theories of the 'absolute' type predict correctly the choice of the stimulus-object in Experiment II, they do not predict above chance, the choice of the stimulus-object in Experiment I.

In the present study, a slight difference in the degree of stimulus-transposition on test-trials resulted in extreme differences in number of choices of the box intermediate in size. When translation along the size-continuum was calculated to minimize preferences between test-objects based upon 'absolute' size, significant evidence of 'relational' learning was found. When the transposition resulted in a definite preference based upon 'absolute' size, no evidence of relational learning was observed. The results of the present experiment suggest that 'lower' primates learn both 'relationships' and 'absolute' stimulus-values; however, the tendency to respond on the basis of 'absolute' values is the stronger tendency and 'relational' properties are employed as a basis for our *Ss*' responses only when differential 'absolute' values are lacking.

The degree of relational learning manifested by the *Ss* in this experiment, although statistically significant, was not as great as that evidenced by chimpanzees or by the group of rhesus monkeys used in the earlier study from this laboratory referred to above. A possible explanation for differences between the two groups of monkeys tested at this laboratory involves differences in experience and of age; the monkeys of the present study being younger and less sophisticated than those of the earlier one.

In view of the relatively weak 'relational' tendencies exhibited in the present study, it is interesting to note that transpositional testing in Experiment I revealed significant 'absolute' response-tendencies as well. Although an effort was made to equate the size-differences between test-stimuli (Boxes 4 and 6) and the positive training stimulus-object (Box 5), the *Ss* developed a significant preference for Box 6. Since a preference for one specific stimulus-object, disregarding relative properties, cannot be predicted from a 'relational' theory, it must be concluded that the *Ss* demonstrated associations for 'absolute' stimulus-properties in the first experiment as well as in the second.

The test for transposition is employed as a *test* of what the *S* has learned during discriminative training. The present study indicates that monkeys actually learn *both* 'absolute' and 'relational' properties and that they attend to both within the *same* situation. Transpositional testing on Experiment I indicated significant attention to 'relational' properties; moreover, the testing procedure reinforced the relational choice. The *same Ss*, after more training on the *same* problem for which relational learning had been demonstrated, evidenced strong associations for 'absolute' stimulus-properties when the testing-situation was changed in Experiment II to allow these associations to become manifest.

SUMMARY

Nine rhesus monkeys were successively trained and tested on two intermediate-size transpositional problems. In Experiment I, responses based upon 'relative' stimulus-properties were opposed to *chance* behavior determined by 'absolute' stimulus-properties, and the *Ss* indicated significant learning of stimulus-relationships. Experiment II was designed in such a way that different non-chance behaviors should result from response to 'absolute' and 'relational' stimulus-properties, and the *Ss* consistently reverted to choices based upon 'absolute' stimulus-properties. It is concluded that rhesus monkeys learn both 'absolute' and 'relational' cues during training in a single stimulus-situation and that there is a strong tendency to respond to 'absolute' stimulus-values whenever feasible.

ON THE NEGATIVE RECENCY HYPOTHESIS IN THE PREDICTION OF A SERIES OF BINARY SYMBOLS

By JULIAN FELDMAN, Carnegie Institute of Technology

The statistical learning-models of Estes and of Bush and Mosteller have been used to explain the behavior of human subjects (*S*s) in predicting a series of binary symbols.[1] The basic postulate of these models is that of positive recency—the occurrence of one symbol on Trial T + 1 will increase the probability of *Ss'* predicting that symbol on Trial T1. Several investigations have presented evidence that appears, however, to be inconsistent with the statistical learning-models. Jarvik has demonstrated the existence of a negative effect of recency—the occurrence of a run of like symbols on successive trials decreases the probability of *S*s' predicting that symbol.[2] Engler has shown that while the statistical learning-models are consistent with the probability of marginal responses of human *S*s, the models are not consistent with the probability of conditional responses of these *S*s.[3] Goodnow and her associates have reconciled these two criticisms of the statistical learning-models in their effort to explain behavior in this type of experiment.[4] They believe that *S*s predicting a series of binary symbols respond to a stimulus that is essentially the length of run of like symbols. They suggest that the agreement between the marginal and conditional probabilities in the stimulus- and response-series is a function of the *S*s' responses to homogeneous runs of various lengths. The purpose of this study is to present additional evidence that appears to support the Goodnow hypothesis and to suggest a broader model of behavior for experiments in binary choice.

Method. The new data presented here were obtained in an experiment similar to that of Jarvik.[5] The features of the current experiment that differ from Jarvik's are as follows: (1) The stimulus-series was not randomized within blocks. The series contained 200 symbols. For half of the *S*s, 142 symbols were 'check' and 58 were 'plus.' The other half of the *S*s heard the same series with the symbols reversed. (2) The *S*s did not keep written records. Their responses, signalled with telegraph keys, were automatically recorded. (3) Forty-three women, students at

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[1] W. K. Estes and J. H. Straughan, Analysis of a verbal conditioning situation in terms of statistical learning theory, *J. exp. Psychol.*, 47, 1954, 225-234; R. R. Bush, and Frederick Mosteller, *Stochastic Models for Learning*, 1955, 274-309.

[2] M. E. Jarvik, Probability learning and a negative recency effect in the serial anticipation of alternative symbols, *J. exp. Psychol.*, 41, 1951, 291-297.

[3] Jean Engler, Marginal and conditional stimulus and response probabilities in verbal conditioning, *ibid.*, 55, 1958, 303-317.

[4] Jacqueline Goodnow, Irvin Rubinstein, and Ardie Lubin, Response to changing patterns of events, Memo 4, Problem-solving projects, Walter Reed Army Institute of Research, 1957; and Jacqueline Goodnow, Betty Shanks, Irvin Rubenstein, and Ardie Lubin, What is the human subject responding to in a two-choice task?, Memo 3, Problem-solving project, Walter Reed Army Institute of Research, 1957.

[5] Jarvik, Probability learning and a negative recency effect in the serial anticipation of alternative symbols, *J. exp. Psych.*, 41, 1951, 291-297.

Margaret Morrison College, were used as *Ss*. They served under four conditions which differed in the amount of information and reward received by them. A comparison of the four conditions did not reveal any regular differences, and the data are pooled for the present analysis.

Results. A comparison of the responses of Jarvik's and our *Ss*, during runs of three or more symbols, is given in Table I. Jarvik's *Ss* show a tendency to predict (a) the least frequent symbol after two occurrences of the most frequent symbol; and (b) the most frequent symbol following an occurrence of the least frequent symbol. Our *Ss* do not show these tendencies. For the most part, they predicted longer runs of both symbols and did not agree, as much as Jarvik's *Ss* did, on the expected length of runs.

TABLE I

Percentage of *Ss* Predicting the Most Frequent Event during Runs of Three or More Homogeneous Symbols

(1=more frequent symbol; 0=less frequent symbol.)

Study	Sequence of symbols preceding prediction							
	0	0 1	0 1 1	0 1 1 1	1	1 0	1 0 0	1 0 0 0
Jarvik's	68	75	67	55	56	52	62	65
This	44	69	79	83	76	53	41	42

TABLE II

Frequency-Distribution of Runs by Length

Study	Length of run										
	more frequent event										less frequent event
	2	3	4	5	6	7	9	10	12	17	3 or more
Jarvik's	23	13	11	2	1	0	0	0	0	0	2
This	8	3	4	3	1	2	1	1	1	1	4

The *Ss* in both experiments appeared to be responding to the run-structure of the symbol-series (see Table II). Jarvik's series of symbols were randomized within short blocks. This procedure yielded a large number of short runs. In the present experiment, the symbol-series was randomized over the whole series, and thus longer runs occurred more frequently.

Discussion. The data presented here appear to corroborate Goodnow's hypothesis. We believe, however, that a complete model of binary choice-behavior must contain more than an hypothesis of the negative effect of recency. This conclusion is suggested by a closer examination of the results of the present study for responses to runs of seven in length where $P(\mathit{1}|0\ 1)$ and $P(\mathit{1}|0\ 1\ 1)$ are both less than the $P(\mathit{1}|0)$.[6] This is the only case that deviates substantially from the over-all prediction for lengths of run. One of the runs of seven was composed of the first symbol shown in the series. The *Ss* expected change in the early part of the series. The second run of seven followed a series of five symbols that comprise a simple alternation (0 1 0 1 0). The *Ss* expected the alternating pattern to continue. This explanation accounts for the low value for $P(1/0\ 1)$. The low value for $P(1/0\ 1\ 1)$ could also result from the structure of the series of symbols. The alternation of 5 was preceded by another alternation of 5 and an occurrence of the most frequent symbol in that order, *i.e.* the 11 symbols preceding the run of 7 were 1 0 1 0 1 1 0 1 0 1. This analysis suggests that (1) *Ss* participating in binary guessing experiments entertain hypotheses regarding patterns in addition to the negative recency hypothesis.

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[6] The italicized symbols indicate the prediction; the symbols following the vertical bar indicate the preceding events in the symbol series. Hence $P(\mathit{1}|0\ 1)$ means the percentage of *Ss* predicting Symbol 1 will occur on Trial T after a 0 has occurred on T-2 and a 1 on T-1.

EXPECTANCY FOR SUCCESS AND CERTAINTY OF RESPONSE

By ERWIN J. LOTSOF, University of California, Los Angeles

This study is concerned with the relationship between expectancy of success (as defined in social learning-theory) and certainty of response.[1] Since expectancy is an important theoretical variable in recent formulations of personality and human learning, its measurement is a pressing methodological problem. Social learning-theory utilizes the basic concepts of *expectancy* and *reinforcement-value* in predicting the potential occurrence of a given behavior.[2] The basic formula for behavior is given as:

$$BP_x,\ S_1,\ R_a = f(E_x,\ R_a,\ S_1\ \&\ RV_a)$$

This formula may be read as follows. The potential for Behavior x (BP_x) to occur in Situation 1 (S_1) in relation to reinforcement a (R_a) is a function of the expectancy (E_x) of the occurrence of reinforcement a (R_a) following behavior x in Situation 1 (S_1) and the value of reinforcement a (RV_a) in Situation 1. The construct-expectancy is treated as a quantifiable variable, and reinforcement-value implies only an empirical law of effect. Expectancy is a function of the past history of reinforcements.

In a previous study, Lotsof investigated the relationship of decision-time and expectancy for success.[3] In the preceding study the measure of response was *S*'s success in pressing one of two buttons. For some kinds of studies, it was felt that decision-time would be difficult to obtain, thus an attempt was made to arrive at a verbal response which would have value and be easily obtained. If a verbal response could be shown to vary with the reinforcement-ratio, then the verbal response could be employed as another operation for expectancy.

Method: Subjects. The *Ss* were 90 students, men and women randomly selected from the introductory course in psychology. They were divided into 9 groups of 10 *Ss* each. Each *S* was tested individually.

Apparatus. *S* sat in front of a panel with a white jewel light on either side; below each there was a red light. Beneath each red light was a push-button. A dummy push-button was located equidistant between the real ones. *E*'s panel was a replica of *S*'s board. *S*'s push-button activated the white lights and *E*'s the red lights on both boards. Relays were used to keep the lights on and a Hunter timer to turn the lights off after ½ sec.

Procedure. Every *S* was randomly assigned to one of the nine groups. The amount of positive reinforcement for each group was as follows: 16, 25, 33, 41, 50, 58, 66, 75, and 83%. Each trial was positively (correct) or negatively (wrong) reinforced by *E* in a predetermined fashion. Within the 48 trials each block of 12 trials contained the same number of reinforcements. Certainty of response, a verbal state-

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[1] J. B. Rotter, *Social Learning and Clinical Psychology*, 1954, 165.

[2] *Ibid.*, 105-111.

[3] E. J. Lotsof, Expectancy for success and decision-time, this JOURNAL, 71, 1958, *16*, 416-419.

ment, was rated by *S*'s calling out 1, 2, 3, or 4, with 1 being quite certain and 4 not sure at all. *S* was instructed to press the button to indicate his expectancy for a red light and to call out their degree of certainty. A trial consisted in *S*'s pressing one of the two buttons, then stating how certain he was of his statement, after which *E* informed *S* via the lights whether *S* guessed correctly. The lights went off and another trial started.

Instructions. *S*'s instructions were as follows.

> On this panel there are two red lights, two white lights, and three buttons. Pushing one of the side buttons will turn on the white light immediately above it. In this experiment the red lights will come on in a certain sequence. Your task is to decide which red light is going to appear next. You will indicate your decision by pressing the button on the appropriate side, which will turn on the white light. A red light will then come on. If the red light is on the same side as the white light, your choice was correct. If the red light is on the other side, your choice was wrong. Both lights will then go out. When this happens you may go ahead with your next choice. We are interested in seeing how accurately you can predict which light is going to appear next. Place your finger on the center button, move it from there to press the side button you have chosen, then move it back till you make your next choice. After you have chosen, then move it back till you make your next choice. After you have made your choice you are to call out 1, 2, 3, or 4 indicating how sure you are of your choice. [1 being 'complete' certainty; 4 'complete' uncertainty].

TABLE I

VARIANCES OF CERTAINTY OF RESPONSE AS A FUNCTION OF REINFORCEMENT

Source	*df.*	MS	*F*	P
(No. reinforcements)	8	4643	10.39	<0.001
Within-Ss	81	447		

Preliminary training consisted in giving each *S* four trials, in order to acquaint him with the apparatus.

Results and discussion. The first 12 trials for each group were used for training. The analysis is based on 36 trials. Each trial could be scored from 1 to 4, giving a maximal score of 144 'complete' uncertainty to a minimal score of 36 'complete' certainty. The data were treated, using the method of analysis of variance. The results are shown in Table I.

The *F*-test, significant beyond the 1/10% level, indicated that the nine groups have different verbal responses as a function of the respective treatments employed in this study. With the *F* significant, Fisher's *t* was used to compare each treatment with every other treatment.

Twenty-two of the 36 *t*-tests were significant—15 beyond the 1% level and 7 at the 5% level. A rank-order correlation was performed to assess the relationship between the reinforcement-ratio and the mean certainty of response. The correlation obtained was 0.93 which is significant beyond the 1% level.

In this study the potentiality of a behavior occurring varied directly with the probability of occurrence (*E*) and the reinforcement-value was controlled. Expectancy was manipulated by setting up different frequencies of reinforcement. The resulting differences among groups regarding certainty of response appear to be related to different probabilities of reinforcement in expectancy. It would appear that the verbal response of certainty may be employed as a measure of expectancy for success.

TABLE II

CERTAINTY OF RESPONSE AND AMOUNT OF REINFORCEMENT
(10 *S*s at each of 9 levels of reinforcement)

Reinforcement	Mean score	*SD*	Reinforcement	Mean score	*SD*
16.6%	123	20.6	58.3%	100	19.3
25.0%	111	20.7	66.6%	91	22.7
33.3%	125	20.2	75.0%	64	17.7
41.6%	104	13.6	83.3%	64	12.3
50.0%	92	23.5			

SUMMARY

Nine groups of 10 *S*s each, were individually tested. Every *S* was asked to guess which of two lights would appear on a panel and to rate his degree of certainty on a 4-point scale that light he designated would appear. Choice was signified by *S*'s pressing a button. Treatment-conditions ranged from 16% to 83% correct. Analysis of the data revealed (1) significant differences in certainty of response among the groups and (2) a strong association between frequency of reinforcement and certainty of response. The results are in keeping with the hypothesis that the higher the expectancy for success the more certain the verbal response; consequently, certainty can be employed as another operation for expectancy as conceptualized by social learning theory.

ADAPTATION TO DISARRANGED EYE-HAND COÖRDINATION IN THE DISTANCE-DIMENSION

By RICHARD HELD and MELVIN SCHLANK, Brandeis University

Although Helmholtz was the first to study adaptation to a sensory-motor disarrangement—reduction of the errors of hand-eye coördination induced by a prism before the eye[1]—Stratton's study of inverted vision is generally considered the classic in this field.[2] Recently Kohler reported the most extensive studies yet performed, revealing new and unexpected kinds of adaptation.[3] Most investigators have concentrated on the development of apparatus to produce the disarrangement and on the measurement of the consequences of exposure to novel conditions, but few have attempted to vary systematically the conditions of exposure to determine which are critical for adaptation. This study is one of a series designed to isolate the critical conditions.[4]

Holst used the terms *re-afference* and *ex-afference* to distinguish between sensory stimulation resulting from self-produced movement and sensory stimulation independent of such movement.[5] The results of two experiments on prolonged exposure to disarranged visual-motor coördination (prisms before the eyes) are consistent with the hypothesis that re-afferent stimulation is necessary for adaptation.[6] In both experiments, re-afferent stimulation produced significant shifts of localization in the adaptive direction, while ex-afferent stimulation did not. The technique of these experiments consisted essentially of equating all conditions of exposure other than the agent producing the bodily movement. Re-afferent visual stimulation was obtained in one experiment when *S* walked a path while viewing his environment and in the other experiment when *S* moved his arm before his eyes. Comparable ex-afferent stimulation was obtained when *E* either moved *S* while seated in a wheel chair over the path formerly walked or moved *S*'s arm. Since the changes in optical stimulation under both conditions were very similar, differences in amount of adaptation were attributed to the difference in the agents producing movement.

Method. Fig. 1 shows S in the apparatus under the two conditions. In Fig. 1a, he marks the apparent location of a target to provide a measure of his eye-hand

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* Received for publication December 22, 1958. This work was supported by a grant from the National Science Foundation.

[1] H. L. F. von Helmholtz, *Handbuch der physiologischen Optik*, 3, 1867, 601-602.

[2] G. M. Stratton, Vision without inversion of the retinal image, *Psychol. Rev.*, 4, 1897, 341-360, 463-481.

[3] Ivo Kohler, Ueber Aufbau und Wandlungen der Wahrnehmungenswelt; insbesondere überbedingte Empfindungen, *Sitzungsber. Oest. Akdd. Wiss.*, 227, 1951, 1-118.

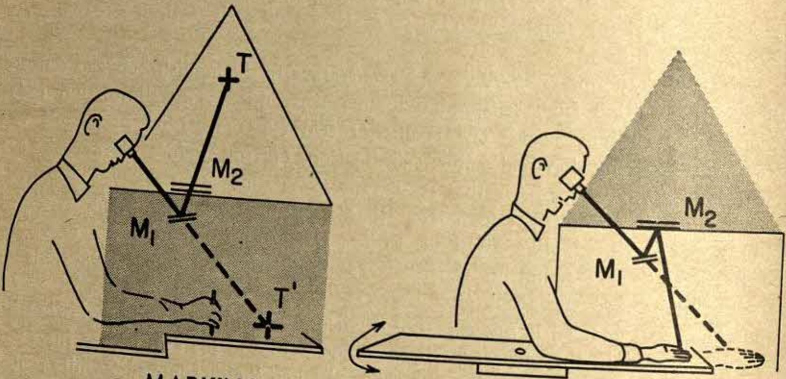
[4] Richard Held, Shifts in binaural localization after prolonged exposures to atypical combinations of stimuli, this JOURNAL, 68, 1955, 526-548; Joseph Bossom and Richard Held, Shifts in egocentric localization following prolonged displacement of the retinal image, *Amer. Psychologist*, 12, 1957, 454 (Abstract); Richard Held and A. V. Hein, Adaptation of disarranged hand-eye coördination contingent upon re-afferent stimulation, *Percept. mot. Skills*, 8, 1958, 87-90.

[5] E. von Holst, Relations between the central nervous system and the peripheral organs, *Brit. J. anim. Behav.*, 2, 1954, 89-94.

[6] Bossom and Held, *op. cit.*, 454; Held and Hein, *op. cit.*, 87-90.

coördination. In Fig. 1b, he views a virtual image of his hand at a distance optically greater than normal. The apparatus consisted of an upper and lower lightbox. The upper lightbox contained the target, T—three black spots on a white field forming an equilateral triangle 3 in. on a side. *S*'s hand was in the lower lightbox. A half-silvered mirror, M_2, divided the two lightboxes. Whether *S* viewed T or his hand depended on whether the upper or the lower lightbox was illuminated. The position of a double-throw switch determined which one of the lightboxes was illuminated. A biting-board held *S*'s head in a fixed position throughout each session.

During marking (Fig. 1a), when the upper lightbox was illuminated and the lower lightbox darkened, light from T passed through M_2 and was reflected into *S*'s eyes from M_1, a fully-reflecting first-surface mirror. *S* saw the virtual image of T at location T′ which coincided with the marking surface. *He did not see his hand or the marking surface* because his vision was restricted by apertures to the field reflected in M_1. *S* was instructed to mark successively the apparent location of the three target-points nine times, for a total of 27 marks.



a. MARKING

b. EXPOSURE

FIG. 1. SCHEMATIC REPRESENTATION OF *S* IN THE APPATATUS UNDER TWO EXPERIMENTAL CONDITIONS

During exposure (Fig. 1b), the upper lightbox was blacked-out while the lower lightbox was illuminated. Under these conditions, the half-silvered mirror, M_2, reflected light from the hand to M_1 and then to *S*'s eyes. Looking into M_1, *S* saw his hand at the increased optical distance. The increase in distance averaged 3 in. at the level of the marking surface. *S*'s hand was strapped to the swivel shown in Fig. 1b. Two conditions of exposure were used. The movement of *S*'s arm was caused either by his own efforts (*self-produced movement*) or by *E*'s movement of the swivel (*passive movement*).

Experiments with each of 15 *S*s were conducted in two sessions; one with self-produced movement and the other with passive movement. Eight *S*s had the self-produced movement first, while the other seven began with passive movement. The two sessions were separated by at least one day.

The schedule of both sessions was as follows. (1) Pre-exposure marking (Fig 1a): With his unseen hand, *S* marked the apparent locations of the three points of the target. This set of marks gave a measure of *S*'s eye-hand coördination before exposure. (2) Exposure (Fig. 1b): *S* viewed the mirrored image of his hand on the swivel at the optically-increased distance as it moved back and forth in a small a.c. (3) Post-exposure marking (Fig. 1a): *S* was instructed to mark the apparent locations of the target-points as he had done in the pre-exposure marking. The difference in positions of the pre- and post-exposure markings measured the influence of the intervening exposure.

In the exposure with self-produced movement, *S* moved his arm and the supporting swivel to which his arm was strapped through an arc of approximately 12°. In the exposure with passive movement, *E* moved the swivel on which *S*'s relaxed arm was secured by straps through the same arc. To insure passive movement during exposure, *S* was given practice before the session in relaxing his arm as *E* moved it. Each of the two exposures lasted 6 min.; and during both of them, *S*'s hand, viewed through the mirrors, remained visible moving back and forth through the 12° arc in rhythm to a metronome set at 72 beats per min.

Views of the hand and its motion were identical in both the self-produced and the passive movement exposures. The only difference between these conditions was the type of movement—self-produced as opposed to passive movement. The re-afference hypothesis predicts that adaptation to the optically-increased distance of the hand should occur only during self-produced movement. A shift of the post-exposure markings toward *S* was adaptive. He would initially have over-reached objects that he viewed through the mirrors had he attempted to localize them.

Results. The mean shift following self-produced movement was 0.52 in. in the adaptive direction, while for passive movement the mean shift was 0.05 in. in the adaptive direction. The difference between the two mean shifts was significant at a level of confidence well beyond 1% (*t*-test for correlated scores). With self-produced movement, 14 of the 15 *Ss* showed adaptive shifts; for passive movement, only about half of the *Ss* showed adaptive shifts. The mean shift of 0.05 in. for passive movement was not significantly different from zero.

The results of this experiment support the proposition that re-afferent visual stimulation is a necessary condition for learning the new eye-hand coordination in the distance dimension. Comparable but ex-afferent stimulation resulting from passive movement did not produce significant shifts, although the optical information for *S* in both exposure conditions was the same. These results, or change in eye-hand coördination in the distance-dimension, can be added to the results of the studies previously mentioned which demonstrate the importance of re-afference in adaptation to disarrangement with other forms of sensory-motor coördination.

SOME FACTORS AFFECTING AUDITORY DETECTION OF AMPLITUDE-MODULATION

By CARL E. SHERRICK, JR., Washington University

A number of recent investigations have related the value of the intensive difference-limen (*DL*) for pure tones to a variety of conditions.[1] All of these have employed techniques which effectively reproduce the stimulus first developed by Riesz for studying this *DL*.[2] The stimulus is a pure audio-frequency tone, sinusoidally amplitude-modulated at the rate of 3 ~. In the course of his original study, Riesz examined the effect of rate of modulation on the value of the *DL*. He obtained a curve of the *DL*, when the rate of modulation was varied that was concave upward with a broad minimum at about 3.2 ~. This result has been reproduced by at least two later investigators.[3]

If the modulation-envelope of the stimulus is examined, it may be seen that a variation in rate of modulation produces a change in two other important stimulus-dimensions, viz. the time of the rise of the increment and decrement, and the duration of the higher values of intensity. It is the purpose of this paper to examine the effects on the value of the *DL* of the manipulation of rate of modulation and duration of the intensity-increment, keeping the time of the incremental rise and fall constant.

Method and procedure. The apparatus was a modified form of the audiometric apparatus described by Hirsh and Bilger.[4] A part of the output of a General Radio Model 1304-A oscillator was sent through attenuators to a mixing circuit. A second part of the output of the oscillator was sent through the modified Békésy audiometer, and from there to the mixer. The mixed signals went to *O*'s headphone. *O* heard a steady tone which periodically increased and decreased in loudness. The increment of loudness was varied in duration and rate by controlling the electronic switch action with a Tektronix Model 162 pulse generator. The time of the rise and fall of the increment was set at 40 m. sec.

Five normal trained *O*s participated in the study. All listening was done individually, monaurally, in a quiet room, and with a calibrated headphone. *O* controlled the attenuator by adjusting the level of the signal until modulation just disappeared or just reappeared. The adjustments were recorded on a calibrated graphic tape, and the *DL* was taken as equal to 20 $\log_{10} [(V + \Delta V)/V]$, where $V =$ voltage across headphone coil. The stimulus was a 1000 ~ pure tone at an intensity of 40 db. above *O*'s threshold. Modulation rates of 1, 2, 3, 4, and 5 ~, and increment durations of 40 to 800 m. sec. were presented in combinations such that the duty cycle of the increment was systematically varied over values of 20, 40, 60, and 80% — duty cycle = [(duration of increment)/(period of modulation)] × 100. Every *O* made 20 judgments on each condition at three daily sessions.

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* Received for publication December 24, 1958. This research was carried out under Contract No. AF 33 (616)-3505, between the Central Institute for the Deaf and the United States Air Force, monitored by the Aero-Medical Laboratory.

[1] F. A. Brogan, A simplified technique for measuring the auditory intensity difference-limen, *U.S.A.F. School of Aviation Medicine Report No. 9*, Nov. 1952; J. E. Jerger, A difference-limen recruitment-test and its diagnostic significance, *Laryngoscope*, 62, 1952, 1316-1332; J. Tonndorf, F. A. Brogan, and D. D. Washburn, Auditory difference-limen of intensity in normal-hearing subjects. *U.S.A.F. School of Aviation Medicine Report No. 55-31*, Aug. 1955; E. Zwicker and W. Kaiser, Der Verlauf der Modulationsschwelle in der Hörfläche, *Akustische Beihefte*, 4, 1952, 239-246.

[2] R. R. Riesz, Differential intensity sensitivity of the ear for pure tones, *Phys. Rev.*, 31, 1928, 867-875.

[3] Tonndorf, Brogan, and Washburn, *op. cit.*, 6; Zwicker and Kaiser, *op. cit.*, 243.

[4] I. J. Hirsh and R. C. Bilger, Masking of tones by bands of noise, *J. acoust. Soc. Amer.*, 28, 1956, 623-630.

Results. The results are shown in Fig. 1, where the *DL* in db. is plotted against the duty cycle of the increment. The parameter for the family of curves is modulation rate. Three results should be noted from these curves: first, the rate commonly-used (3 ~) does not yield the minimal *DL* under these conditions; secondly, each curve, with the exception of the 5 ~ rate, appears to pass through a minimum at values of the duty cycle between 40 and 60%; thirdly, under the conditions of the present experiment, it would appear that even slower rates of modulation may yield small *DL*s, *i.e.* no minimal *DL*, as a function of rate, appears on the graph.

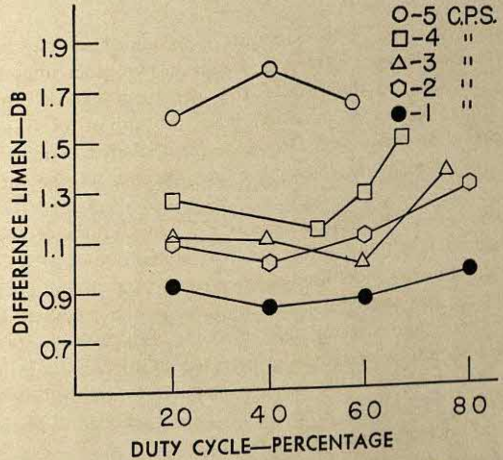

FIG. 1. VARIATION OF THE INTENSIVE *DL* WITH RATE AND DURATION OF THE INTENSITY INCREMENT

The significance of these results cannot be fully assessed until the dimension of incremental rise- and fall-time is studied, but a practical issue is raised. Some audiometers of recent manufacture (*e.g. Maico, Beltone,* and *Peters*) are equipped with modulating devices that may be used to determine the *DL* of the hard-of-hearing patient. These devices typically produce fixed modulation-rates of 3 ~ and modulation-envelopes of a trapezoidal form, following the technique of

Lüscher and Zwislocki.[5] Until more is known about the interaction of the dimensions described above, especially as they may relate to changes in the *DL* as a function of hearing loss, caution should be exercised in the interpretation of results obtained from such apparatus.

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[5] E. Lüscher and J. Zwislocki, A simple method for indirect monaural determination of the recruitment phenomenon (difference limen in intensity for different types of deafness), *Acta Otologica,* Suppl., 78, 1948, 156-158.

SATIATION-THEORY AND THE MÜLLER-LYER ILLUSION

By George Moed, Children's Seashore House, Atlantic City, New Jersey

That the magnitude of the Müller-Lyer illusion may be reduced, even reversed, in the course of repeated exposure has been demonstrated in several experiments.[1] The purpose of the present study is to evaluate the explanation of this phenomenon in terms of the satiation-theory of Köhler and Wallach.[2] The theory suggests that asymmetrical satiation is a necessary condition for reduction of the illusion. It also suggests a relation between the duration and number of exposures; a large number of brief exposures should be as effective as a smaller number of longer exposures. Both deductions were tested in this study.

TABLE I

Experimental Design

	Part 1			Part 2		
Condition	A_1	B_1	C_1	A_2	B_2	C_2
No. of trials	25	25	25	50	50	50
Total time (min.)	12.5	12.5	12.5	12.5	12.5	12.5
Exposure-time (sec.)						
First orientation	60	45	30	30	22.5	15
Second orientation	0	15	30	0	7.5	15

Method. The plan of the study is outlined in Table I. The symmetry-asymmetry hypothesis was tested in three different treatments. One treatment (C), designed to achieve symmetrical satiation, consisted of a 180° reversal of the Müller-Lyer figure from trial to trial, with equal exposure-time in each orientation. A second treatment (*A*), designed to effect asymmetrical satiation, consisted of presenting the figure always in the same orientation. A third treatment (*B*), also designed to create asymmetrical satiation, consisted of trial-to-trial reversal of the figure with exposures three times as long in one orientation as in the other. The purpose of this treatment was at once to eliminate the factor of reversal of the stimuli as a possible cause of changes in the illusion and to study a condition intermediate between the

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* Received for publication January 20, 1959. I am indebted to Dr. William A. Shaw and to Dr. James C. Diggory for assistance in the preparation of this manuscript.

[1] C. H. Judd, Practice and its effects on the perception of illusions, *Psychol. Rev.*, 9, 1902, 27-39; The Müller-Lyer illusion, *Psychol. Rev. Mon. Suppl.*, 7, 1905, 55-61; E. O. Lewis, The effect of practice on the perception of the Müller-Lyer illusion, *Brit. J. Psychol.*, 2, 1908, 294-306; C. E. Seashore, E. A. Carter, E. C. Farnum, and R. W. Seis, The effect of practice on normal illusions, *Psychol. Rev. Mon. Suppl.*, 9, 1908, 103-148.

[2] Wolfgang Köhler and Hans Wallach, Figural after-effects: An investigation of visual processes, *Proc. Amer. Philos. Soc.*, 88, 1944, 269-357; Wolfgang Köhler and Julia Fishback, The destruction of the Müller-Lyer illusion in repeated trials: I. An examination of two theories, *J. exp. Psychol.*, 40, 1950, 267-281; Köhler and Fishback, The destruction of the Müller-Lyer illusion in repeated trials: II. Satiation patterns and memory traces, *J. exp. Psychol.*, 40, 1950, 398-410.

extremes of symmetry and asymmetry. The test of relationship between duration and number of trials was achieved by a comparison of groups A_1, B_1, and C_1 (Part 1) with A_2, B_2, and C_2 (Part 2). In Part 1, there were 25 exposures of long duration; in Part 2, there were 50 exposures half as long.

From the symmetry- asymmetry hypothesis it was predicted that the illusion should be reduced most sharply under Condition *A* and not at all under Condition *C*. From the hypothesis concerning the relation between duration and number of trials, it was predicted that the two parts of the experiment should produce equivalent results.

Subjects. There were six groups of 10 *S*s each, all student nurses at the Hospital of the University of Pennsylvania, and all with reasonably good vision, corrected or uncorrected. The *S*s ranged in age from 17-21 yr. All were women, paid to participate in the experiment. They were assigned at random to the various treatments.

Apparatus. The Müller-Lyer figure was drawn on heavy white cardboard in black India ink. Main lines were 100 mm.; oblique lines were 20 mm. at angles of 45° from the main lines. By mounting both segments of the figure on a rack with a sliding groove, the figure was made adjustable. At the back of the apparatus there were two millimeter-scales, one backward, which enabled *E* to read the amount of illusion directly in mm. regardless of the orientation of the figure. A large white cardboard slid back and forth in front of the figure to cover it between trials. These items were so mounted on a stand that the figure was at about eye-level. A fixed rod on this stand fitted through two eyelets attached to the figure and held it in place. The figure was reversed by lifting it off the rod, turning it over, and sliding both eyelets back on the rod.

Procedure. Five of the 10 *S*s in each condition began with the short segment on the left, the other five with the short segment on the right. With chin in a chin-rest, *S* was instructed to fixate the apex of the middle angle of the figure throughout the experiment, and to bring her eyes back to this point whenever she became aware of moving from it. Each *S* was required, at six different times during the experiment, to adjust the length of one of the segments until both looked equal. *S*s in Part 1 were asked to make an equality-setting before the first trial, and after Trials 5, 10, 15, 20, and 25. *S*s in Part 2 set the figure at the beginning and after Trials 10, 20, 30, 40, and 50. At the end of the experiment, each *S* was asked if she had previous knowledge of the illusion and whether she had set the segments to apparent or to objective equality. The duration of each trial in the different conditions ranged from 7.5–60 sec., values judged sufficient for satiation since previous investigators had operated effectively within the same limits.[3]

RESULTS

Of 60 *S*s, 33 had prior knowledge of the illusion. All *S*s reported that they had set the segments to apparent rather than to objective equality, and the scarcity of negative settings lends credence to this finding. The average intertrial interval was 3 sec., and the average judgment-time 6 sec. Total exposure-time (trial-time *plus* judgment-time) amounted to approximately 13 min.

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[3] Köhler and Wallach, *op. cit.*, 269-357; E. R. Hammer, Temporal factors in figural after-effects, this JOURNAL, 62, 1949, 337-354.

The initial magnitude of the illusion ranged from 0–30 mm. with a grand mean of 16.1 mm. Group-means ranged from 14.7-17.6 mm. with no two groups significantly different from each other as determined by Mann-Whitney *U*-tests. Five *Ss* had no initial illusion. Of these, one did not experience the illusion at any time during the experiment, while the others ranged variably about zero. These *Ss*, two of group A_2 and three of group B_2, were eliminated from the analysis. Table II shows the mean setting for each judgment made by each of the six groups.

Symmetry. Analysis of data by means of Alexander's general test for trend showed that substantial reductions in magnitude of the illusion were achieved by all groups, *i.e.* the slopes of all curves differed significantly from zero.[4] Of the 55 *Ss*, the 42 who showed decreases are distributed about equally over all groups. In neither the *A* nor the *B* groups was the amount of reduction in the illusion significantly greater than in the *C* groups. The evidence fails to support the symmetry hypothesis.

TABLE II

MEAN MAGNITUDE OF THE ILLUSION (IN MM.) FOR EVERY JUDGMENT IN EACH GROUP

Group	Judgment					
	1	2	3	4	5	6
A_1	15.8	10.5	12.9	9.9	10.2	7.6
B_1	15.8	6.9	9.3	5.5	5.8	6.0
C_1	16.7	13.0	12.8	9.5	10.1	11.1
A_2	16.5	11.4	9.4	9.5	10.0	8.2
B_2	17.6	14.3	12.1	9.7	10.4	12.1
C_2	14.7	12.5	10.0	10.3	10.4	9.9

Frequency and duration of exposure. Alexander's method showed that the curves of Groups B_1 and C_1 deviated significantly from a straight line while the curves of Groups B_2 and C_2 did not. It appears that a small number of long trials results in a curvilinear decrease, while a large number of short trials does not. The hypothesis demands, however, that there be a reduction of the illusion either when the exposures are few and relatively long, or when they are many and relatively short, and all four of these groups show such significant reductions. From this point of view, the data support the duration-and-number hypothesis as tested in this experiment.

CONCLUSION

Exposure to the Müller-Lyer figure appears to be the only necessary condition for reduction of the illusion, and very little exposure-time is required. Some *Ss* do not experience the illusion at all. The data of the present experiment provide scant support for satiation-theory.

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[4] H. W. Alexander, A general test for trend, *Psychol. Bull.*, 43, 1946, 533-557.

THE ROLE OF REPETITION IN LEARNING

By MICHAEL WOGAN and ROLLAND H. WATERS, University of Florida

In a recent paper on paired-associates learning, Rock reported no significant differences between the performance of a control group which learned a constant set of pairs and an experimental group for which new pairs replaced those missed on any trial.[1] The present experiment was designed to replicate Rock's study and, in addition, to test the hypothesis that the control group would relearn more rapidly than the experimental group after an interval of one week. Since Rock's results seem to call into question the role usually assigned to repetition in learning—the formation as well as the strengthening of associations—a replication of his study in another laboratory to test the generality of his findings is desirable. Rock suggests that the role of repetition in associative learning is that of strengthening associative linkages which are formed on a single trial. From this assumption it would follow that the control group should exhibit better retention than the experimental group.

Procedure. The stimulus-items were letters and doubleletters (*A, AA, B, BB, Z, ZZ*). The response-items were numbers from 1-50. The response-numbers were drawn at random and paired with successive stimulus-items to form a pool of 50 pairs. All the letters of the alphabet were used except *I*. The pairs were typed on 5 × 5½-in. cards and presented by means of a Tachitron tachistoscope. On the back of each card, the appropriate item was typed in the stimulus-position, with the response-position left blank.

A metronome was used to regulate the duration of exposure for each pair during each trial. (A trial included a presentation and an immediate test of recall.) One pair was exposed for 3 sec., a 5-sec. interval followed, the next pair was exposed for 3 sec., and so forth. One minute after presentation of the list, the stimulus-items were shown at the rate of one every 5 sec. and *S* was required to give the appropriate response. The next trial followed after a 30-sec. interval. The same procedure was followed both in learning and in relearning. The order of the pairs was randomized by shuffling before all trials throughout both learning and relearning. The stimulus-items were again shuffled before each recall test.

Undergraduate men and women, students in psychology at the University of Florida, served as *S*s. They were randomly divided into two groups of 25 each. During learning, *S* was required to master a list of 10 pairs to a criterion of one errorless trial. A list for each *S* in both groups was randomly selected from the pool of 50 pairs. Each member of the control group was shown his original list on each successive trial until it had been mastered. Each member of the experimental group was shown his original list on the first trial. All of the pairs which were correctly associated, as shown by the recall-test on this trial, were retained for the next trial; all pairs not correctly associated were removed from the list and new pairs, drawn from the pool at random, were substituted. The same procedure was followed on all subsequent trials until the criterion was reached.

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* Received for publication December 8, 1958. We are indebted to Miss Joan Williams for her help in the statistical analysis.

[1] Irving Rock, The role of repetition in associative learning, this JOURNAL, 70, 1957, 186-193.

During relearning, one week later, *S* first was given a test for retention of the learned material, and he then was required to relearn the material to one errorless trial. For *S*s in the experimental group, the list presented was one on which the criterion in learning had been achieved. During relearning, no substitutions were made in the lists. Total errors and total number of trials to the criterion were taken as measures of learning and relearning.

Results. The results are shown in Table I. There was no significant difference between the control and experimental groups on Trial 1. Since the conditions for both groups were identical with the exception of random list-differences, this result supports the assumption that the two groups were equal in learning ability. Nor did the two groups differ significantly either in errors or in trials to criterion.

TABLE I

PERFORMANCE OF THE TWO GROUPS

	Experimental	Control	Difference
Original learning			
Errors			
Trial 1	6.92	6.84	0.08
To criterion	16.12	15.60	0.52
Trials	4.52	4.52	0.00
Relearning			
Errors			
Trial 1	2.80	3.48	−0.68
To criterion	3.00	4.60	−1.60
Trials	0.84	1.52	−0.68*

* Significant at the 5% level.

Vincent curves, plotted for both groups, showed their rate of progress to be approximately equal.

In relearning, neither mean errors on the first test of retention nor mean errors to criterion showed significant differences between the control and experimental groups. In terms of trials to criterion, however, the experimental group was *superior* at the 5% level of confidence.

Discussion. The results for original learning are in agreement with those obtained by Rock. The results for relearning might lead us to go even further than Rock and conclude that frequency neither facilitates the formation of connections nor strengthens corrections already formed, but there are, of course, other alternatives. One possibility is that the experimental *S*s are given the opportunity to construct a list which is easier to learn and to remember than that which *E* arbitrarily selects for the control *S*s. Learning the easiest items first, the experimental *S*s then select new items which fit well (interfere least) with those already mastered. This interpretation remains to be evaluated empirically.

SUMMARY

Rock's results on the role of frequency in paired-associates learning were replicated and extended. The experimental group, for which missed items were replaced from a pool, not only learned as rapidly as the control but showed better retention. The interpretation is offered that the substitution-procedure permits the experimental group to select an easier list than that of the control group.

THE EXPRESSION OF CERTAINTY

By P. J. Foley, Defence Research Medical Laboratories, Toronto, Canada

In a recent experiment, the subjects (*S*s) had to predict which of two stimuli would be presented next in a series. It was noticed that statements such as 'Stimulus A' or 'Stimulus B' were not made. Instead, replies tended to take the form "I think it will be B," or "I am sure it will be A," and so on. Since the interpretation of these statements required some evaluation of their meaning as expressions of certainty, the following investigation was undertaken.

Subjects. Thirty-eight *S*s were used. Twenty-nine were RCAF Flight Cadets, all of whom had been educated up to at least Grade XII standard. The remaining nine were members of the professional staff of these laboratories.

Procedure. The following questionary was prepared and given to each *S* to complete.

> Assuming the probability that an event X will occur can be assigned a value ranging from 1 (X will not occur), to 10 (X will occur), what values would you assign to the following statements:
> (1) I am sure X will occur ________
> (2) I suppose X will occur ________
> (3) I am certain X will occur ________
> (4) I think X will occur ________
> (5) I am positive X will occur ________

Results. A frequency-distribution showing the number of *S*s who assigned values to each statement is shown in Fig. 1. These distributions include the results from both groups of *S*s—the Cadets and members of the professional staff, there being no difference between them. The distributions were also plotted as cumulative frequency-polygons and examined, using the Kolmogrov-Smirnov Test, which is sensitive to any kind of difference between distributions.[1] It was apparent from this test that the distribution for 'suppose' is significantly different from the distribution for 'think,' both these distributions being different from the other three. These three, 'sure,' 'certain,' and 'positive,' form a group, *i.e.* they have the same general form of skewed distributions. Within this group, however, the distribution for 'sure' is significantly different from the distribution for 'positive.' The distribution for 'certain' lies between these for 'sure' and 'positive,' being much closer to the former.

Conclusions. With regard to the types of *S* tested the following conclusions may be drawn.

(1) The five words express different degrees of subjective certainty; the order for increasing certainty is 'suppose,' 'think,' 'sure,' 'certain,' and 'positive.'

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* Received for publication December 19, 1958. Project No. 76, DRML Report No. 76-6, PCC No. D77-94-20-21, H.R. No. 167.

[1] H. M. Walker and J. Lev, *Statistical Inference*, 1953, 426-428.

(2) The words also differ in the range of meanings each one has. 'Positive,' for example, is relatively unambiguous, whereas 'think' may express any degree of certainty from very little confidence to complete assurance. "I think that X will occur" seems to mean for some *S*s, "I think X is unlikely to occur." Ranking the words in terms of degree of ambiguity, the order from most to least is 'think,' 'suppose,' 'sure,' 'certain,' and 'positive.'

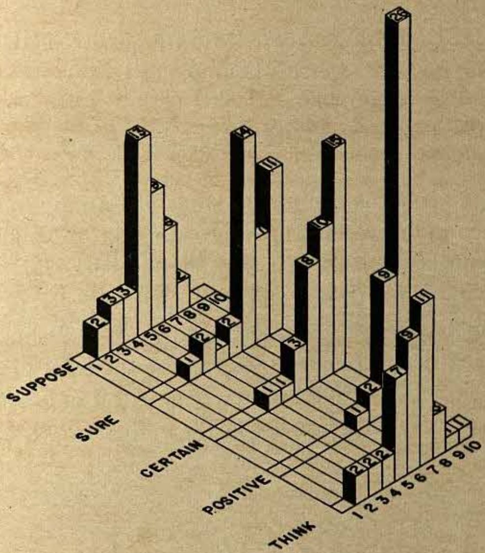

FIG. 1. FREQUENCY-DISTRIBUTIONS SHOWING THE NUMBER OF *S*S WHO ASSIGN VALUES TO EACH OF THE FIVE WORDS

APPARATUS

IMPROVED APPARATUS FOR THE STUDY OF LEARNING IN FISH

By Nicholas Longo and M. E. Bitterman, Bryn Mawr College

A lever-depression technique for the study of learning in fish, described several years ago in this Journal,[1] has been used to advantage in a recent series of experiments with the African mouthbreeder, *Tilapia macrocephala*.[2] The fish is trained to strike at a target lowered into its individual living-tank. Displacement of the target, which is mounted on a light lever, completes an electrical circuit at a point above the level of the water and thus provides an objective index of response. A bit of food dropped into the water serves as reinforcement. (In the earliest work, *E* introduced a small live worm with the aid of an eye-dropper; later the worm was replaced by a pellet of dry food—Aronson's mixture—also manually introduced; now an automatic pellet-dispenser is activated by *S*'s response.) The technique is adaptable to a wide range of problems. One may, in the Skinnerian fashion, measure rate of response to a continually available target, or one may measure the latency of discrete responses to a target that is lowered into the water at the beginning of each trial and withdrawn after *S* has responded. With two or more targets presented simultaneously, the determinants of choice may be studied.

The only difficulty thus far encountered in such experiments derives from the insensitivity of the mechanical system which has been used to register contact with the target. The responses of a well-trained fish are detectable even with the relatively crude equipment, but the delicate, tentative contacts of *S*s being trained for the first time may fail to register. When the target is baited with liver to encourage approach in pretraining, the difficulty of detecting response is increased, because the liver tends to evoke a biting rather than a striking movement. Whether *E* intervenes at the outset to reward contacts which do not register, or whether he does not, a substantial proportion of *S*s may fail to develop a satisfactory response. Some colleagues have been unable to train even a single animal, although others, of course, have been more successful.[3]

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* This work was supported in part by Grant G-2246 of the National Science Foundation to the Department of Psychology, Bryn Mawr College.

[1] J. V. Haralson and M. E. Bitterman, A lever-depression apparatus for the study of learning in fish, this Journal, 63, 1950, 250-256.

[2] Jerome Wodinsky and M. E. Bitterman, Discrimination-reversal in the fish, this Journal, 70, 1957, 569-575; M. E. Bitterman, Jerome Wodinsky, and D. K. Candland, Some comparative psychology, this Journal, 71, 1958, 94-110; Wodinsky and Bitterman, Partial reinforcement in the fish, *ibid.*, 72, 1959, 184-199.

The equipment now used in this laboratory is as sensitive in detecting responses as one might wish.[4] The target (a disk of light metal or a fold of window-screening) is mounted on a light rod (a 4-in. length of stainless-steel tubing, 1/16 in. in diameter) set into the needle-holder of a crystal phonograph cartridge (Astatic 16). Movement of the rod is damped sharply by a small, felt-lined arc of plastic which bears against it an inch or so above the target (Fig. 1). No housing of any kind is required for the rod or for the target. The cartridge is fixed to a plastic wedge at the end of a counterweighted metal arm by means of which the target is moved in and out of the water. The angle of the wedge is determined by the angle of the needle-holder (now rod-holder) relative to the case

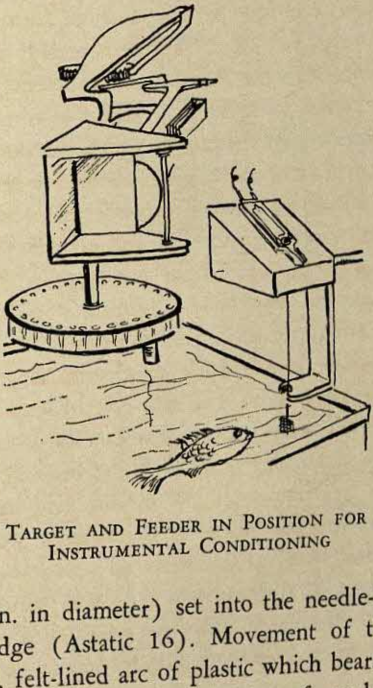

FIG. 1. TARGET AND FEEDER IN POSITION FOR SIMPLE INSTRUMENTAL CONDITIONING

of the cartridge. The output of the cartridge is led to a high-gain amplifier, and the output of the amplifier activates a set of relays which register response and control its consequences.

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[3] W. A. Deterline, An automatic device for the investigation of operant behavior in the fish, this JOURNAL, 69, 1956, 291-295.

[4] We are indebted to Mr. P. J. Sentner, of the Department of Physics, Bryn Mawr College, for suggesting the method, and to Mr. L. R. Holland, of the same Department, for assisting us in its development.

The amplifying and controlling circuit is diagrammed in Fig. 2.[5] The output of the amplifier activates *RL1* (Advance 9813B, 10,000 Ω, 4PDT). Two of its four sets of contacts, which run to binding posts (shown in the diagram as solid circles), may be used for recording response and for supplementing its sensory feedback. A third set of *RL1* contacts (normally open) is used to activate *RL2* (Sigma 11F-60000-G/sil, SPDT), which in turn shorts out *RL1* for an interval from 0 to about 1 sec. as determined by the setting of a variable resistance (marked *TIME* in the diagram). The output of the crystal thus is converted into a discrete pulse. A pair of binding posts in parallel with the normally open *RL2* contacts which take out *RL1* (marked *HOLD* in the diagram) are connected externally whenever the target is not in the water. A fourth set of *RL1* contacts (normally open) is used to energize *RL3* (Advancce 9B138, 115-v. AC, 4PDT) whose contacts (led to binding posts) may be used to reinforce response, to record reinforcement, and to provide whatever secondarily reinforcing stimuli may be required. By means of a pair of binding posts in parallel with the *RL1* contacts which activate *RL3* (marked *T*), it is possible (*e.g.* in pretraining) to introduce reinforcement independently of response. Response is reinforced only when

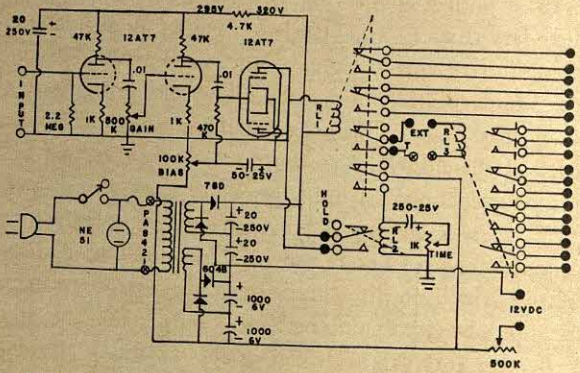

FIG. 2. DIAGRAM OF THE AMPLIFYING AND CONTROLLING CIRCUIT
(In the new models of the apparatus, a bank of capacitors is substituted for the variable resistor marked 'TIME.')

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[5] This equipment may be purchased at a very reasonable price from Mr. John H. Litchert, 715 Woodland Drive, Llanerch, Pennsylvania.

a connection is made across another pair of binding posts in series with the coil of *RL3* (marked *EXT*). The circuit also provides a 12-v. DC supply of better than 1 a. for the operation of supplementary devices.

A simple and serviceable pellet-feeder (shown in Fig. 1) may be made from a Western Electric 44-position stepping relay (D-162768, 12 v. DC, continuous rotation). The contacts of the relay are removed, and its shaft is replaced with a longer one of stainless-steel tubing, to which is fixed a Plexiglass disk (½ in. thick and 4 in. in diameter). The disk rides on a stationary metal plate of the same diameter which is supported by a secondary shaft inside the hollow one. Forty-four ⅛-in. holes, each of which may be filled with one or more pellets of food, are drilled at the border of the disk, and as the disk rotates (stepped by the relay) the contents of each hole fall into the water through an opening in the metal plate. The pellets employed are screened repeatedly for uniformity of size. A pellet-feeder is preferred to a powdered-food dispenser, such as that described by Deterline,[6] because it makes for greater consistancy in consummatory behavior and because it eliminates the spurious reinforcement in the intervals between trials which is afforded by stray particles of food.

With such equipment, training *S* to hit the target is a rather simple affair. The individual living-tank is carried to the experimental situation (after waste materials have been siphoned out and the standard water-level restored). At predetermined intervals, a training timer led to the binding posts at *T* makes a connection between them which delivers a pellet of food and simultaneously dims an overhead lamp. When *S* is taking these pellets readily, a liver-baited target is introduced—the liver, protected by wire screening, is unattainable—and each contact with the target automatically is reinforced with a pellet of food. The response transfers readily to the unbaited target. Under these circumstances, a stable response develops rapidly and dependably in the mouthbreeder. As Fig. 3 indicates, the technique is satisfactory even with a form as delicate as the killifish, *Fundulus heteroclitus*.[7]

The tracings reproduced in Fig. 3 were made with the excellent Davis cumulative recorder.[8] The upper one was made by a mouthbreeder and the lower one by a killifish, each on a fixed-interval schedule of 2 min. The mouthbreeder had about 170 continuous reinforcements followed by about 360 reinforcements on the fixed-interval schedule before these tracings were made; the killifish had about 15 continuous and 210 fixed-interval reinforcements.

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[6] Deterline, *op. cit.*, 291-295.

[7] We were introduced to this animal by our colleague, Miss Jane Openheimer (Functional regulation in *Fundulus heteroclitus* embryos with abnormal central nervous systems *J. exp. Zool.*, 115, 1950, 461-492), whose work on the suppression, rearrangement, and duplication of its brain parts by transplantation during gastrula stages suggests some interesting behavioral studies.

[8] Manufactured by Davis Scientific Instruments, 12137 Cantura Street, Studio City, California.

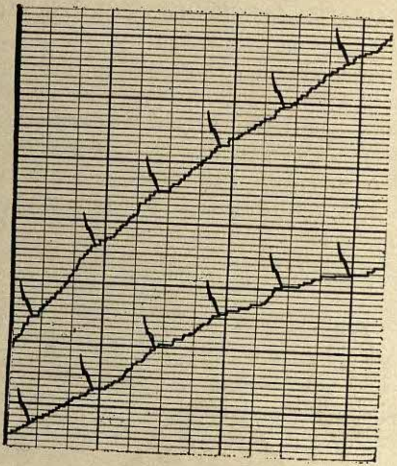

FIG. 3. CUMULATIVE FREQUENCY OF RESPONSE IN MOUTHBREEDER AND KILLIFISH ON A FIXED-INTERVAL SCHEDULE

(The upper record was made by a mouthbreeder and the lower one by a killifish. The pips indicate reinforcements.)

It should be obvious that the apparatus described here can be used for the study of learning in a variety of small animals other than fish.

APPARATUS NOTE

A RAT-HOLDER WITH ELECTRODES FOR *GSR* MEASUREMENT

The problems involved in obtaining records of *GSR* from the rat are considerable. This animal has a remarkable capacity for squirming out of diverse types of restraining sling or harness and for disengaging its feet from electrodes which have been fastened to them in various ways. The apparatus illustrated in Fig. 1 represents a reasonably satisfactory solution to these problems, arrived at after a surprisingly long series of contests from which the animal emerged victorious.

The platform of the holder is cut from ½-in. lucite sheet. The dimensions given are suitable for the average adult laboratory rat. The upper edges of the various

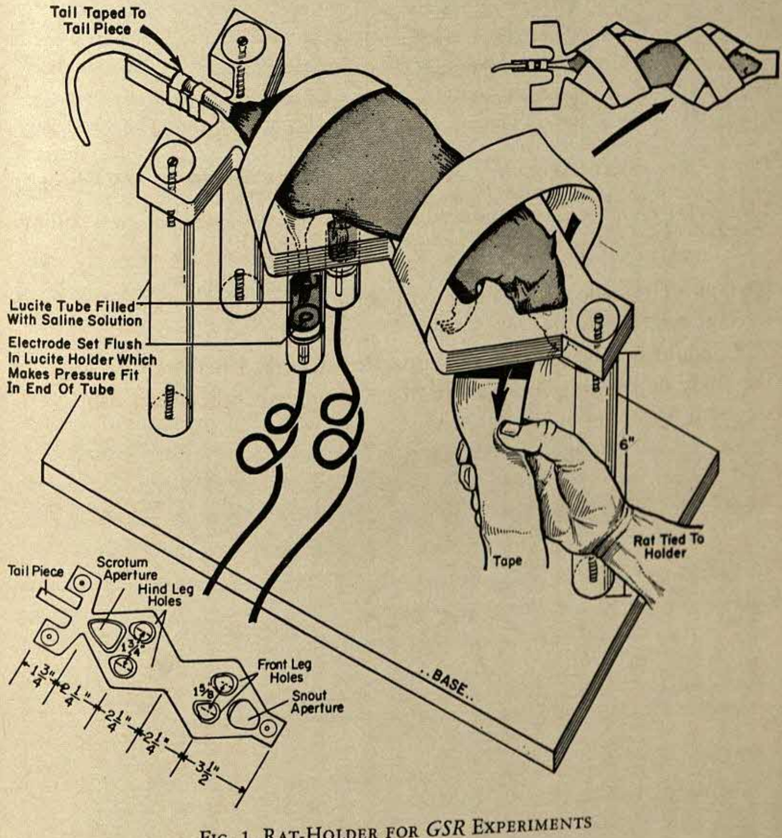

FIG. 1. RAT-HOLDER FOR *GSR* EXPERIMENTS

openings are flared or rounded for the comfort of the animal. Lucite tubes, 3 in. long, are cemented to the bottoms of the hind-leg holes and the electrode-holders are in the form of lucite plugs which make a pressure fit at the lower ends of these tubes. Electrodes are inset flush in the upper faces of these plugs with the connecting wires coming out the bottom. The simplest satisfactory electrode is a ½-in. disk of pure zinc, which must be sanded clean before each use. It should be coated with a zinc-sulphate paste before insertion in the tube; a disk of plastic sponge is then pressed down over each electrode and two-thirds of the tube filled with saline solution. Alternatively, a two-element type of electrode may be used, consisting of a ¼-in. center disk of pure lead with a concentric ring, ⅛-in. thick, also of lead, having a ½-in. inner diameter. The ring-electrodes are to be connected to the source of constant current and the center electrodes to an independent high-impedance voltage-measuring instrument. These electrodes also are cleaned by rubbing with fine-grit emory paper and may be used directly with the saline electrolyte.

With the electrodes ready, *S*'s hind legs are inserted through the openings into the electrode-tubes. Next, the tail is firmly attached near the butt to the tail-piece with a length of plastic tape (this is a two-person game). Finally, while one *E* keeps the animal in place, the other proceeds to wrap him snugly to the platform with 2-ft. lengths of 3-in. elastic bandage, the ends of which are attached under the two rear edges of the platform. These are wrapped, one at a time, diagonally over the *S*'s hindquarters, back under the middle of the platform, then diagonally forward again over the shoulders, where they are fastened to the leading edge. Care should be taken with male animals not to wrap under the scrotum. The wrapping should be tight enough to prevent the rat from lifting its legs from the holes but not so tight as to interfere with breathing.

A rat can remain in such a holder for several hours without any apparent ill effects. A variety of stimuli may easily be presented. It would be possible to make a lever-manipulandum available to the forepaws, although that has not yet been attempted in our laboratory.

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David T. Lykken
Richard Rose

NOTES AND DISCUSSIONS

A TABLE OF DISTANCES FOR THE SEMANTIC ATLAS

In an earlier publication the writers presented an atlas of semantic profiles for 360 words to provide the raw material for testing several hypotheses concerning the semantic differential and its relation to other instruments.[1] The present report announces a further treatment of these data intended to facilitate the investigation of hypotheses concerning the nature of the 'semantic space.'

Osgood, Suci, and Tannenbaum suggest that meanings may be considered as points distributed through an *n*-dimensional Euclidean space.[2] An hypothesis which immediately follows is that generalized distance in the semantic space between any given two points may be related to the kind, magnitude, or intensity of other behaviors involving the points as stimuli or responses. In other words, judgments of similarity, phenomena related to generalization of both stimuli and responses, and mediational effects in general may be expected to manifest themselves in proportion to the distance between points in the space. It is further suggested that 'distance' (D) may be best measured by the generalized formula, $D_{i1} = (\Sigma^n \, d_{i1}{}^2)^{\frac{1}{2}}$ where D_{i1} is the distance between concepts i and 1, and $\Sigma^n \, d_{i1}{}^2$ equals the sum over n factors of the squared difference between i and 1 on each of the factors. In practice, instead of using factor-scores, investigators have computed D directly from the scale-values of the semantic differential, usually observing the caution that an equal number of scales is used to represent each factor. This technique and findings which bear on its validity may be found in Osgood, Suci, and Tannenbaum's book on meaning.[3]

Table of Distances. Research on the psychological significance of D has encountered difficulties. To select certain characteristics in the Ds being

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* This note refers to investigations carried out at the University of Minnesota under Contract N8 onr-66216 with the Office of Naval Research. We are indebted to the staff of the Digital Computer Laboratory of the University of Illinois for invaluable assistance.

[1] J. J. Jenkins, W. A. Russell and G. J. Suci, An atlas of semantic profiles for 360 words, this JOURNAL, 71, 1958, 688-699.

[2] C. E. Osgood, G. J. Suci and P. H. Tannenbaum, *The Measurement of Meaning*, 1957, 90-97.

[3] *Ibid.*, Chaps. III and IV.

studied, *E* requires a large population of *D*s. Lack of such populations has led to the study of words or concepts which are selected in the anticipation that they will lead to *D*s of given relative sizes. Such populations may be biased by the original selection and obviously are not wholly suitable for research on *D* itself as an index of similarity and difference. To facilitate such research the table of *D*s for the 360 words in the semantic-differential atlas has been computed. With the ILLIAC computer, the *D*s between every atlas-word and the other 359 words were computed, a total of 64,620 *D*s. These *D*s were punched into IBM cards and duplicated to provide a full set of entries in all the rows and columns (129,240 entries). The complete table was printed on a Ditto master by IBM tabulator and reproduced by the Ditto process as a 720-page table.

The table consists of simple *D*s between the concepts across the 20 *scales* used in the atlas-study. The *D*s therefore do not represent each factor equally, but rather are in rough proportion to the amount of variance accounted for by each factor. There are eight 'evaluative' scales, three 'potency'-scales, three 'activity'-scales, and two scales each for 'stability,' 'tautness,' 'novelty' and 'receptivity.' The atlas-data can be converted into factor-scores and *D*s computed across the factors, of course, but the writers preferred to explore the *D*s computed from the scales first. It was felt that this analysis remained 'close to the data' and would not be much influenced by shifts and changes in the factor-structure, if such occurred in the subgroups of concepts being considered at any one time. The choice, however, is necessarily arbitrary. Experimental work alone can decide the most fruitful procedure.

To facilitate certain kinds of work with the table, Kent-Rosanoff word-association data drawn from the Minnesota norms have been included in the table where appropriate.[4] For each Kent-Rosanoff stimulus-word appearing in the atlas, a list is given of the free-association response-words also appearing in the atlas. The rank of each response and the frequency with which it occurs are also given.

Use of the Table. The Table of Distances may be used in the following fashion. A word is selected from the list of words in the "Atlas of semantic profiles for 360 words." The list-number provides an entry into the "Table of Distances." This number can be found in the upper left-hand corner of two pages in the table. The first page reports the distances between the word selected and each of the other atlas words numbered from 1-240. The second page reports the distances between the word selected and the other atlas words numbered from 241-360, and lists the associative responses to the selected word, when available. The distances are given to three decimal places; the decimal points are omitted.

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[4] Russell and Jenkins, The complete Minnesota norms for responses to 100 words from the Kent-Rosanoff word-association test, *Technical Report No. 11* University of Minnesota, Contract number N8 onr-66216, 1954, 3-39.

Example. Suppose one is interested in the data related to the word AFRAID. This is Word 4 in the atlas. One looks up the pages numbered 4 in the Table and finds:

(1) 3460; (2) 4839; (3) 8081; (4) ——; (5) 7798; (6) 10575; etc.

This line may be read: AFRAID is located in the space at a point 3.460 units from Word 1 (*abortion*), 4.839 units from Word 2 (*abrupt*), 8.081 units from Word 3 (*adorned*), 7.798 units from Word 5 (*agile*), 10.575 units from Word 6 (*America*), etc. The theoretical range of the *D*s is from 0.000-26.833 with 20 seven-step scales. The word closest to AFRAID is Word 111 (*fear*) with a *D* of 1.658, and the word farthest away is Word 142 (*God*) with a *D* of 11.831.

The associative data are presented as follows:

Word-Number	Word	Rank	Frequency
111	Fear	1	261
74	Dark	3	98
34	Brave	4	63
270	Run	6	12
150	Happy	7	11 etc.

These entries indicate that the primary, free-associative response to AFRAID is *fear* with a frequency of 261 out of 1008 *S*s; the third most popular response is *dark* with a frequency of 98 out of 1008 *S*s, and so forth. These data, in conjunction with the *D*s, make it possible to select words with varying strengths of association and semantic distance simultaneously.

Availability of the Table. Because of the high cost of reproducing the material and the unusual size of the Table, only a limited number of copies have been prepared. Investigators who are interested in using these data for research purposes may borrow copies of the Table from the University of Minnesota Library through the Inter-Library Loan Service. A set of IBM cards containing all the *D*s (129,240 cards) is available at the Psychology Department, University of Minnesota, and a duplicate set is available at the Institute for Communications Research, University of Illinois.

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ISOLATION AS A FACTOR IN IMMEDIATE RECALL

Using lists of combined nonsense-syllables and numbers, Siegel found better immediate recall of isolated items than of non-isolated ones.[1] Siegel's results were replicated by Postman and Phillips who used his lists; intentional learners recalled the isolated items more readily, although incidental learners did not.[2] Still another study with Siegel's lists was done by Saul and Osgood.[3] They constructed a scale of homogeneity, the opposite of isolation, with which to rate *each* of the items in the lists. Neither in their own data nor in Siegel's, which they reanalyzed, did they find a consistent relationship between homogeneity and percentage of immediate recall.

The procedure of Saul and Osgood leaves something to be desired. For one thing, the score of each item is based on a comparison with both preceding and succeeding items in the series. Three points are given if either adjacent item is of the same class as the one being scored (class referring to whether it is a nonsense-syllable or a number), two more points are given if either item once removed is similar, and one point is added if either item twice removed is similar. Considering the fact that the items are presented sequentially for their first and only exposure, the initial perception of an item can hardly be thought of as being influenced by the items which follow it in the series. Insofar as *perceptual* organization is concerned, a rating of homogeneity based on factors which cannot enter into perception is inappropriate. Furthermore, scores given to the items at either end of the lists by the procedure of Saul and Osgood may be questioned. The first item is scored as if it were preceded by three unlike items, the second item is scored as if there were two unlike items preceding the initial item, and the third item is scored as if there were one unlike item preceding the first item. The last item is scored as if it were succeeded by three dissimilar items, and so on.

An alternative rating procedure, which seems more appropriate, is the following: A rating of 'O' is given if the two immediately preceding items are unlike the one being rated; '1' if the immediately preceding item is unlike but the one before *it* is like the one being rated; '2' if the immediately preceding item is like but the one before *it* unlike; and '3' if the preceding items are like the one being rated. Under this system, succeeding items are ignored, and only the two preceding items are taken into account. Moreover, the first two items in the series are ignored.

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[1] P. S. Siegel, Structure effects within a memory series, *J. exp. Psychol.*, 33, 1943, 311-316.

[2] Leo Postman and L. W. Phillips, Studies in incidental learning: I. The effects of crowding and isolation, *J. exp. Psychol.*, 48, 1954, 48-56.

[3] E. V. Saul and C. E. Osgood, Perceptual organization of materials as a factor influencing ease of learning and degree of retention, *J. exp. Psychol.*, 40, 1950, 372-379.

Data for 54 *S*s obtained in a manner similar to that of Saul and Osgood were analyzied in terms of the revised rating, and the data of Siegel, of Postman and Phillips, and of Saul and Osgood were reanalyzed in the same way. The results are shown in Fig. 1. It is immediately apparent that, contrary to Saul and Osgood, there is a negative relationship between homogeneity and the percentage of immediate recall. (The same is true even in the case of Postman and Phillips' incidental learners, although the slope of the line is greatly reduced.) The more isolated items are recalled more easily than the less homogeneous items in every case.[4]

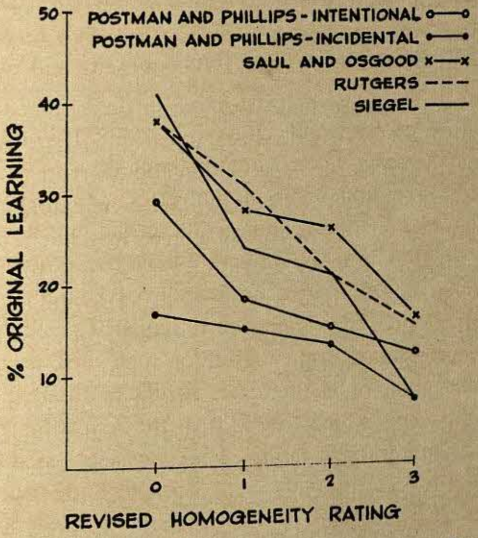

FIG. 1. RELATION BETWEEN REVISED HOMOGENEITY-RATING AND PERCENTAGE OF IMMEDIATE RECALL

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[4] It should be noted that the revised homogeneity rating, like that of Saul and Osgood, fails to relate to ease of retention as measured by percentage loss over immediate recall. Moreover, with reference to the data of Postman and Phillips, the revised rating shows no consistent relationship with immediate recall scores for items differing in color (black vs. red) as well as type (numbers vs. nonsense syllables).

Recently, Köhler had this to say about homogeneity: "Isolation in our [Gestalt] sense is a psychological (or neural) fact, and whether this fact will really occur under particular conditions cannot be decided by merely counting how many dissimilar items are objectively present in the neighborhood of a crucial item."[5] The present author agrees, but she has attempted to demonstrate here that, until some more appropriate phenomenological measure comes along, a judicious counting of neighboring items appears to offer a reasonable approximation of such a measure.

Rutgers University MARY E. MOORE

SALIVARY CONDITIONING BEFORE PAVLOV

The figure of Pavlov now bulks so large in the history of conditioning that his predecessors are virtually lost from sight. It may be interesting, therefore, to review some of the speculation and experimentation that anticipated Pavlov's initial findings. This will also help to make clear what his own contributions were. Let us first note briefly how Pavlov described salivary conditioning in an early paper and then turn to his predecessors.

In his speech of acceptance, when awarded the Nobel Prize in 1904, Pavlov summarized the research on the digestive processes that had gained him this honor and indicated the new direction in which he was pursuing this work—the study of conditioning alimentary processes.[1] During his studies of digestion, Pavlov discovered that psychical stimulation had, quite unexpectedly, stood out prominently among the factors that determined the activity of the digestive glands. He wrote:

> True, it has long been known that the sight of tasty food makes the mouth of a hungry man water. . . . In physiology, mention was made also of the psychical stimulation of both the salivary and gastric glands. It should be pointed out, however, that psychical stimulation of the gastric glands has not been universally recognized, and generally speaking, the outstanding role of psychical stimulation in the processing of food in the digestive canal has not met with proper acknowledgment.[2]

Pavlov then, after describing experiments distinguishing between the two types of stimulation of the salivary glands, continued:

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[5] Wolfgang Köhler, Perceptual organization and learning, this JOURNAL, 71, 1958, 313.

[1] I. P. Pavlov, *Selected Essays*, 1955, 129-148. (This book was published by the Foreign Languages Publishing House, Moscow. An edition that may be more accessible in America and that, over most of its length, is identical with the Moscow edition was published in New York, 1957, under the title, *Experimental Psychology and Other Essays*. The Nobel Prize speech is given in very incomplete form in the well-known edition translated by W. H. Gantt, I. P. Pavlov, *Lectures on Conditioned Reflexes*, 1928.)

[2] *Op. cit.*, 141.

> There is the reflex evoked by stimulation of the oral cavity; it is physiological, constant, and *unconditioned.* Then there is the reflex evoked by stimulation of receptors on other parts of the body surface; it is permanently subject to fluctuation and is, therefore, *conditioned.*[3] . . . Any phenomenon of the external world can be made a temporary signal of the object which stimulates the salivary glands, provided the stimulation of the mucous membrane of the mouth by the object has been associated once or more times with the action of the given external phenomenon on other receptor areas of the surface of the body.[4]

Pavlov evidently felt it unnecessary to document his statement that "it has long been known that the sight of tasty food makes the mouth of a hungry man water." It may be worthwhile to provide here some documentation of the prior knowledge of the effectiveness of 'psychical' stimulation.

The use of the expression, "the mouth waters" (meaning that it secretes abundant saliva in anticipation of appetizing food) dates back, according to *The Oxford English Dictionary,* at least to the sixteenth century. Here is an early citation:

> 1555 Eden *Decades*. . . . "These craftie foxes [*i.e.* cannibals] . . . beganne to swalowe theyr spettle as their mouthes watered for greedines of theyr pray."[5]

In the mid-eighteenth century Albrecht von Haller wrote what has been called the first textbook of physiology. In his discussion of salivation he noted, without elaboration, that hunger can provoke the flow of saliva.[6]

A few years later, Robert Whytt made a fuller statement in his pioneering treatise of reflex action:

> Thus the sight, or even the recalled *idea* of grateful food, causes an uncommon flow of spittle into the mouth of a hungry person; and the seeing of a lemon cut produces the same effect in many people.[7]

At the beginning of the nineteenth century Dumas dwelled at length in his textbook of physiology upon the way in which the salivary secretion is adapted to the vicissitudes of life. He wrote:

> There is no other humor which so promptly shares the effect of the impressions that the animal feels.[8] . . . Appetite or desire for food, subordinated to the power of habit, provokes a copious excretion of saliva at hours when we are accustomed to eat. Habit alone determines its flow into the mouth, if one resists the desire that it brings and whose satisfaction it presses.[9]

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[3] A better and more accurate translation of Pavlov's terms would have been 'unconditional' and 'conditional.' For reasons that the adjectival forms are to be preferred and why they have not been generally used, see M. A. Brazier (ed.), *The Central Nervous System and Behavior,* 1959, 201 f.

[4] Pavlov, *op. cit.,* 147.

[5] *The Oxford English Dictionary,* 12, 1933, 164.

[6] Albertus von Haller, *Primae Linae Physiologiae,* 2nd ed., 1758, 235.

[7] Robert Whytt, *An Essay on the Vital and Other Involuntary Motions of Animals,* 1763, 280.

[8] C.-L. Dumas, *Principes de physiologie,* 4, 1803, 565.

[9] *Idem,* 568.

In the *Dictionaire des Sciences Médicales* of 1820 there is an 11-page article by Ribes on saliva in which mention of these phenomena occurs at several points:

> When we are hungry and hear mention of foods that we like, the secretion of saliva increases; the odor or the sight of foods that we like produce the same effect.[10] . . . One of the best ways to procure it [saliva for chemical analysis] . . . consists of depriving an animal of food, for example a dog, putting a gag in its mouth and bringing toward it a morsel of roast meat that is still steaming. At once the salivary glands are excited; they swell and secrete so much saliva that for a time it forms an almost continuous trickle.[11]

Johannes Müller, in his *Handbuch der Physiologie des Menschen,* mentioned the effect of mental processes upon secretion in a chapter entitled, "Reciprocal action of the mind and organism":

> An idea excites a stream of nervous energy toward that organ whose secretion is appropriate to the idea, and the more so when it is accompanied by an emotional experience. Thus, saliva is secreted in greater abundance when food is imagined; milk, when the mother has her child nearby and pictures it desiring her; semen, when lewd pictures are presented."[12]

In 1852, Bidder and Schmidt published an important book on digestion. In it they stated that casual introspection demonstrates that the sight or even the mere thought of food can provoke the flow of saliva.[13] They experimented on gastric secretion by making fistulas into the stomach of dogs. In an observation that Pavlov was to cite, they noted that simply showing food to a dog could provoke the flow of gastric juice.[14]

Let us add one more name to this roster—that of James Ward. He alluded to the salivary phenomenon in his article on psychology in the ninth edition of the *Encyclopedia Britannica,* 1878. In a section on "Imagination or Ideation" he gave this example:

> The dog's mouth waters only at the *sight* of food, but the gourmand's mouth will also water at the *thought* of it.[15]

Having seen that 'psychical' stimulation of salivation had indeed been long known and extensively commented upon before Pavlov's work, we can now consider whether the principle of the formation of conditional reflexes had also been anticipated.

It will be recalled that Pavlov stated a conditional salivary reflex will be formed "provided that stimulation of the mucous membrane of the mouth by the object has been associated once or more times with the action of the given external phenomenon on other receptor areas of the surface of the body."

Compare Pavlov's statement with the following:

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[10] F. Ribes, Salive, in C. L. F. Panckoucke (ed.), *Dictionaire des sciences médicales,* 25, 1820, 463.

[11] *Idem,* 460 f.

[12] Johannes Müller, *Handbuch der Physiologie des Menschen,* 2, 1840, 568-569.

[13] F. Bidder and C. Schmidt, *Verdauungssäfte und Stoffwechsel,* 1852, 13.

[14] *Idem,* 35.

[15] James Ward, *op. cit.,* 9th ed., 22, 1878, 57.

> The flow of saliva into the mouths of hungry animals at the sight or smell of food is seen in dogs standing round a dinner table. The increased actions of the salivary glands have usually been produced by the stimulus of agreeable food on their excretory ducts during the mastication of it; and with this increased action of their excretory ducts the other terminations of those glands in the capillary arteries have been excited into increased action by the mutual association of the ends of the canals; and at the same time the pleasurable ideas, or sensual motions, of the sense of smell and of sight have accompanied this increased secretion of saliva. Hence this chain of motions becomes associated with those visual or olfactory ideas, or with the pleasure, which produces or attends them.

In this passage we have descriptions of two ways of exciting salivation. The first occurs through stimulation of the salivary ducts by food. The second occurs through stimulation of other receptors, provided this has previously been associated with the first type of excitation. The passage is from the 1796 edition of *Zoönomia* by Erasmus Darwin, grandfather of Charles Darwin.[16] The succeeding paragraph of this book gives another, and perhaps even clearer, example of how a visual stimulus can come through association to evoke a local circulatory reaction:

> The nipples of lactescent women are liable to become turgid at the sight of their young offspring. The nipple has generally been rendered turgid by the titillation of the lips or gums of the child in giving suck; the visible idea of the child has thus frequently accompanied this pleasurable sensation of parting with the milk, and turgescence of the tubes, which constitute the nipple. Hence the visual idea of the child, and the pleasure which attends it, become associated with those increased arterial actions, which swell the cells of the mammula, and extend its tubes; which is very similar to the tensio phalli visâ muliere nudâ etiam in insomnio."[17]

Darwin's examples are similar to those that Müller was to give in 1840, which we have already quoted. Müller, however, did not note that these responses were acquired.

The control of salivary secretion was studied in detail around the middle of the nineteenth century. Carl Ludwig demonstrated that salivation is controlled neurally. French investigators made fistulas in the ducts of the various salivary glands in order to analyze and measure their separate secretions. In his physiology course of 1855, Claude Bernard gave several lectures on salivation based on his research during the previous seven or eight years. It was probably during this period of research that Bernard made interesting observations on psychic control of secretion, observations which he seems to have published 20 years later in a popular article. Here he described how psychic stimulation could replace oral stimulation in evoking the salivary response; his experiments, furthermore, were performed with a physiological preparation very similar to that which Pavlov was to use half a century later:

> The nervous system, both sensory and vasomotor, governs all the phenomena of local circulation which accompany organic functions. Thus, saliva flows abundantly when a tasty object stimulates the nerves of the mucous membrane of the mouth, and gastric juice forms when foods touch the sensitive surface of the stomach. This mechanical excitation of the peripheral sensory nerves, coming to play upon the organ by reflex action, can, however, be replaced by purely psychic or cerebral excitation. A simple experiment demonstrates this. Taking a fasting horse, you expose the duct of the

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[16] Erasmus Darwin, *Zoönomia; or, the Laws of Organic Life,* 2, 1796, 444 f.

[17] *Idem,* 445.

parotid gland on the side of the jaw; you divide the duct, and nothing comes out; the gland is at rest. If you then show the horse some oats, or better, if, without showing it anything, you make a movement which indicates to the animal that you are going to feed it, immediately a continuous stream of saliva flows from the parotid duct, at the same time that the tissue of the gland becomes congested and becomes the seat of more active circulation. Dr. Beaumont observed analogous phenomena in his Canadian subject. The idea of a succulent morsel not only caused secretion of the salivary glands, but it also provoked an immediate inflow of blood to the mucous membrane of the stomach.[18]

Suggestive evidence that these observations of Bernard were actually made around 1850 comes from the fact that similar experiments were reported by another French invetigator, G. Colin, in 1852. Fig. 1, from Colin's text of 1854, shows a horse prepared for measurement of secretion of the parotid and submaxillary glands.[19] Colin looked for psychic secretion in both horses and ruminants, but with completely negative results. He could not evoke parotid secretion "in the hungriest of animals" by sight or odor of food.[20] This failure to obtain psychic secretion was balanced, however, by Colin's equally striking failure to obtain responses to chemical stimulation of the oral cavity. Putting salts, weak acids, or aromatic substances in the mouth were all ineffective, he claimed, in arousing salivary secretion during abstinence from food.[21] He noted that his results on salivation were often not in harmony with

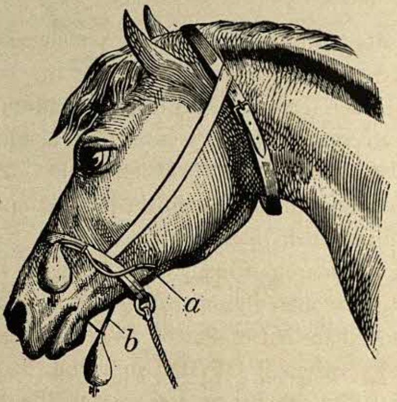

FIG. 1. HORSE PREPARED FOR COLLECTION OF SALIVA FROM THE PAROTID DUCT (*a*) AND FROM THE SUBMAXILLARY DUCT (*b*).
(From Colin, *op. cit.*, 1, 1854, 471.)
Claude Bernard also described similar preparations in detail in publications of the same period. These preparations were used for tests of both psychic and reflexive stimulation of salivation.

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[18] Claude Bernard, Des fonctions du cerveau, *Revue des deux mondes*, 98, 1872, 378.

[19] G. Colin, *Traité de physiologie comparée des animaux domestiques*, 1, 1854, 468.

[20] *Idem*, 471.

[21] *Idem*, 471.

those of Bernard, although he did not specify Bernard's work on psychic secretion. Bernard, although he cited certain observations of Colin, seems not to have mentioned cases where their results did not agree.

It appears then that Bernard, as well as Bidder and Schmidt, had made experimental demonstrations of psychic secretion in the early 1850s, Bernard working on salivation and Bidder and Schmidt on gastric secretion. Neither they nor their contemporaries seemed greatly surprised or even greatly interested in these demonstrations. Certainly they did not attempt to carry them farther.

Pavlov apparently never knew of Bernard's experiments.[22] In another paper of 1904, Pavlov did refer to "the beautiful adaptation of [the salivary] glands to external stimulations, as had already been foreseen by Claude Bernard.[23] In this case, however, he referred only to work showing that the amount and quality of saliva is appropriate to the substances put into the mouth.

From this brief survey, it is clear that the phenomenon of conditional reflexes had been observed several times during the nineteenth century. Furthermore, it had been given an explicit associationistic interpretation by Erasmus Darwin a century before Pavlov offered a similar interpretation. Nevertheless, it is also clear that much was left for Pavlov to do. The earlier investigators demonstrated already established conditional responses; they did not attempt to prove their interpretation by trying to establish new associations to stimuli which are not likely to be related to food under natural circumstances. This is undoubtedly one of the reasons that the earlier work attracted less attention than that of Pavlov. When a dog salivated to food, that didn't seem news; when it salivated to a bell, that was news. Since they did not attempt to establish new associations, the earlier workers did not produce any information about the actual progress of the formation of conditional responses. Only a thoroughgoing program of investigation, such as that begun by Pavlov, could show how fertile this field could become.

University of California MARK R. ROSENZWEIG

TWITMYER AND THE CONDITIONED RESPONSE

In the preceding Note,[1] Rosenzweig shows that the salivary reflex was well known before Pavlov; that numerous investigators had noted an increased flow of saliva when food was seen, smelled, recalled, thought of, or imagined; and that some (Bernard and possibly Dumas) had observed the arousal of the reflex by non-food stimuli—by movements associated with the giving of food, and by habit alone.[2] Neither Bernard nor Dumas saw the significance of their observations or followed them further. As far, therefore, as the conditioned salivary response is concerned, Pavlov is the discover of it. By showing that a dog's salivary reflex could be established to a bell—to an associated stimulus that was not a food—he supplied the *experimentum crucis* that established it.

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[22] M. A. B. Brazier (ed.), *The Central Nervous System and Behavior,* 1959, 170.
[23] Pavlov, *Lectures on Conditioned Reflexes,* 1928, 61.

[1] *Supra,* 628.

Rosenzweig limited his review to the salivary reflex—probably because Pavlov's early work on conditioning was concerned primarily with it. By so doing, Twitmyer's study of the knee-jerk, in which the discovery of the conditioned response was announced and the phenomenon first described, escaped his attention.

Pavlov reported his results in 1904; Twitmyer reported his, in his doctoral dissertation submitted to the University of Pennsylvania, in 1902.[3] The purpose of this Note is to invite attention to Twitmyer's study and to consider the reasons for its obscurity.

Twitmyer's study was undertaken to determine the variability of the knee-jerk. His discovery of the response, that was later to be called "conditioned" by Pavlov, was made accidentally. As a preparatory signal to warn his subject (*S*) that the stimulus-hammer was about to be dropped upon the patellar tendon, he sounded a bell. One day, while adjusting the apparatus for a new series of trials, he accidentally tapped the bell. Though the stimulus-hammer had not been released nor the patellar tendon struck, the knee-jerk followed. His *S* reported that the reaction was involuntary. Regarding this unexpected outcome, Twitmyer wrote:

> Two alternatives presented themselves. Either (1) the subject was in error in his introspective observation and had voluntarily moved his legs, or (2) the true knee-jerk, or a movement resembling it in appearance, had been produced by a stimulus other than the usual one.[4]

He immediately turned from his main problem to determine which of these alternatives was correct. An extended series of experiments upon 6 *S*s yielded the following results:

> Knee-jerks without taps on the tendons were obtained from all the subjects after a large number of preliminary experiments had been performed in which a bell was struck 150° before the blow fell on the tendon. These responses were not the result of voluntary effort on the part of the subject. Attempts to inhibit these kicks were wholly unsuccessful. The movement displayed the characteristic jerky or explosive appearance of the true knee-jerk. . . . No difference in the form of the graphic records of the new response and those of the normal knee-jerk was observed. . . . Preliminary experiments with both the tap of the bell and the blow on the tendon were necessary before kicks could be obtained with the bell alone; the number varied

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[2] *Supra*, 629 631 f.

[3] E. B. Twitmyer, *A Study of the Knee-Jerk*, 1902.

[4] Twitmyer, *op. cit.*, 24 f.

from 150 to 238 trials. With an increase in the number of experiments performed, the regularity of response with the bell alone was greatly increased.[5]

Though he found no differences in the graphic records of the two responses, Twitmyer suggested, anticipating future experiments, that the response might not be exactly the same, that differences between them might be detected if more adequate methods, a faster running kymograph, for example, were employed in recording the responses. He concluded his report with a statement that he intended to continue his investigation of the new phenomenon.

He did not, however, keep this promise; that much is a matter of historical record. The experiments, upon which his doctoral dissertation was based, were the only ones he ever reported upon this phenomenon. He had one of the most, if not the most, important experimental discoveries of his day and generation in his hand and he let it slip through his fingers! Why did he discontinue his work on this problem? Why were his findings ignored when reported? Why have they remained buried in obscurity ever since? The answers to these questions are matters of opinion, but the following facts are pertinent.

Twitmyer's dissertation was privately published. A small number of copies were deposited at the University library for exchange with a group of associated libraries and a few were presented to friends and professional acquaintances. Its circulation was consequently small and its readers few in number. The title of his dissertation, *A Study of the Knee-Jerk,* was not particularly exciting nor attention-compelling; it did not indicate that the study was concerned with anything new or especially important. It is doubtful, moreover, whether any of its readers at that time would have attached any great significance to his results. His study, being privately printed, was not listed in any of the annual indexes of psychology,[6] hence, as far as the psychological world at large was concerned, it was almost as though his study had not been reported.

To make up for the dissertation's lack of publicity, Twitmyer presented his work at the annual meeting of the American Psychological Association in 1904. The title of his paper, "Knee-jerks without stimulation of the patellar tendon,"[7] was a little, though not much, better than that of the dissertation. William James, president of the Association that year, presided at the meeting at which Twitmyer reported. His report, though presented before the elite of American psychology, fell dead. Not one of his hearers commented upon it after his presentation. The most important paper, as we now know, of that and many succeeding meetings of the Association was followed by—to Twitmyer—embarrassing silence![8] Irwin writes, in his necrology of Twitmyer, that "Twitmyer's own recollections of this occasion were always mingled with feeling of disappointment at the failure of his auditors to express interest in his results, of which he recalled no discussion whatever."[9]

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[5] Twitmyer, Knee-jerks without stimulation of the patellar tendon (abstract), *Psychol. Bull.*, 2, 1905, 43 f.

[6] It is not listed in the *Psychological Index,* in the annual bibliographies published by the *Zeitschrift für Psychologie,* nor in Twitmyer's bibliography published in *The Psychological Register,* 3, 1932, 507.

[7] *Op. cit., Psychol. Bull.,* 2, 1905, 43 f.

We have here the answer to our first question. Twitmyer did not continue his investigations of the new response because he received no encouragement to do so. The silence and indifference of his audience were prejudicial to further work. Twitmyer had just recently received his doctorate. He had his position in his University and in psychology to win and to hold. He could ill afford to continue work on a phenomenon in which no one but himself was interested. He turned, therefore, to problems approved by his confrères: to the psychological clinic and to the study and treatment of defects of speech, to which he devoted the remainder of his life.

Regarding the remaining questions—Why was Twitmyer's work ignored? Why has it remained obscure?—Irwin is of the opinion that "the intellectual climate [Boring's *Zeitgeist*] in which lived the American psychologist of the first years of this century" was responsible.[10] This is an easy answer and it has the virtue of closing the questions. Much more than an unreceptive *Zeitgeist* is, however, involved; at least more positive and concrete answers are available.

Twitmyer's first report (his dissertation) was, as we have seen, bound to obscurity by the very nature of its publication. His second attempt to gain a hearing—his paper before the American Psychological Association—fared no better. Indeed, in one respect, it fared worse as it not only failed to win the recognition he desired but in addition the indifference of his audience discouraged him from continuing work upon the problem. Though an abstract of his paper, which, as Irwin observed, "presents perfectly clearly the essentials of the discovery, a spark surely sufficient to fire the imagination of any one who was adequately prepared for it,"[11] was published in the proceedings of the meeting, it did Twitmyer's cause little good. Abstracts are not listed in bibliographical indexes and abstracts in the proceedings of yesterday's meetings are rarely read. A scholar, as Rosenzweig exemplifies in his review, would search in vain for references to Twitmyer's work. Unindexed abstracts of papers of 50-odd years ago and necrologies are not sources the historian could reasonably be expected to examine. Surely, the *Zeitgeist* is not responsible for the obscurity of Twitmyer's reports. Twitmyer, and he alone, is accountable for that. He never really published his results; never really promoted them. Had he 'christened' his discovery, given it an intriguing name, such as 'conditioned reflex' instead of calling it simply a "new phenomenon,"[12] had he published his reports in a periodical with world-wide circulation whose papers were listed in bibliographical indexes, had his dissertation come to Pavlov's attention, and had he, despite discouragement, pushed his investigations forward and effectively reported them, the history of this experiment would have been very different.

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[8] A good chairman, after throwing the paper open for discussion, would, particularly in the case of a young man giving his first report, have asked the first question to thaw the audience's reticence and to start the discussion rolling. Had James done that, the audience's reaction might have been different. Had Twitmyer received a spark of encouragement, he would have continued his investigation. Had he done that, 'conditioning' might have had its effective beginning in America instead of Russia. "Of all sad words . . . the saddest . . . it might have been."

[9] F. W. Irwin, Edwin Burket Twitmyer: 1873-1943, this JOURNAL, 56, 1943, 452.

[10] Irwin, *op. cit.*, 452.

Pavlov and Twitmyer announced their discoveries at approximately the same time and in the same *Zeitgeist*. Though Pavlov made his against the sounding board of the Nobel Prize, it did not fare much better than Twitmyer's; but Pavlov's survived whereas Twitmyer's did not. Something more than a *Zeitgeist* is required to explain this difference and that something is, as we believe publicity and promotion. 'Salivating to a bell by a dog' was news, after it had been vigorously promoted; but 'jerking of the knee to a bell by a man' was not 'news' when promotion was lacking. One does not hide one's light under a bushel if one wishes it to be seen; publicity and promotion are necessary to bring it to the fore.

Twitmyer had little ability, experience, or inclination along promotional lines. He 'missed the boat,' not because of an unfavorable *Zeitgist*, but because he was a young, inexperienced scholar, not a promoter. He did not know how to promote his discovery and he could not withstand discouragement. Pavlov, on the other hand, was experienced in the promotional arts, and he utilized them to their fullest extent. He gave his phenomenon a distinguishing name, "conditioned response," and, despite initial discouragement, he continued his experiments and reports. He could, of course, afford to do this as he was a man of established position and reputation, and he was also a man who could not be discouraged by an adverse intellectual climate. He persisted in his 'promotion' until he had created a *Zeitgeist* favorable to his work. History is replete with similar instances.[13]

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[11] *Idem.*

[12] There is much in a name, if only as a convenient handle for indexing and reference.

The University of Texas KARL M. DALLENBACH

ESTIMATING AVERAGE ERROR FROM THE STANDARD AND CONSTANT ERRORS

Occasionally one wishes to determine the average error (AE) from data for which the constant error (CE, bias) and the standard deviation (SD) or the standard error (SE) have already been computed.[1] One reason for this might be the desire to compare one's results with those of early workers who rather commonly employed the AE as their measure of randomness. Where only a rough comparison is needed, the task of going back to the raw data and calculating the AE seems unnecessarily arduous, and an easy way to estimate this quantity would be a help. The present Note describes a method.*

Handbooks of statistics[2] tell us that for a normal distribution, the average deviation from the mean (AD) is $SD(2/\pi)^{1/2}$. This value is obtained by integrating $y \cdot x$ over one-half of the distribution and dividing by the area under the normal curve for that half. The present method follows the same procedure, except that the averaging is done from the correct value of x rather than from the mean value. Knowing the CE, the difference between the correct value and the mean value of the variable, and knowing the SD (or the SE), we can readily determine the AE for a normal distribution. The SE and the SD are related, as Woodworth reminds us, by $SE^2 = SD^2 + CE^2$.

For convenience in making the derivation, let us express our data in standard scores so that x denotes a deviation from the mean of the distribution divided by the SD. Let x_o denote the difference between the correct value of x and the mean value divided by SD. Then $x_o = CE/SD$. We wish therefore to obtain the average of the deviations around x_o.

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[13] Cf. E. G. Boring, The problem of originality in science, this JOURNAL, 39, 1927, 70-90.

*This work was stimulated by a study of sound-localization conducted under Research and Development Contract NObsr-72627 between the Bureau of Ships and the Defense Research Laboratory of The University of Texas. A brief but frantic search of the easily available literature failed to uncover a method of relating our standard errors to the average errors of earlier investigators. The present paper should help to make the formula more readily available to other workers in psychophysics than it has been (if it exists at all in the literature).

[1] R. S. Woodworth, *Experimental Psychology*, 1938, 396. Woodworth distinguishes between the SD computed from the mean, and the SE, the RMS error computed from the correct value, the 'Standard' of psychophysics, or the 'target.'

[2] See *e.g.* R. S. Burington and D. C. May, *Handbook of Probability and Statistics*, 1953, 84.

For the normal distribution, the ordinate, y, is given by $y = (2\pi)^{-1/2} \exp(-x^2/2)$. The average over part of the distribution is obtained by integrating $y \cdot x$ over that part of the distribution, and dividing by the corresponding area under the curve. For our purpose we must integrate the parts which lie above x_o and those which lie below it separately and add them so that both parts have the positive sign (absolute values). Our expression for AE, also expressed in SDs, becomes

$$AE/SD = \frac{\dfrac{1}{(2\pi)^{1/2}} \displaystyle\int_{-\infty}^{x_o} (x_o - x) e^{-x^2/2} dx + \dfrac{1}{(2\pi)^{1/2}} \displaystyle\int_{x_0}^{\infty} (x - x_o) e^{-x^2/2} dx}{\text{Total area under the normal curve} = 1}$$

$$= \frac{1}{(2\pi)^{1/2}} \int_{-\infty}^{x_o} - x e^{-x^2/2} dx + \frac{1}{(2\pi)^{1/2}} \int_{x_0}^{\infty} x e^{-x^2/2}$$

$$+ \frac{x_o}{(2\pi)^{1/2}} \int_{-\infty}^{x_o} e^{-x^2/2} dx - \frac{x_o}{(2\pi)^{1/2}} \int_{x_0}^{\infty} e^{-x^2/2} dx.$$

The first two terms are of the form $\int e^u du$ and are integrable. The last two terms represent areas under the normal curve and are tabulated in all statistics books. Let us designate those areas for notational convenience as $A_{\infty}{}^{x_o}$ and $A_{x_0}{}^{\infty}$ respectively. Integrating, we have

$$AE/SD = \frac{1}{(2\pi)^{1/2}} \left| e^{-x^2/2} \right|_{-\infty}^{x_o} + \frac{1}{(2\pi)^{1/2}} \left| -e^{-x^2/2} \right|_{x_0}^{\infty} + x_o A_{-\infty}{}^{x_o} - x_o A_{x_0}{}^{\infty}$$

$$= \frac{1}{(2\pi)^{1/2}} e^{-x_o{}^2/2} + \frac{1}{(2\pi)^{1/2}} e^{-x_o{}^2/2} + x_o(A_{-\infty}{}^{x_o} - A_{x_0}{}^{\infty}).$$

The difference of areas in the last term is equal to twice the area between the mean and x_o. We will denote it as $A_o{}^{x_o}$. We have, therefore

$$AE/SD = (2/\pi)^{1/2} \exp(-x^2/2) + 2x_o A_o{}^{x_o}$$

and

$$AE = SD[(2/\pi)^{1/2} \exp(-x^2/2) + 2x_o(A_o{}^{x_o}].$$

To illustrate the formula, let us apply it in the following example:

Error (e)	f	$f \cdot e$	$f \cdot e^2$
+3	1	+3	9
+2	4	+8	16
+1	6	+6	6
0	4	0	0
−1	1	−1	1
Sums	16	18	32
Sum of absolute values		16	

$CE = +16/16 = +1.0$

$AE = 18/16 = 1.125$

$SE^2 = 32/16 = 2.0$

$SD^2 = SE^2 - CE^2 = 1.0$ or $SD = 1.0$

$x_o = 1.0/1.0 = 1.0$ (x_o is always taken as positive)

Estimated $AE = 1.0\ [(2/\pi)^{1/2} \exp(-1/2) + 2 \times 1.0 \times A_o^1]$. Looking in a table of e^{-x}, and in a table of the normal distribution, we have finally,

$$\text{Estimated } AE = 0.7979 \times 0.6065 + 2 \times 0.3413 = 1.17$$

The formula has given a good approximation of the computed value of the AE, even for the rather small number of cases in the example. The approximation would, of course, have been poorer had the distribution of error departed farther than it did from the normal distribution.

The University of Texas LLOYD A. JEFFRESS

EIGHTEENTH ANNUAL MEETING OF THE CANADIAN PSYCHOLOGICAL ASSOCIATION

The eighteenth annual meeting of the Canadian Psychological Association was held in Saskatoon, Saskatchewan, at the University of Saskatchewan June 11-13, 1959. Total registration was 92. Twenty-nine papers, four symposia, two special addresses and the annual business meeting comprised the program. Special addresses were: "Personality correlates of susceptibility to hypnosis," by E. R. Hilgard, Stanford University; and "Experimental psychology and the problem of behavior disorders," by Dalbir Bindra, McGill University, President of the association.

The following officers were elected for the ensuing year: President, Dr. Gordon Turner, University of Western Ontario; and President Elect, Dr. Reg. Bromiley, Defence Research Medical Laboratory, Toronto.

Carleton College, Ottawa F. R. WAKE

THE SIXTEENTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

The Sixteenth International Congress of Psychology, organized under the auspices of the International Union of Scientific Psychology by the German Society of Psychology, is to be held next summer in Bonn, Germany, from Monday, July 31 to Saturday, August 6. Dr. Johannes von Allesch, Professor Emeritus, University of Gottingen, is President of the Congress, and Dr. Friedrich Sanders, Professor of Psychology at the University of Bonn, is Chairman of the General Organization Committee.

Meetings of four kinds are planned:

(1) Symposia on 19 topics divided among 8 fields: Theory and Methods; Perception; Motivation; Developmental Psychology; Social Psychology; Expressive Behavior; Personality; and Animal Psychology;
(2) Colloquia, of which a small number will be lead by invited guests;
(3) Individual papers, not more than 20 minutes in length, on subjects related to the symposia; and
(4) Public lectures, three in number, by invited speakers.

Membership in the Congress will be of two kinds: Active and Associate. Active members must be psychologists or specialists in cognate fields. They will be privileged to participate in all the activities of the Congress and will receive the Congress's publications. Associate members may attend all of the functions of the Congress but they may not participate in the discussions of papers nor receive the publications. Students and recent graduates will, upon their request, be accorded the privileges of the Associate members.

Applications for membership and for position on the program should be sent The Secretary, Psychological Institute, University of Bonn, Am Hof 1 a, Bonn, Germany, before January 1, 1960.

K. M. D.

Edwin Ray Guthrie: 1886-1959

Edwin Ray Guthrie died at the age of 73 years in Seattle, Washington, on April 23, 1959. The immediate cause of death was a heart attack but he had been partially incapacitated by a stroke for over a year prior to his death.[1]

Guthrie's professional career was a varied and distinguished one which culminated as Dean of the Graduate School at the University of Washington. In the course of his career he received numerous professional honors, including being elected to presidency of the American Psychological Association (1945), receiving an LL.D. from his Alma Mater, the University of Nebraska (1945), and winning the Gold Medal Award given by the American Psychological Foundation (1958). He was well known in a wide range of academic circles because of the breadth of his competencies and interests, which led to his contributions in the philosophy of science, learning theory, abnormal psychology, social psychology, and educational psychology. Among psychologists he will doubtless be best remembered for his theory of learning, which is still a modern contender after almost three decades of scrutiny. In the more select population of those who have been his colleagues or students he will be equally remembered as a witty and warm-hearted sage, who was not only wise but also always ready with an apt way of phrasing his wisdom.

Edwin Ray Guthrie was born in Lincoln, Nebraska, on January 9, 1886. His parents were Edwin Ray and Harriet Pickett Guthrie, both of whom had been born in Illinois. Edwin Sr. was a minister's son and operated a piano store in Lincoln when Edwin Jr. was a boy. Harriet Louise Pickett was the daughter of a newspaperman and taught grade school before her marriage. There were five children in the Guthrie household in Lincoln, three boys and two girls; Edwin Jr. was the eldest.

Edwin's scholastic promise was shown early, and by the time he was in the eighth grade he was reading such books as Darwin's *Origin of Species*. His senior thesis in high school, which was critical of attempts by science and religion to arrive at Absolute Truth, was so insightfully written that the principal, who later was to be one of his teachers during graduate study, summoned him for an interview in order to make sure that the essay was in fact the product of a high-school student.[2]

Upon finishing high school in 1903 Edwin entered the University of Nebraska, majoring in mathematics but taking several courses in philosophy as well as the one in general psychology. He was graduated with Phi Beta Kappa honors in 1907 and decided to continue at the University of Nebraska as a graduate student, taking courses in mathematics and psychology but this time majoring in philosophy, a field in which he obtained his master's degree in 1910. During this three-year period he also taught mathematics in a Lincoln high school. From Nebraska Guthrie entered the University of Pennsylvania in 1910 as Harrison Fellow in philosophy, obtaining his Ph.D. under E. A. Singer in 1912 with a thesis in the area of symbolic logic, an argument that "Russell's paradoxes" involved semantic fallacies.

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[1] Acknowledgments should be made to Peter M. Guthrie for help with details of his father's earlier years, only a portion of which could be included here, and to Leonard Carmichael, Ernest R. Hilgard, George P. Horton, and William R. Wilson for personal communications which helped guide this writing.

[2] The high school principal was H. K. Wolfe, who took his Ph.D. with Wundt and was temporarily out of university employment when Edwin was a high-school senior.

Upon receiving his Ph.D. Guthrie did not immediately find an acceptable college or university position and returned to teaching high-school mathematics, this time at Boys Central High School in Philadelphia (1912-1914). In 1914 he was offered an instructorship at the University of Washington in the Department of Philosophy, which was headed by William B. Savery, a former student of William James. In 1919 Guthrie shifted into the department of psychology under Stevenson Smith, where he remained for the rest of his academic career, being promoted to assistant professor in 1920, associate professor in 1926, and professor in 1928. In 1943 he accepted the position of Dean of the Graduate School, which he held until he achieved emeritus status in 1951. During the latter half of this period he also served as Executive Officer in charge of academic personnel (1947-1951). As Emeritus Dean and Emeritus Professor he participated in teaching and other academic affairs until his final retirement in 1956.

Two excursions outside the academic realm, in each case occasioned by war, played significant roles in his career. During the first World War he was a second lieutenant in the army, serving at Officer Training Schools in both Infantry and Artillery, an experience which he says turned his attention from books to men.[3] During the second World War he served in Washington, D.C., as a high-ranking civilian consultant to the War Department, first in Military Intelligence (1941-1942) and later in the Office of War Information (1942-1943). The emphasis in these positions was on propaganda, and as a result Guthrie became more of a social psychologist. He also became a more widely known psychologist due to his membership in the sizable 'in-group' of behavior scientists who converged on Washington, D.C. in connection with the war effort.

As is apparent in this brief account of Guthrie's education and career, he did not follow a conventional route to distinction in psychology. His early inclinations were toward philosophy despite a fair exposure to psychology. His potential interest in psychology was weakened at Nebraska by a laboratory course in psychophysics. It was revived somewhat under the influence of Singer at Pennsylvania, and by Singer's address before The American Philosophical Association at Princeton in 1910 on "Mind as an observable object"[4] which was described by Guthrie as "the most stirring event of my academic life."[5] While this address by a philosopher probably converted Guthrie to a behavioristic point of view, it did not result in his immediate conversion to psychology. The titles of his early papers suggest only a philosopher specializing in logic, as for example: "Formal logic and logical form" (1914), "Russell's theory of types" (1915), "The field of logic" (1916).[6]

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[3] E. R. Guthrie, Association by contingunity, in Sigmund Koch et al., *Psychology: A Study of a Science*, 2, 1959, 158-195.

[4] E. A. Singer, Mind as an observable object, *J. Phil., Psychol., & Sci. Methods*, 8, 1911, 180-186.

Guthrie was 33 years old before his full-fledged conversion to psychology, which can be dated from 1919 when he started collaboration with Stevenson Smith on a textbook which was first published locally as *Chapters in General Psychology* but was later revised and published nationally as *General Psychology in Terms of Behavior* (1921).[7] Of this venture Guthrie says:

> [It] proved to be invaluable training because it represented from four to five hours of argument and writing usually seven evenings a week over a period of a year. Smith had had far more training in psychology and the experience of clinical work with children for six years. Our diverse backgrounds, one with a degree in philosophy and mathematics and the other with a degree in psychology and neurology, made almost every paragraph of the book being written the occasion for an argument.[8]

The product of these efforts was a small classic in its simplicity, its logical consistency, and the scope of the topic to which it applied the behavioral approach. Because of its treatment of the conditioned response as the basic unit of learning, including Thorndikian learning which was deduced from conditioning by contiguity, it established the authors as important contenders for a unified theory of learning.

As an aside on this 'conversion' to psychology we may note that the character of the book with Smith was in complete harmony with Guthrie's earlier enthusiasm for Singer's "mind as behavior." Guthrie's epistemological studies moreover, led him to view logic and philosophy in a critical and skeptical vein and made him quite ready to turn his enthusiasm into psychology. At no time, however, did he completely abandon his initial philosophical orientation, which shows up in his conceptions of the nature of science in parts of his major writings and in the titles of such papers as: "Purpose and mechanism in psychology" (1924, "On the nature of psychological explanations" (1933), and his presidential address, before the American Psychological Association, "Psychological Facts and Psychological theory" (1946).[9]

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[5] Guthrie, *The Psychology of Learning*, 1935, vii.

[6] Guthrie, Formal logic and logical form, *Midwest Quart.*, 1, 1914, 146-155; Russell's theory of types, *J. Phil., Psychol., & Sci. Methods*, 12, 1915, 381-385; The field of logic, *ibid.*, 13, 1916, 152-158.

[7] Stevenson Smith and E. R. Guthrie, *General Psychology in Terms of Behavior*, 1921.

[8] *Op. cit.*, in Koch et al., *Psychology: A Study of a Science*, 2, 1959, 161.

For several years after the book with Smith, Guthrie made no new contributions to learning theory, but soon after publication of Anrep's translation of Pavlov's *Conditioned Reflexes* he wrote one of his most significant theoretical papers: "Conditioning as a principle of learning" (1930).[10] This paper is perhaps the most outstanding single example of Guthrie's theoretical talents, and in it he embodied most of the ideas which later became known as "Guthrie's Theory." The article soon came to the attention of the profession because it was attacked by Pavlov in 1932 in the only paper Pavlov ever published in an American psychological journal.[11] In the exchange with Pavlov, which included a rebuttal by Guthrie (1934),[12] the difference between the two men is quite apparent. Pavlov tended to interpret each of his various phenomena as reflecting a separate property of the central nervous system, whereas Guthrie saw them as reflecting various arrangements of stimulus-conditions, as using only one critical property of the nervous system, conditioning by contiguity.

The decade from 1930 to 1940 was a very productive period for Guthrie. In addition to the two papers just cited he published a paper taking exception to the idea that associative strength depends on the time-interval between stimuli (1933).[13] He presented his views on the law of effect (1934);[14] published his major work, "The Psychology of Learning" (1935);[15] clarified his theoretical position with regard to the function of rewards (1939);[16] and made his insightful distinction between 'acts' and 'movements' in the course of stating his position on the emerging controversy over single *versus* dual process views of learning (1940).[17] These seven works contain essentially all of Guthrie' theory from the standpoint of the assumptions, hypotheses, and major deductions involved; his subsequent publications dealing with the theory have served primarily to clarify or to apply in new contexts. Notable among these clarifications and applications are his chapter in the 1942 *Educational Yearbook*,[18] the 1952 revision of his *The Psychology of Learning,* and his chapter in Koch's *Psychology: A Study of a Science.*

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[9] Purpose and mechanism in psychology, *J. Phil.*, 21, 1924, 673-681; On the nature of psychological explanations, *Psychol. Rev.*, 40, 1933, 124-137; Psychological facts and psychological theory, *Psychol. Bull.*, 43, 1946, 1-20.

[10] *Op. cit.*, *Psychol. Rev.*, 37, 1930, 412-428.

[11] I. P. Pavlov, The reply of a physiologist to psychologists, *Psychol. Rev.*, 39, 1932, 91-126.

[12] Pavlov's theory of conditioning, *Psychol. Rev.*, 41, 1934, 199-206.

[13] Association as a function of the time-interval, *Psychol. Rev.*, 40, 1933, 315-368.

[14] Reward and punishment, *Psychol. Rev.*, 41, 1934, 450-460.

[15] *Op. cit.*, 1935, 1-258.

[16] Effect of outcome on learning, *Psychol. Rev.*, 46, 1939, 480-485.

[17] Association and the law of effect, *Psychol. Rev.*, 47, 1940, 127-148.

During this same ten-year period Guthrie also established a reputation in abnormal psychology by the publication of his book, *The Psychology of Human Conflict* (1938).[19] This departure into a different field may have been a surprise to the profession but was completely in character with his various teaching roles at the University of Washington, where he taught at the time a course entitled "The Psychology of Adjustment." The only prior hints in the literature that he had views on personality or behavior disorders appeared in a collaborative paper with Stevenson Smith on the topic of exhibitionism[20] and a study of the validity of the concepts of 'extroversion' and 'introversion.'[21] Guthrie's book reflected his respect for the ideas of Pierre Janet, which had already been shown by his translation—in collaboration with his wife, Helen Macdonald Guthrie—of Janet's *Principles of Psychotherapy,*[22] and which was still in evidence in his memorial article on the occasion of Janet's death in 1947.[23] The book was not too well received in the some quarters, perhaps because it favored Janet over Freud and took a generally critical attitude toward psychoanalysis. Nevertheless it established Guthrie as one of the clinical theorists and was responsible for his being invited to write a theoretical chapter in Hunt's volumes on *Personality and the Behavior Disorders* (1944).[24] The book also had some significance in amplifying his learning theory, partly because of the inductive force of the numerous specific applications and partly because it enlarged the concept of "emotional reinforcement of action," which appears in several places in Smith and Guthrie and is the basis for a short chapter in *The Psychology of Learning.*

Another product of the fruitful 1930-1940 period was the collaborative study, with George P. Horton, of the stereotyped behavior of cats in the puzzle box. Except for this study Guthrie's contributions were almost exclusively in the realm of theory rather than research. He was the chief American exponent of the conditioned response as the basis of all learning, but he never conducted a conditioning study, being content mainly to interpret the findings of others.[25] The project with Horton was carried out during the period from 1936 to 1939, at a time when Guthrie was past 50 but in what was perhaps the busiest period of his life. His late afternoons in the department's fourth-floor vivarium, taking notes in shorthand while Horton set up the trials and supervised the photographing of the cats' responses, were an inspiring feature of the department to the graduate students of that era. The findings lent fairly dramatic support to some of Guthrie's theoretical contentions, and although the results were not published fully until 1946,[26] they became fairly well known much earlier from preliminary reports, and a movie made available through the Psychological Cinema Register.[27]

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[18] *Year Book nat. Soc. Stud. Educ.,* 41, 1942 (Part 2), 17-60.

[19] *Op. cit.,* 1938, 1-408.

[20] Smith and Guthrie, Exhibitionism, *J. abn. soc. Psychol.,* 17, 1922, 206-209.

[21] Measuring introversion and extroversion, *ibid.,* 22, 1927, 82-88.

[22] Pierre Janet, *Principles of Psychotherapy,* trans. H. M. and E. R. Guthrie, 1924, 1-314.

[23] Guthrie, Pierre Janet: 1859-1947, *Psychol. Rev.,* 55, 1946, 66 f.

[24] Personality in terms of associative learning, in J. McV. Hunt et al., *Personality and the Behavior Disorders,* 1, 1944, 19-68.

Guthrie's style as a scientific writer set him considerably apart from his peers in learning theory, chiefly because of his avoidance of technical language, his use of anecdotal documentation, and his penchant for the interjection of an irreverent brand of humor which he used with matchless artistry to illuminate his ideas. His books had the student audience at least as much in mind as the professional audience and were written on the assumption that comprehension would be fostered by presenting the theory in the context of interesting or amusing meaningful illustrations. His major book contains no tables of supporting data even when it cites supporting studies, but it contains a number of examples with obvious undergraduate appeal, such as the case of the boys who "retrained" their pastor's horse by shouting "whoa" just before a jab from behind with a hay fork.[28]

Guthrie's use of humor was not confined to books designed for student consumption. His outlook was always affected by an appreciation of comedy, a fact apparent even in his professional papers, where often he made a serious point by a humorous analogy. For example, in his important article on *Conditioning as a Principle of Learning* he counters the Gestalt position on patterning by noting that the child who has learned to read the raised letters on his blocks will not ordinarily recognize the patterns when he sits on them,[29] and he illuminates his statistical deduction of the shape of the forgetting curve by the analogy of:

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[25] Guthrie did, of course, serve as major advisor on several master's theses concerned with conditioning. In one of these (G. K. Yacorzinski and E. R. Guthrie, A comparative study of involuntary and voluntary conditioned responses, *J. gen. Psychol.,* 16, 1937, 235-257) the candidate named Guthrie as co-author. More conventionally the advisor was acknowledged only in a footnote.

[26] E. R. Guthrie and G. P. Horton, *Cats in a Puzzle Box,* 1946.

[27] Cf. *Psychol. Bull.,* 34, 1937, 774; 36, 1939, 521; *Psychol. Cinema Register,* 1938.

[28] *Psychology of Human Learning,* 1935, 48.

> The decreased expenditures of a certain artist whose method of protecting himself from starvation was to change the proceeds of his rare sales into dimes and broadcast these about his large and disordered studio. The following day dimes were retrieved easily in numbers. As time went on more and more search was required, though he seldom reached such a pass that an afternoon's search would not yield a dime.[30]

The anecdotal style for which Guthrie was sometimes criticized in his writings had free reign in the classroom, and as a teacher he rarely documented a proposition with a research finding if he knew of an interesting and convincing example from authentic experience. From Guthrie, and from the solid Guthrian front presented for many years in most of the courses in psychology at the University of Washington, the majors in psychology acquired a systematic position, which, right or wrong, had the advantage and the attendant conviction of providing them with a basis for making fairly ready and consistent interpretations of behavioral events.

To those who have not been exposed to this full Guthrian treatment his 'theory' may seem like hardly a theory at all. Guthrie himself contributed to this impression by sometimes stating publicly that he did not have a theory but only a "point of view." His avoidance of formalized theory was apparently deliberate, motivated not only by a general skepticism of the ultimate validity of deductively proven 'truths' but also by a belief that construction of formalized scientific systems hampered the search for new knowledge.[31] He nevertheless produced an informal system with fairly specific assumptions—and a fairly large number of novel deductions. When, for example, he made the deduction that conditioning in a variable environment will progress slowly compared to that in a stable environment, because of the need to connect the response to the varied forms of the neutral-cue pattern, he added a theorem to his theory, and not only provided an alternative to the hypothesis that repetition per se accounts for gradual acquisition but also suggested a factor influencing the shape of the acquisition curve. His extension of this deduction to the radical assumption of one-trial learning was not a necessary part of his original inference, althought it opened the way for the formal elaboration of Guthrie found in Estes' statistical model.[32]

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[29] *Op. cit.*, 426. [30] *Ibid.*, 422. [31] Cf. Koch et al., *op. cit.*, 173, 193.

[32] W. K. Estes, The statistical approach to learning theory, in Sigmund Koch et al., *op. cit.*, 380-491.

Guthrie's theoretical position was marked by an insistent peripheralism. This was partly a reflection of his belief that a science could be made only out of observables, a belief which was accompanied by lack of faith in the utility of hypothetical constructs, especially those with a pseudo-neurological flavor, as in the case of Pavlov, or a mentalistic flavor, as in the case of Tolman, whose system Guthrie summed up as leaving the rat "buried in thought" with no basis for translating central affairs into overt action.[33] Guthrie's peripheralism was also a reflection of a perhaps more fundamental opinion that covert muscular movements and their proprioceptive feed-back were the basis for perception and thought. This anti-central and anti-construct bias was not, however a necessary aspect of most of his associationistic theory. The hidden strength of his theory is that its implications are chiefly derived from only one assumption: that contiguity in time is a critical factor in associative learning whether or not it is sufficient. This 'assumption' is an almost self-evident fact, and any of its logical implications have little chance of future refutation whether they are applied to motor habits, to perceptual learning, or to the 'association of ideas.'

After 1940 Guthrie's activities were dominated by his duties outside the realm of academic psychology, in connection first with his participation in the war effort and second in his roles as Dean of the Graduate School and Executive Officer in charge of academic personnel at the University of Washington.

Guthrie's initial wartime appointment was indicative of his national reputation at the time. He was recommended for the position by Leonard Carmichael and Walter Hunter, who comprised a committee representing the NRC in conferences with the war department on the topic of "psychological warfare." Their recommendation of Guthrie as consultant to Military Intelligence was based on their decision that what was needed was not a specialist but rather "a wise, sensible man with a knowledge of learning theory and motivation."[34] An important Guthrian contribution during the war was the instigation of the questionary methods of assessing soldier morale in American troops. This 'attitude' research was initiated in 1941 by Guthrie's agency in Military Intelligence, his chief collaborator within his agency being Allen L. Edwards. The initiation of such research led to the creation of a separate organization headed by Samuel A. Stouffer with a staff of sociologists, anthropologists, and psychologists, who had the chief mission of reporting on the morale of American troops. Thus Guthrie had at least an avuncular role in the well-known work of the "Research Branch."[35]

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[33] *The Psychology of Learning,* 1935, 172.
[34] Personal communication from Leonard Carmichael.

Guthrie's interests turned to educational affairs in his positions as Dean of the Graduate School and Executive Officer in charge of academic personnel at the University of Washington. A major publication during this period was *Educational Psychology,* a collaborative book with F. F. Powers of the School of Education, which treated educational psychology in the context of Guthrie's theory of learning.[36] A number of other publications relevant to education during this period were concerned with his long standing interest in the evaluation of teaching ability. He published on this subject early in his career as a psychologist,[37] and one of his last publications, *The State University: Its Function and Its Future* (1959), is partly concerned with this same issue.[38]

Guthrie's role as a University administrator during this period of his life is not relevant to a consideration of his contributions to the field of psychology, but he undoubtedly did some service to his profession in bringing to it the prestige of his high status in a large university, a university which recognized Guthrie—as well as in other ways—by naming a building in his honor after his final retirement in 1956. Edwin Ray Guthrie Hall at the University of Washington stands, appropriately enough, contiguous with the building which had been named several years previously in honor of William (Briggs) Savery, the philosopher who hired Guthrie as an instructor in 1914. Guthrie's recognition in his local community is well expressed in the following excerpt from the editorial page of *The Seattle Times* on the day after Guthrie's death:

> He will be long remembered by his faculty associates and by the thousands of students who, during his long service at The University of Washington, were inspired by his personal example, his fruitful scholarship, and his wise and friendly counsel.

Yale University FRED. D. SHEFFIELD

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[35] *Studies in the Social Psychology of World War II,* auspices of Soc. Sci. Res. Counc., 4 vols., 1949-1950. The reference is to the various chapters by S. A. Stouffer in the different volumes.

[36] Guthrie and Powers, *Educational Psychology,* 1950, 1-530.

[37] Measuring student opinion of teachers, *School & Soc.,* 25, 1927, 175-176.

[38] *The State University: Its Function and Its Future,* 1959.

BOOK REVIEWS

Edited by GEORGE L. KREEZER, Washington University

The Polish Peasant in Europe and America. By WILLIAM I. THOMAS and FLORIAN ZNANIECKI. New York, Dover Publications, Inc., 1958. Vol. I, pp. xv, 1-1116; Vol. II, pp. vi, 1117-2250, $12.50.

This major contribution to the literature of sociological theory and methodology is now made available in a moderately priced republication of the second revised edition. For twenty-five years the work has been out of print and accessible to students only in some libraries and occasionally on the rare book market at several times the price being asked for this edition. The work of two of the foremost sociologists, this monograph in two huge volumes is, in the estimation of the reviewer, the outstanding sociological classic of the twentieth century. It is a 'must' for all scholars who work in the behavioral sciences.

The monograph is opened with an 86-page methodological note in which the authors state their basic assumptions, the procedures followed, the fallacies of common-sense sociology, and the criteria by which data are selected and judged. The note has long been regarded as a brilliant discussion of sociological method and theory. It introduces a number of concepts which have become valuable tools of sociological analysis. For example, nowhere in the literature does one find a comparable treatment of the interrelatedness of individual attitudes and social values.

The authors follow up the note with a lengthy introduction to such facets of Polish peasant society and culture in the old country and in America as the great family structure, sexuality and marriage, the class system, the typical stages of economic development, the bifurcated social milieu, the nature and place of religious and magical attitudes, and theoretic and esthetic interests. Here the reader is given a picture of an essentially static folk culture being subjected to the impact of social forces making for changings which, in turn, make for social disorganization, the radical alteration of attitudes and values, and the demoralization of the individual immigrant.

Perhaps the most remarkable feature of the work is the extensive and effective use of documents—letters, case studies, and the autobiographical life-history of a young Polish immigrant. Some fifty series of letters, numbering over 700 in all, comprise the last 800 pages or so of the first volume. These letters are broken down into four categories—correspondence between members of family groups, between husbands and wives, between persons outside the kinship relation, and individual letters and fragments of letters showing the deterioration of family solidarity. The series touch on many subjects and are written by nearly illiterate peasants, workers, middle-class trades people, and by near intellectuals. The collection is unique and provides a penetrating insight into the variety of social forces, such as urbanization, education, military service, labor mobility, revolution, impending war, and similar factors, which impinge upon family, community, and the individual to alter the original Polish culture and to cause reinterpretations, new alignments, new needs, and new ways of wish satisfaction to emerge.

Volume II consists of three parts of the monograph: Part II, Disorganization and Reorganization in Poland; Part III, Organization and Disorganization in America; and Part IV, Life-Record of an Immigrant. The concepts of social disorganization and social reorganization get one of the best analyses to be found anywhere. Similarly, personal organization and personal disorganization are expertly handled. It is in connection with these discussions of the broken family, the decay of communal solidarity, the rise of class and religious revolutionism, *anomie* and personal demoralization, economic dependency, disruption of the conjugal relation, criminality, vagabondage, sexual immorality, and the efforts at the amelioration and correction of these problems in the old country and in America that the authors introduce more than 100 primary source documents: court records, newspaper articles, parish reports, cultural surveys, and documentary materials. With documentation of cases, the writers picture the struggle of communities, groups, and individuals to meet new opportunities and challenges and explain how and why they so often fail. The long and difficult passage to assimilation for the immigrant into a new environment is told with telling effect.

An introduction of some 80 pages prefaces the final document—the autobiography of a Polish worker. This introduction ranks with the methodological note in Volume I as among the sociological classics of our time. As the authors say, the problem of this part of the monograph is "the application of the methods of social psychology to an evolving personality." It is here that the merits of the life-history document in socio-psychological research are elucidated and illustrated. It is here also that one finds a thorough and insightful discussion of the typology of personality development; the authors' contribution of the typical-lines-of-genesis concept; the rather ambiguous and dubious term *temperament* (as defined by Thomas and Znaniecki) and its relation to character development; the importance of the definition of the situation; the postulated Philistine, Bohemian, and creative man forms of personal determination toward which social personalities tend to evolve, with an indefinite number of variations, of course; and a reformulation of the wish concept and its four generalized forms. The latter portion of the introduction carries the discussion of personality development through a set of four parallel and interdependent processes: (1) Determination of the character on the ground of the temperament; (2) Constitution of a life-organization which permits a more or less complete objective expression of the various attitudes included in the character; (3) Adaptation of the character to social demands put upon the personality; and (4) Adaptation of individual life-organization to social organization.

Wladek's autobiography is, in the words of the authors, an "exclusively scientific, not historical" document. The personality of Wladek is "entirely insignificant from the point of view of the cultural development of Polish society, since he is a typical representative of the culturally passive mass." This self-revelatory life-history of 312 pages is vivid, detailed, and absorbing in the interest it holds for the general reader as well as for the student. It is this document, taken with the introduction to it, which will have the greatest importance for the social psychologist.

The University of Texas WARNER E. GETTYS

Rorschach Responses of Elementary School Children. By NETTIE H. LEDWITH. Pittsburgh, University of Pittsburgh Press, 1959. Pp. x, 185, $4.00.

Over a period of six years beginning in 1946, the author collected nearly 1000 Rorschach protocols on young children in a carefully designed study aimed at providing normative data on the test. This monograph contains a description of the study, together with extensive tables that other Rorschach workers can use in their own research or clinical practice with children. Unlike most other attempts to provide normative Rorschach data on specified populations, this one is based upon rigorous sampling methods and adequate control groups to examine the effects of repeated testing upon performance.

One hundred and sixty six-year-olds from 30 different schools in Pittsburgh were given the Rorschach test and the Revised Stanford-Binet Examination, Form L, by the author. Special care was taken to standardize the method of administration and scoring and to gain effective rapport with the child. The Rorschach was repeated annually for six years, each time presented with a different test of intelligence or related mental function. During the six-year period the original sample shrank only slightly and no child refused to coöperate, yielding a total of 138 cases—68 boys and 70 girls—for each of whom there were six complete sets of test-data. The success of the sampling and subsequent testing is attested to by the striking agreement between the distribution of *IQ*s obtained from the Stanford-Binet and the test-norms for urban children published by Terman and Merrill. In addition to the longitudinal sample, Ledwith obtained control groups of 73 cases at age 7 yr. and 80 cases at age 11 yr.

Most of the normative data are reported for sex and age-level, using scores very similar to those suggested by Klopfer. An attempt was made to control for the confounding effect of the varying number of responses (R) upon other variables by converting most raw scores into percentages, a technique far from satisfactory because of the complex, curvilinear relation between R and most other scores. In addition, a card-by-card summary of content categories is given with norms for children's "Populars." A somewhat cursory comparison is made between the children's norms and the rough adult 'norms' suggested by Klopfer, revealing a general tendency for the later age-levels to approach the adult figures. Most Rorschach workers will find the tables and sample case presentations simple and straight-forward to apply.

In view of the care that went into the sampling and testing of children, it is disappointing to find no statistical analysis beyond some group comparisons and simple normative tables. No attempt is made to investigate the nature of intra-individual variability through time, a crucial question for which Ledwith's data could provide an answer. Nor has the author presented any findings of relationships between the Rorschach scores and any other test data or personality assessments. It is to be hoped that these other analyses will be completed and published soon.

The University of Texas WAYNE H. HOLTZMAN

Water Witching U.S.A. By EVON Z. VOGT and RAY HYMAN. Chicago, University of Chicago Press, 1959. Pp. xi, 248, $4.95.

Few topics, especially in rural America, arouse more impassioned arguments than 'water witching.' The authors of *Water Witching U.S.A.* do not, however, take sides; they treat the subject soberly and sympathetically as one worthy of consideration and they follow their evidence to its logical conclusion. They are well equipped by experience and training to do this. Dr. Vogt is an anthropologist, who had previously done field work in water witching, and Dr. Hyman is a psychologist, who had previously been a professional magician.

Their book is divided into 9 chapters, 2 appendices, and a bibliography of 114 titles. In Chapter 1, entitled "Why Water Witching?," they consider, by reference to case records, why some people, in this day and age of scientific and rational thinking, turn in their need for water to the witcher. They also, in this chapter, outline their book, which is organized around six questions: (1) What is water witching? (2) Does it work? (3) What causes the rod to move? (4) Who is the water witcher? (5) Under what conditions is it practiced in the U.S.A.? (6) Why does it continue to be practiced there?

The first three questions are answered for the most part from their review and analysis of the literature on the subject. Question (1) is considered in Chapter 2, in which the history of witching is recounted and the various methods currently used in America are described. Question (2) is answered in Chapters 3 and 4 which deal, respectively, with (a) the evidence gained from case histories and field tests and (b) from controlled experiments. The evidence from both sources is, as they find, negative, *i.e.* no better than chance. Question (3) is explained in Chapters 5 and 6. Beginning with Clever Hans, the first 'talking horse,' many esoteric phenomena (mind reading, the magic pendulum, ouija board, tapping and turning tables, automatic writing, etc.) are examined and shown to be similar in kind to the diviner's movement of his rod.

The last three questions are answered, in Chapters 7-9, from the results of the authors' investigations which include field studies, interviews with dowsers, and returns from a questionary sent to 500 agricultural extension agents in the rural counties of the U.S.A.

Their results and conclusions, which are statistically supported, are not new; they are in complete agreement with scientific thought of today. The collection and presentation of their evidence mark their contribution to the explanation of this ancient folkway.

The questionary used in the study, together with its accompanying letter, is given in Appendix I. In Appendix II, Dr. H. E. Thomas, of the U.S. Geological Survey, describes the scientific methods of locating water wells.

The book is illustrated by 23 cuts which are, much to the inconvenience of its readers, printed on an unnumbered 8-page form and inserted between pages 52 and 53. It lacks an index; readers, who wish to use the book for reference, must prepare their own.

K.M.D.

Jung and the Problem of Evil. By H. L. PHILP. New York, Robert M. McBride Company, 1959. Pp. xiii, 271. $4.95.

Dr. Philp, of London, England, a theologian as well as a psychologist, embarks in this book on a study of Jung's works in a search of implications for Christianity. In his earlier book, *Freud and Religious Belief,* 1956, he had made a similar study of Freud's work. His conclusions in that book were that lack of sophistication in theology limited the value of Freud's "atheistic pronouncements" to the conceptions of an "amateur." He comes to similar conclusions in the present book regarding Jung.

In addition to Jung's works, the source materials of the present book are written 'question-and-answer' exchanges between the author and Jung. The Introduction, Part I, and Part III rest on Philp's correspondence with Jung concerning the writing of the book and his critical comments regarding Jung's answers to five challenging questions, viz. Truth, the unconscious, God, the psyche, and the significance of evil. The last topic covered the concept of *privatio boni,* Satan, the Fall, and the meaning of suffering. Part II is primarily concerned with a critical analysis of Jung's *Answer to Job.*

Philp criticizes Jung for venturing into fields other than psychology and strongly disapproves of what he believes to be Jung's confusion of empirical psychology and religion. He found that Jung, even to direct questions, would not give sufficiently elaborated replies and that, at times when he attempted to pin Jung down, the issue was frequently clouded by invalid 'theological arguments.' Throughout the book the incompatibility of their frames of reference was in evidence. Jung at times seemed to communicate in terms of a restricted empirical psychology and, at other times, in terms of a transcendental *Weltanschauung.*

Despite these obvious imperfections, the reviewer recommends this book as it gives its readers an opportunity of evaluating Jung's theory of personality from his responses to specific questions. The sarcastic tone of some of Jung's answer to Philp's questions not only lends spice to the material, but it may, as the reviewer suspects, be responsible for Philp's refusal to permit Jung to see and to comment upon the completed manuscript.

The book ends with a spotty glossary in which "Active imagination"—which conspicuously characterizes Jung's behavior, is defined as "intense concentration on the background of consciousness; a technique that is perfected only after long practice" (256). Indeed, Jung has had long practice and the reviewer is amazed to discover how this 84-yr.-old man not only retains a vital interest in his work but continues to do so with such rare vigor and enthusiasm.

University of Houston RICHARD I. EVANS

The Mammalian Cerebral Cortex. By B. DELISLE BURNS, London, Edward Arnold, Ltd., 1958. $5.00.

This concise and compact volume (107 pages with more than 200 references) will be of value, not only to physiological psychologists, but to all who are interested in the relationship of the nervous system to behavior. Burns' book indicates that although neurophysiologists have not ventured as far into the 'stream of consciousness' as psychologists, their footing is more secure.

One especially enlightening chapter, which includes the author's own research, is concerned with *properties of the isolated cerebral cortex.* In this chapter, Burns describes and illustrates the interesting phenomena observed when a slab of isolated but living cortex is stimulated under a variety of conditions.

Another chapter deals with *after discharges.* These, some believe, may be the neuroelectrical building blocks essential to higher mental processes, such as learning and memory. Different types of after-discharge phenomena, such as self-recruiting circuits, persistence of humoral transmitters, and after-discharges due to differential repolarization are clearly described and illustrated.

On the topic of spontaneous activity of central neurones, the author points out that as yet no one can state, with assurance, that the cells of the cerebral cortex have the power of spontaneous activity. The final two chapters are devoted to the problems of transmission of excitation within the cerebral cortex and memory. The latter chapter, on memory, will probably be of most interest to psychologists. It contains much material, and might-well have been expanded.

V. A. Hospital
Salt Lake City

EDWARD C. BECK

Common Sense about Gifted Children. By WILLARD ABRAHAM. New York, Harper and Brothers, 1958. Pp. 1-268. $5.00.

Dr. Abraham's book contains a wealth of information about gifted children and of their needs at different developmental periods. After reporting the results of genetic studies of gifted children he reviews the pioneering work done on them and discusses the resources and opportunities outside the home and school that may be used to advance their optimal development.

He clearly sets forth the parents' responsibility for recognizing the unique nature of their gifted child and for guiding him during the preschool years. In his discussion of the characteristics of gifted children, Dr. Abraham suggests ways of meeting their needs and of fostering their continuous growth. He describes the educational plans and methods used, *e.g.* the special school, the special class, partial segregation, specialized highschools; and discusses the use of acceleration and the practice vaguely designated as 'enrichment.' He also treats of the characteristics of the effective teacher.

Dr. Abraham's treatment is objective and impartial. He avoids the pitfall of recommending any particular method. He writes: "No single method of teaching is necessarily best for all gifted children. No particular school or classroom organization will meet all their needs. No clearcut personality type makes the best teacher for the gifted. No simple solution suits all schools or communities" (p. 67).

In the opinion of the reviewer Dr. Abraham's book is one of the best in the field of the gifted child. It is a good book for psychologists to have at hand to recommend to those seeking information in this field.

Northwestern University

PAUL A. WITTY

Mental Subnormality. By RICHARD L. MASLAND, SEYMOUR B. SARASON, and THOMAS GLADWIN. New York, Basic Books, Inc., 1958. Pp. 400. $6.75.

Mental Subnormality attempts to overcome fixation on the stupidity of the retarded and to look more broadly upon the personality and dynamics of the mentally deficient. This book is the result of a multidisciplinary search for pertinent variables associated with the etiology of the condition.

The reader is taken on a detailed review of the mentally subnormal by a physician, a psychologist, and an anthropologist. A total of 631 studies are reviewed. Masland, writing on biological factors, devotes more attention to current research than do Sarason and Gladwin who treat, respectively, of the psychological and cultural problems and devote more space to the well known, classical researches—probably on the grounds that they are basic to their surveys.

The authors are thorough in their reviews and in the topics they discuss which range from regeneration of the nervous system to the question of why psychological testing is unproductive in understanding mental subnormality. Even the data of Hiroshima and Nagasaki on the nature of gene action are reported.

The general conclusion of the book is that biological factors underlying mental deficiency are little understood today; that these factors offer a rich field of study; and that the cultural determinants of mental subnormality have not been given the importance they deserve. This book is a scholarly contribution to the field of abnormal psychology which students in this field should read.

The University of Texas — Austin E. Grigg

Emotional Problems of Adolescents. By J. Roswell Gallagher and Herbert I. Harris. New York, Oxford University Press, 1958. Pp. xii, 174, $3.50.

This book was written by two physicians at a non-technical level for use by those who have frequent, continuing contact with adolescents. It is an excellent piece of work: its style is clear; its breadth of coverage is broad. Anyone who has dealt with adolescents—parents, teachers, psychologists, psychiatrists—will immediately appreciate the thought and planning which have gone into this small volume, for the authors have managed to describe and elaborate those troublesome problems which are most often encountered in this age group. Thus there are excellent statements regarding parent and sibling relationships, academic underachievement, sex development, anti-social behavior, psychosomatic disorders, etc. The emphasis is never focused on the symptom, but always on the adolescent, with the symptoms being seen as the product of a disturbance in interpersonal relationships.

This volume will be a welcome addition to the all-too-limited list of volumes treating of the adolescent. It will be especially useful to those professional workers who like to have parents read helpful material about teenagers.

More important, this work will be valuable if applied at a preventive level, for its wisdom can help show the way to healthy development of young people. The authors repeatedly emphasize that understanding the adolescent implies an awareness of growth and changing needs. "Adolescence is not static; it is a process."

University of Miami — Jess Spirer

Psychology as Applied to Nursing. By Andrew McGhie. Edinburgh, E. & S. Livingstone, 1959. Pp. xii, 247, $4.50.

This book is primarily of interest to psychologists teaching nurses. McGhie organized his material logically. He traces briefly the development of personality from infancy through childhood, adolescence, adulthood, and old age; then, in turn, discusses human motivation (which he deals with, in terms of instincts, because of their historical interest), attitudes, opinions, unconscious motivations, and defense mechanisms; the relationship between heredity and environment; sketches interactions with the environment through attending, perceiving, learning, remembering, and thinking; and reviews variables of group behavior in identification, communication, morale, and leadership.

The author utilizes recent experimental reports (*e.g.* the sensory isolation studies of Hebb and his group, Wexler and Solomon and their co-workers, and Lilly), spans the range of psychological thinking from McDougall's instincts through Kallman's genetic investigations (including Freudian psychoanalytic theory, Piaget's studies of child language and thinking), and reviews recent studies of the hospital (Stanton and Schwartz) as a social structure. Though there are instances of moralizing and preaching, McGhie does well in applying psychological knowledge to the training and understanding of the student nurse in her own development and in her interactions with patients, peers, and supervisory personnel.

University of California, Los Angeles IVAN N. MENSH

The Gifted Group at Mid-Life. By LEWIS M. TERMAN and MELITA H. ODEN. Stanford, California, Stanford University Press, 1959. Pp. xii, 187. $4.50.

This book, Volume V of Terman's *Genetic Studies of Genius,* and a follow-up after more than 30 yr. of the first study, gives a fascinating picture of the possible outcome of the longitudinal study of human beings. The superior adult generally becomes the able adult, but the real test of mental ability is its use. Good as the intelligence tests are, they do not predict direction, nor level of attainment. The later comparison of those who did and those who did not achieve up to their presumed potential point to the importance of personality factors, especially persistence in working toward goals, self-confidence, and freedom from feelings of inferiority.

Despite the importance of the large body of substantive data reported here and in the earlier volumes and journal reports, it may turn out that the by-products of Terman's study will be just as important—including not only the normative information on children's health, sleep, reading, and interests and adults' marriage attitudes, but also the tests devised to measure all the attributes of child development which Terman thought relevant.

St. Louis University WALTER L. WILKINS

Trends in Gerontology. By NATHAN W. SHOCK. Second edition. Stanford, California, Stanford University Press, 1957. Pp. x, 214. $4.50.

While written primarily for the lay reader, this book is of value to the specialists as well since it, in addition to reporting the results of current researches, stimulates new developments and fields of activity. Dr. Shock, a physiological psychologist, head of the Gerontology Branch of the National Heart Institute, USPHS—the most important research organization in the field of aging—regards gerontology as a global, multi-disciplinary subject. The wide field of aging, with chapters on demography, employment, retirement, maintenance of income and health, education, social welfare, and research, is accordingly covered in this book.

Psychologists will in particular be interested in the three chapters on research which deal, respectively, with trends, potentials, and recommendations. The ideal Institute of Gerontology, according to Dr. Shock, should be multidimensional, *i.e.* devoted to research, teaching, and the rehabilitation and care of the aged.

Washington University ROBERT W. KLEEMEIER

Loathsome Women. By LEOPOLD STEIN with Martha Alexander. New York, McGraw-Hill Book Co., 1959. Pp. ix, 243. $4.50.

Dr. Stein, former chairman of the Society of Analytical Psychology, narrates in story form, with the assistance of Miss Alexander, the psychotherapy (Jungian) he used in treating four modern day "witches" (sic). The story provides easy and interesting reading, but non-Jungians will be struck by the therapist's intrusion of counter transference and by his interpretation of dreams for the construction of those symbolic, primordial images "residing in a common pool of stored experience" that are used in deciphering his patients' "basic needs, desires, and relationships."

Persons of other than Jungian persuasion will not conceive of these women as "witches" as they will find little difficulty in retranslating the dynamics of these cases without recourse to the *Malleus maleficarum.*

The University of Texas **JEROME S. MILLER**

Pictorial History of Philosophy. By DAGOBERT D. RUNES. New York, Philosophical Library, Inc., 1959. Pp. x, 406. $15.00.

Defining philosophy broadly as "Man's orientation in the cosmos" (p. x), "the study of ethical principles" (p. 1)—for "philosophy is ethics, or it is nothing at all" (p. x)—Dr. Runes portrays in pictures and brief pen-sketches the "thinkers" of the past 3000 years who have contributed to the understanding of this problem. He reproduces nearly 1000 illustrations—portraits and other pictorial material germane to the day and generation of the men he treats—that he had gathered during years of search through libraries, museums, and private collections in America and abroad.

His book is organized along religious, racial, and nationalistic lines; within each of these divisions the materials are presented chronologically. He opens his *Pictorial History* with Judaism and closes it with thinkers of the New World—some of whom are still living. The intervening chapters are devoted to Indian, Chinese, Grecian, Roman, Christian, Islamic, French, Spanish, Italian, British, German, and Russian thinkers.

Runes chose the thinkers included in his *History.* He assumed "full responsibility" (p. x)—was the sole arbiter of their selection. "Historians and teachers," he writes, "have attributed great significance to thinkers whom, at least as far as this anthology is concerned, the editor has relegated to oblivion. Perspectives differ, and so do the colors of evaluation" (p. x). Indeed, perspectives and colors of evaluation do differ and the differences exemplify the fact that no man is wise and erudite enough to assume alone the burden of such selections. Different perspectives and the blending of colors of many evaluations are needed in undertakings of this kind. In consequence of Runes's personal selection, thinkers of slight and of no significance, who should have been "relegated to oblivion," are included, and thinkers, whose contributions have greatly influenced philosophical thought, are excluded. Runes would have been wise, therefore, to have enlisted the assistance of an advisory committee composed of "historians and teachers" whose judgments he so cavalierly spurned. He would also have done well to have excluded the living. None of them has withstood the test of time and we stand too close to them to view them in their proper perspective.

The pen-sketches which accompany the illustrations are excellent. They give, within the compass of a few words, the reasons for including the individuals discussed. In every sketch, the tenets of the thinker's philosophy are given with just enough biographical data to place him in his proper perspective. Runes's style is delightful; it is light and airy and interesting. His sketches are accurate and trustworthy, but now and then a startling error appears. For example, Runes writes that "Rush Medical College, now affiliated with the University of Chicago, was so named in honor of Benjamin Rush" (p. 368). True enough, it was named in Rush's honor, but, though once affiliated with the University of Chicago, it was merged many years ago with the University of Illinois Medical School in Chicago.

The typography and illustrations are excellent. Two appendices, one of "Picture credits" and the other of "General acknowledgments," and an index of thinkers treated close the volume.

K. M. D.

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